SCIENCE IN CULTURE
PART B

KNOWLEDGE MAKING

Deakin
University

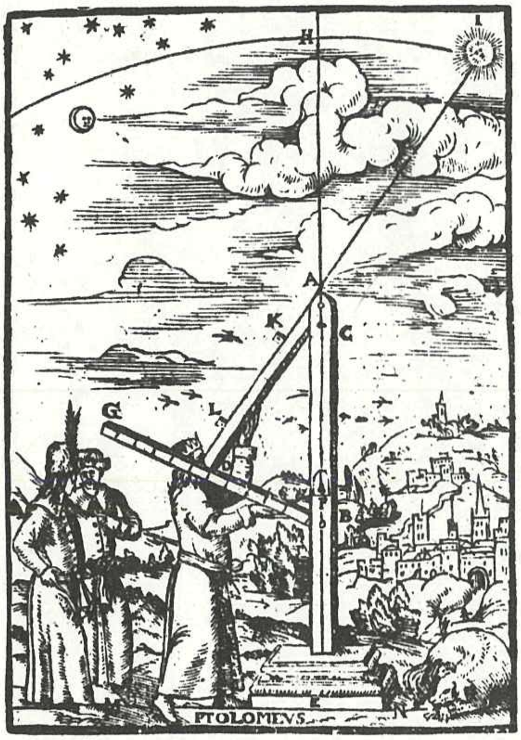
Teaching 15

SCIENCE IN CULTURE

PART B

KNOWLEDGE MAKING

STUDY GUIDE & READINGS



Deakin University

This book forms part of the *Science in Culture* course offered by the School of Humanities in Deakin University's Open Campus Program. It has been prepared for the Science in Culture course team, whose members are:

Barry Butcher
Wade Chambers
Struan Jacobs
Jock McCulloch
David Turnbull (chairperson)

Consultants
David Dickson, Science journalist
Eveleen Richards, University of Wollongong
Ian Reid, Deakin University
John Schuster, University of Wollongong
Steven Shapin, Edinburgh University
Terry Stokes, University of Wollongong

The course includes:
HUS305 Part A: Medicine & Society
(Study Guide and Readings)

HUS306 Part B: Knowledge Making
(Study Guide and Readings)

HUS307 Part C: Knowledge Using
(Study Guide and Readings)

HUS308 Part D: Science & Society
(Study Guide and Readings)

Published by Deakin University
Geelong, Victoria 3217
First published 1985
Revised annually

Edited and designed by
Deakin University Production Unit
Printed by Deakin University Printery

ISBN 0 7300 0622 0

Cover illustration:
A sixteenth-century illustration of Ptolemy (wearing a crown because he was wrongly identified with the Ptolemy royal family), guided by the muse Astronomy and using a quadrant. An armillary sphere is shown, lower left. From Gregor Reisch, *Margarita Philosophica*, 1508.

Title page illustration:
Ptolemy's ruler for measuring the celestial bodies. From William Cunningham, *The Cosmographical Glasse*, 1559.

STUDY GUIDE

ESSAY QUESTIONS

1 needs new text update

KNOWLEDGE MAKING: AN INTRODUCTION

David Turnbull

1a good no need for main

THE PROBLEM OF EXPERIMENT

Steven Shapin

2 little revision

DEMARCATION

David Turnbull

3 cut/revise new big picture stuff

SCIENTIFIC REVOLUTIONS

Struan Jacobs

4 too long but good

GALILEO AND THE CATHOLIC CHURCH

John Schuster

5 cut

SCIENCE AS TEXT

Ian Reid and Terry Stokes

6 big update

WOMEN AND SCIENCE

Eveleen Richards

8 update.

SCIENCE AND THE MEDIA

David Dickson

ESSAY QUESTIONS

1a

THE PROBLEM OF EXPERIMENT

Prepared for the course team by Steven Shapin

The Problem of Experiment

Experiment is fundamental to our present-day conceptions of scientific knowledge and scientific activity. It is difficult for us to imagine what science would be like without a central role for experiment and the observations that arise from it. When we want to conjure up an image of the scientist in the popular mind, we portray him or her doing an experiment in a laboratory, a place purposely designed for experimental work.

Experiment is thought to be the key to the reliability of scientific judgments. The routine and unproblematic replication of experiments underwrites the objectivity of scientists' findings. Experiment is regarded as the ultimate guarantee that scientists do not construe the natural world arbitrarily or subjectively. Experimental observations bind scientists' accounts to the real world.

In some of the most influential philosophical portrayals of science, experimental observations are the firm foundations upon which 'hypotheses' and 'theories' are erected. Experimentally produced facts have been likened to a building, their theoretical interpretation to the scaffolding we may erect and then take down at will. Facts are simply as they are; theories and the like are the constructs of the human mind.

Ultimately, it is said, experimental facts will win the day. They will discriminate between theories that are 'true' or 'false', and between those that are 'probably true' or 'less probably true'. As the biologist T.H. Huxley said in the late nineteenth century, 'The great tragedy of Science is the slaying of a beautiful hypothesis by an ugly fact' (*Collected Essays* Vol. 8, p. 244). A theory may, in various accounts, be regarded as 'true' or 'supported' if it uniquely 'fits' the experimental facts, or if it fits more than rival candidates, or it it fits those deemed most 'important'. Indeed, the notion of a 'crucial experiment' has played a vital role in our understanding of science. The scientist is frequently enjoined to search out experiments whose results discriminate effectively between competing theories.

So solid and irreversible do experimental facts appear to us that we often indulge in the 'logomorphism' described by Gellner. (See 'Knowing in culture', HUS308 *Science and Society*, Deakin University.) We have an inclination to believe that our linguistic representations of the world mirror the reality of the world. In other words, we equate 'facts' with 'statements of fact', how things are in the world with our verbal accounts of how things are. (Indeed, it is cumbersome to identify consistently the different senses in which one uses the word 'fact', and you may wish to note the slippage in usage in this Introduction!)

Let us call these orientations towards experiment and the place of experimental observations 'the canonical model'. Given the strength and the pervasiveness of the canonical model of experiment, it is remarkable how little attention scholars have paid to how experiments are actually done in real scientific practice, how experimental facts come to be what they are, how experimental observations practically connect with theoretical accounts and judgments of truth and falsity, and even to how experimentation historically came to be viewed as the proper way to do natural science and to establish scientific knowledge. Historians, sociologists and philosophers of science have, in the main, tended to take experiment and its role as 'self-evident'. The canonical model has been an important resource for describing, explaining and justifying science, but experiment has scarcely been a topic of inquiry in its own right.

In this section we want to make scientific experiment and its functions into a *problem*. We want to suspend our natural sense of the self-evidence and matter-of-factness of experiment temporarily and to adopt the sort of perspective that a modern anthropologist would bring to studying the beliefs and practices of an alien culture (see M. Douglas, *Implicit Meanings: Essays in Anthropology*, Routledge & Kegan Paul, London, 1975; and B. Latour & S.

Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, Sage, London, 1979). What, after all, are the bases of our canonical beliefs about experimentation, experimental observations and the inferences that are made from experiment?

There are both 'academic' and 'practical' reasons for this exercise in making experiment problematic. Throughout this course we have sought an understanding of how science really works, and how to dissociate certain idealisations of science from the real world of scientific work. Experiment presents itself as one of the last obstacles to such an understanding. But we have also tried to examine the role of scientists and their knowledge in society. And here, too, experimental facts appear recalcitrant. It sometimes seems that we can have a sociological understanding of the 'uses' of experimental facts, but not a sociology of facts themselves.

When scientists are called upon to give their expertise in public forums, the central role of experimental facts becomes particularly evident. In some cases there is a consensus among experts both about what the facts are and about their correct interpretation. In many cases, however, experts disagree about what the experimental facts may mean while agreeing that they are indeed facts. It is, however, relatively rare that the public witnesses the spectacle of scientists disagreeing about what is to count as an experimental fact. The effect of this pattern is to entrench in the public mind the self-evidence and the unproblematic character of experiment and its role in producing reliable knowledge. Yet all citizens who wish to live in a society in which scientific and technological experts are accountable to the people have a responsibility to shoulder if the democratic character of our society is to be protected. They should try to understand how experimental facts are made and how they come to acquire the status they have. In other words, we need to have a practical understanding of how experiment *works*.

The 'canonical model' has not gone unchallenged. Indeed, philosophers like Pierre Duhem (1861-1916) and Karl Popper (b. 1902) long ago registered their opposition to canonical views of the status of scientific facts and of the independence of fact and theory. In recent years a number of sociologists and historians have sought systematically and empirically to probe experiment in a manner that goes beyond what has been done before. (See, for example Bibliography: Collins; Pinch; Shapin & Schaffer.) Their findings broadly suggest that many taken-for-granted notions about the role of experiment in science need to be substantially revised. Some things that are self-evidently known about experiment turn out not to be knowledge at all. Four conclusions emerge from this new research which conflict with the 'canonical model' of experiment.

First, experimental facts do not derive simply and directly from how things are in nature. Instead, they are found to be the products of scientists' labour. We customarily oppose the fact with the artefact, what nature gives us with what we construct. But it appears that scientists do an enormous amount of artful work to make and to establish experimental matters of fact. Statements of fact are artefacts. They do not come to scientists ready-made by nature. What nature gives us is an extremely rich and complex set of sensory stimuli. Scientists labour to select and to focus upon certain of those stimuli and to make *statements* about them. Moreover, *experimental* phenomena, as opposed to those available for inspection in nature, are *artificially produced*. They have to be made into phenomena by scientists; they are the result of practical *work*.

Second, experimental facts are *not* necessarily hard and durable. What one scientist claims to be a fact may be contested by another, equally qualified, scientist. Judgments of what counts as an experimental fact are therefore contingent and they are revisable over time. Nor can scientists unambiguously appeal to notions of 'correct experimental methodology' in order to gauge what is an authentic observation and what is not. The idea that there is a universal and efficacious 'scientific method' that can adjudicate competing claims seems unsupportable. Moreover, there appears to be an intimate relationship between scientists' views of proper experimental method and their commitments to views of what nature is like.

Third, the relationship between experimental observations and theoretical ways of accounting for those observations is not as direct and binding as assumed in the 'canonical model'. Scientists do not experience the phenomena produced by experiment directly but only as mediated by some *theory*. An absolute distinction between fact and theory seems unsustainable. And the notion that we can test theories by their correspondence with experimental facts appears deeply problematic. A number of theories can 'correspond' equally well with the experimental facts.

Finally, there does not seem to have been any inevitability about the historical emergence of the experimental way of doing and justifying natural science. The experimental program developed in specific historical and political circumstances, and it was developed for specific contingent purposes. Nor was its emergence uncontested even in the historical context where it originally found most adherents. Indeed, some thinkers, including eminent philosophers, found the experimental program to be an inadequate and an improper way of securing scientific truth. The debates between those who approved of a central role for systematic experimentation in science and those who rejected it appears to have interesting connections with contrasting theories of how society ought to be organised.

Read Reference 1a.1(a): Shapin, 'Boyle's literary technology', and Reference 1a.1(b): Boyle, 'Air pressure experiment'.

The first extract comes from Boyle's first publication of air-pump experiments (1660); the second from a further series performed with a remodelled pump (published 1669). The first extract describes a well-known phenomenon of cohesion: two very smooth marbles or plates of glass can be made spontaneously to cohere. Boyle reckoned that this cohesion was due to the effects of the air's pressure acting on the free surfaces of the marbles and not on the facing surfaces. It seemed to him that, if this was so, cohered marbles ought to fall apart in an evacuated air-pump. In fact, in the original experiment this did not happen, and the failure was a continuing source of worry to Boyle.

The second extract contains Boyle's claim of eventual success in this experiment: using a new pump and a slightly different experimental set-up, the marbles were said to separate as the air was removed.

The Scientific Revolution of the seventeenth century involved radical changes in how natural philosophers thought the world worked. For example, organismic metaphors were widely replaced by mechanical metaphors. It also involved substantial shifts in how it was thought proper to go about obtaining reliable knowledge of nature. Instead of relying upon the authority of ancient texts from the Greeks and Romans, it was increasingly said that the ultimate source of authority in science should be *experience*. This tendency was particularly strong in England, where Francis Bacon (1561-1626) elaborated an enormously influential *empirical* and *inductive* philosophy of scientific method.

While Bacon's philosophy remained largely at a relatively programmatic level, it was seized upon and developed into a systematic *experimental* practice by Robert Boyle (1627-1691) and his colleagues in the Royal Society of London (founded in 1660). Boyle's work in chemistry and physics is widely regarded as one of the towering achievements of the Scientific Revolution. Many modern scientists and philosophers treat Boyle as a 'founder' of what they take to be 'the scientific method' and especially of the proper method of performing, describing and interpreting experiments. In the new experimental program it was argued that *matters of fact* ought to be generated using artificially constructed devices (like Boyle's air-pump). These experimental facts would constitute the solid foundations of natural knowledge. Philosophers could then discourse about the physical *causes* of these facts, but knowledge of causes was regarded as at best *probable*. Indeed, it was insisted that much of the divisiveness that had characterised previous scientific thought had arisen from expecting too high a degree of certainty from explanatory items.

In the extracts from Boyle we see his attempts to display a model of how consensus could be secured and stabilised in science. We see the range of artful practices used to *construct* experimental facts. We assess just how much labour was required to produce a fact and to win widespread recognition that it was indeed a fact. You may want to refer to Section 5 ('Science as Text') in this connection.

What does Shapin mean by 'virtual witnessing'?

Why was it important that experiments be in the public domain?

What was the importance of Boyle's distinction between 'matters of fact' and their causal explanation?

Do you believe that the experiments related in Boyle's extracts were actually done, and done in the way Boyle described? If you believe some claims and not others, try to say why.

Pretend that you are an enemy of Boyle, and that you want to charge him with fraud. How would you go about discrediting the reliability and authenticity of his experimental reports? (Suppose, for example, that you want to say that the experiments were not actually done as described. If you find it impossible to disbelieve Boyle, reflect upon why you feel this way.)

Does Boyle's prose look like that of a modern scientific paper? In what respects is it similar and dissimilar to present-day scientific writing?

Read Reference 1a.2: 'Gravitational radiation and experimenters' regress'.

We regard experiment as a powerful force in science. Its power is frequently said to reside in its role as an independent test of competing theories about how the world is. If we are to put nature to the test, we must ensure that the test is indeed independent. And if the test depended upon some theory or some view of how the world is, it would not be independent, nor, we might say, would it be a powerful test.

Where Shapin dealt with the historical origins of systematic experimentation, Collins treats an episode from the very recent past and which, indeed, is not completely closed at present: the claim to have experimentally detected high fluxes of gravitational radiation. The precise scientific setting of these experiments need not detain us, but it involves the empirical implications of Einstein's Theory of General Relativity.

In the late 1960s and early 1970s Joseph Weber claimed to have detected fluxes of gravitational radiation which most physicists reckoned could not exist: his results conflicted with deeply entrenched and highly valued ways of understanding the physical world. Weber's opponents thought his claims were wrong, and they set out to demonstrate this experimentally.

Nothing could seem more straightforward in science. If you are in doubt about whether an experimental claim is true, you *replicate* the experiment and see what the result is. However, Collins points to an important feature of the controversies over Weber's work: the fact that experiment was not, and could not be, an *independent test* of the claim in question. Collins calls this the *experimenters' regress*: in order to test the view that there were high fluxes of gravitational radiation, you have to decide what counts as a well-designed and well-working experimental detector; but conversely, in order to decide what is a competent detector, you have to decide whether such things as high fluxes of gravitational radiation exist.

The 'circle' is not, Collins shows, broken by *calibration* itself, but by the social control of how experiments may permissibly be *interpreted*. The experimenters' regress is a constitutive feature of what Collins calls 'extraordinary science': episodes in which there is no consensus about fundamental aspects of what the natural world is like.

What does Collins mean by the 'experimenters' regress'? Describe what this regress was in the case under study.

Why wasn't the problem of whether high fluxes of gravitational radiation existed simply decided by scientists seeing who had followed proper experimental procedure and who had not?

Describe attempts to end the dispute through 'calibration' experiments. Did they work? How does Collins account for the eventual outcome of the controversy, that is, the almost total scientific consensus that high fluxes of gravitational radiation do not exist?

Read Reference 1a.3: Bloor, 'Truth and convention'.

Collins described a situation in which experiment could not constitute an independent test of how things stood in reality: one had to decide how the world was before one could gauge whether an experiment was adequately performed. Experiments were not independent of scientists' theoretical commitments. Collins's work is therefore a challenge to the view that 'facts' and 'theories' are independent entities. These selections from David Bloor's philosophical treatment of the correspondence view of scientific truth address the relationship between theory and experimental facts at a general level.

The overthrow of the 'phlogiston theory' by the oxygen theory of combustion is regarded as one of the great achievements of the 'Chemical Revolution' of the late eighteenth century. The eminent English chemist Joseph Priestley (1733-1804) upheld the phlogiston theory, while the Frenchman Antoine Laurent Lavoisier (1743-1794) was an advocate of the oxygen theory. In the contest between the different theories certain experimental evidence was regarded as crucial.

One experiment was treated by Priestley as especially telling. Using modern terminology, this experiment involved the heating of lead oxide in a dish floating on water within a vessel containing hydrogen gas. (See p.33, figure 2.) Continuing to use present-day language, what we 'see' when this experiment is done is this: the oxygen combines with the hydrogen gas, forming water; the water level in the vessel rises, indicating the consumption of free hydrogen gas. Broadly speaking, this is also what Lavoisier 'saw'. The experimental evidence 'fitted', or 'corresponded with' Lavoisier's oxygen theory of combustion.

But what did Priestley, and other proponents of the phlogiston theory 'see'? And, if this experimental result 'fitted' the oxygen theory, did it not 'fit', as we might expect, the alternative view? In fact, Priestley and Lavoisier saw the same experiment in entirely different light. As Bloor describes the situation, Priestley 'saw' a substance he called 'minium' turning into lead through its absorption of 'phlogiston', with the rise in water-level confirming that absorption.

Of course, if we think about it neither we nor the eighteenth-century scientists concerned 'saw' phlogiston or minium or oxygen or lead oxide or hydrogen or any of these entities combining with the other. No one has direct access to the reality which is the reference of the theory, and, as Bloor argues, 'the indicator of truth that we actually use is that the theory works', that the theory be internally consistent and coherent. Bloor examines how Priestley was able to elaborate his phlogiston theory in the face of an apparent anomaly so to save its overall coherence.

We want to say that Priestley was wrong and that Lavoisier was right. But look again at the experimental observations and at the theories that were brought to bear upon them. Was there anything about these experimental phenomena that did not 'fit' Priestley's phlogiston theory?

To be sure, the 'canonical model' does not necessarily deny that certain experimental facts can be equally well explained by more than one theory. That is why scientists look for certain, especially revealing experiments that can discriminate between rival theoretical accounts. (In the seventeenth century Bacon, Hooke and Newton called such experiments 'crucial', and the usage has continued to the present day.) In the case of phlogiston theory, it is usually claimed that a famous experiment by Lavoisier involving the red calx of mercury (what we call HgO) performed this role. (See J.B. Conant (ed.), *Harvard Case Histories in Experimental Science* Vol. 1, Harvard University Press, Cambridge, Mass., 1948 (and many subsequent editions), pp.68 ff.)

Nevertheless, certain problems remain for the 'canonical model'. First, followers of the phlogiston theory modified and improved their theory to bring it into line with these observations. Second, Lavoisier's theory itself carried with it a whole range of difficulties. Lavoisier's oxygen theory was not just a theory of combustion; it was also a theory of acidity. He believed that anything containing oxygen would be an acid. This claim encountered trouble because no one could extract oxygen from 'marine acid gas' (what we now call hydrochloric acid). Furthermore, Lavoisier had to explain why water was not an acid given that it was known to contain oxygen.

In general, the problem is this: we must realise that all theories typically involve areas where they 'work' and areas where they 'don't work'. Saying that they do, or do not, fit the facts therefore involves unavoidable selectivity and judgment. It is scientists who do the selection and who make the judgments, not reality.

In the book from which this extract derives Bloor argues for a thorough-going sociological account of scientific knowledge. It is sometimes charged against the sociology of scientific knowledge that it disregards the role of scientists' sensory experience of natural reality. Is this the case in Bloor's work? What role does he give to experimental observations?

Were there any experimental facts that the phlogiston theory could not explain?

In light of your answer to the previous question, speculate about why the scientific community came to prefer the oxygen theory to the phlogiston theory.

Read Reference 1a.4: Shapin & Schaffer, 'Hobbes versus Boyle'.

We started this section with a discussion of the 'canonical model' of experiment: a set of assumptions that had the effect of making experiment and its role in science self-evident. We have now reached a point at which many features of experiment seem problematic. How do the results of experiment become matters of fact? How can we say that experiments 'test' or 'decide between' theories? What is the relationship between the outcomes of experiments and our theoretical commitments?

In confronting these questions, it has been suggested that experiment does nothing by itself: scientists *work* to construe, to 'make out' experiments as having the role and the meaning attached to them. Put another way, experimental practice in science is grounded in a set of *conventions*. These conventions are *social* in character: they are transmitted by the institutions that train scientists into proper ways of behaving, and they work on individual scientists with the force of the community to which they belong. These conventions regulate the ways in which it is deemed permissible to perform and to interpret experiments. Like all social conventions, they have a dual character. On the one hand, those who live by them may feel them to be part of the hard and fast natural order of things. These conventions may be, indeed they usually are, referred to how things are in nature, rather than to the domain of human artefact. Yet social conventions vary from one sort of society to another and they change over time. What seems a self-evidently right and proper course of conduct to people in one setting may seem pointless or wrong to another group of people.

We want to conclude by looking at the context in which many features of proper scientific practice (as recognised by modern scientists and philosophers) were developed and institutionalised. We will go back to the seventeenth-century English context treated in Reference 1a.1. We will look at the historical origins of a program in natural science which stressed the importance of systematic experimentation and certain ways of interpreting experimental results.

In Reference 1a.1 we examined the program advocated by Robert Boyle and supported by his colleagues in the early Royal Society of London. It was hinted in that reference that Boyle's program was not unopposed. Indeed, it met with vehement criticism by several English and

Continental natural philosophers. In England the most articulate and influential critic was Thomas Hobbes (1588-1679), who is best known today as the author of *Leviathan*, an essay in political philosophy, but who was also the author of a number of tracts in natural philosophy and mathematics.

We want to understand on what grounds Hobbes opposed a program which seems to many of us so obviously proper and fruitful. Was he, as several historians have indeed suggested, simply a stupid and stubborn old man? Was it that he 'misunderstood' what Boyle was recommending? In trying to appreciate the *sense* of Hobbes's criticisms we will also develop a sense of the radical incompatibility of the opposed programs, and a sense that preferences for one or the other might be rooted in commitments to different 'forms of life', that is, to far-reaching conventional structures that structure social relations, including the social relations used in making and justifying knowledge. Philosophers following Boyle and those following Hobbes would inhabit different types of *society*.

Broadly speaking, Hobbes reckoned that Boyle's experimental program reached for, and obtained, an inadequate grade of certainty in natural philosophy. People were left too free to differ about things that ought to be the foundations of *certain* knowledge. The mark of genuine philosophy for Hobbes was agreement, especially agreement about the causes of things. How could Boyle's experimental program be called philosophy if it assured participants that causal knowledge was at best probable and that they could legitimately differ about physical causes? How was it legitimate to erect boundaries between factual and causal knowledge? And how could experimental observations secure the agreement Boyle claimed for them if experimental outcomes could, as a matter of principle, always be disputed?

While Boyle advertised the *freedom* of experimental practice as a basis of its moral as well as its scientific superiority, Hobbes looked for practices that *compelled* people to assent. England had recently passed through decades of civil war. Many thinkers attributed that social disruption to incorrect *knowledge*. If you wanted to solve the problem of social order, you would have to solve the problem of making and justifying knowledge. Boyle maintained that only a community of free-acting individuals, consulting experience, would be stable. Hobbes claimed that only compulsion, exercised either by the sovereign power or by the tools of reason, would secure order. Attitudes to the experimental form of life were thus informed by commitments to different visions of how people should form themselves into a society.

Boyle maintained that his air-pump yielded indisputable matters of fact. How did Hobbes attempt to show that its 'facts' were not facts at all?

Hobbes charged Boyle and the experimentalists of the early Royal Society with debasing the philosopher's role. What was the force of that charge and how did it bear upon conceptions of experimental work?

How did Hobbes's way of writing natural philosophy manifest his view of the proper role of logic and compulsion? Contrast Hobbes's 'literary technology' with Boyle's (as described in Reference 1a.1).

ANNOTATED BIBLIOGRAPHY

Barnes B. 'Problems of intelligibility and paradigm instances'. In J.R. Brown (ed.), *Scientific Rationality: The Sociological Turn*. Reidel, Dordrecht, 1984, pp.113-25.
Deals with the respective roles of the 'social' and of 'reality'. Example from modern marine biology.

Collins H.M. *Changing Order: Replication and Induction in Scientific Practice*. Sage Publications, London, 1985.

Collins H.M. & Pinch T.J. *Frames of Meaning: The Social Construction of Extraordinary Science*. Routledge & Kegan Paul, London, 1982.
Instance of 'extraordinary science', to be read in conjunction with Collins's study of gravitational radiation disputes. Judgments of experimental adequacy.

Dear P. 'Jesuit mathematical science and the reconstitution of experience in the early seventeenth century'. *Studies in History and Philosophy of Science*, vol. 18 ,1987, pp. 133-75.
Historical emergence of senses of 'experience' and 'experiment'.

Farley J. & Geison G.L. 'Science, politics and spontaneous generation in nineteenth-century France: The Pasteur-Pouchet debate'. *Bulletin of the History of Medicine*, vol. 48, 1974, pp. 161-98.
The influence of 'external factors' upon experimental judgments.

Franklin A. *The Neglect of Experiment*. Cambridge University Press, Cambridge, 1986.
Modern experimental physics. Role of experiment in deciding between competing theories. How scientists distinguish 'signal' from 'noise' in experimental work.

Galison P. *How Experiments End*. University of Chicago Press, Chicago, 1987.
High-energy physics. Strategies of 'demonstration'. How experimental phenomena are made manifest.

Glas E. 'Bio-science between experiment and ideology, 1835-50'. *Studies in History and Philosophy of Science*, vol.14, 1983, pp. 39-57.
How the isolation and analysis of protein was interpreted in the nineteenth century. Links between processes in wider society and judgments of experimental method.

Gooding D. '"In Nature's school": Faraday as an experimentalist'. In D. Gooding & F.A.L. James (eds), *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday, 1791-1867*. Macmillan, Basingstoke, 1985, pp. 105-35.
Distinction between experiment as research and experiment as demonstration.

Gooding D. 'How do scientists reach agreement about novel observations?'. *Studies in History and Philosophy of Science*, vol. 17, 1986, pp. 105-30.
Scientists' representational practice in relation to experimental findings. Examples from early nineteenth-century electromagnetism.

Gooding D., Pinch T.J. & Schaffer S. (eds). *The Uses of Experiment: Studies in Natural Science*. Cambridge University Press, Cambridge, 1988.
Empirical-theoretical studies of experimental practice. See especially paper by Schaffer on Newton's optical experiments.

Holton G. 'Subelectrons, presuppositions, and the Millikan-Ehrenhaft dispute'. In G. Holton, *The Scientific Imagination: Case Studies*. Cambridge University Press, Cambridge, 1978.
Deconstruction of classic physics experiment.

Kohler R.E. 'The reception of Eduard Buchner's discovery of cell-free fermentation'. *Journal of the History of Biology*, vol. 5, 1972, pp. 327-53.
Judgments of experimental competence informed by technical and professional commitments. Example from late nineteenth-century biochemistry.

Koyre A. *Measurement and Metaphysics: Essays in the Scientific Revolution.* Harvard University Press, Cambridge, Mass., 1968.
Rationalist historian defends rationalist scientific practice. See, especially, his treatment of Pascal. Useful materials to address the question: why bother to do experiments?

Kuhn T.S. *The Structure of Scientific Revolution.* 2nd edn, University of Chicago Press, Chicago, 1970.

Kuhn T.S. 'The function of measurement in modern physical science'. In T.S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change.* University of Chicago Press, Chicago, 1977, pp. 178-224.
In what sense can it be said that experimental results provide a test of theory?

Kuhn T.S. 'Mathematical versus experimental traditions in the development of physical science'. In T.S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change.* University of Chicago Press, Chicago, 1977, pp. 31-65.
How important was experimentation in the Scientific Revolution?

Lynch M. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory.* Routledge & Kegan Paul, London, 1985.
'Ethnomethodological' study of laboratory practice in neurophysiology. Special reference to the creation of scientific representations.

Pickering A. 'The role of interests in high-energy physics: The choice between charm and colour'. In K. D. Knorr, R. Krohn & R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences*, Vol.IV, Reidel, Dordrecht, 1980, pp. 107-38.
Role of socially acquired skills and competences in scientific controversy.

Pickering A. 'The hunting of the quark'. *Isis*, vol. 72, 1982, pp. 216-36.
Theory-experiment relations in high-energy physics. Judgments of experimental competence.

Pickering A. 'Against putting the phenomena first: The discovery of the weak neutral current'. *Studies in History and Philosophy of Science*, vol. 15, 1984, pp. 85-117.
The practical construction of experimental phenomena in high-energy physics.

Pickering A. *Constructing Quarks: A Sociological History of Particle Physics.* University of Edinburgh Press, Edinburgh, 1984.
The day-to-day practice of scientists.

Pinch T.J. 'Theory testing in science: The case of solar neutrinos: Do crucial experiments test theories or theorists?'. *Philosophy of the Social Sciences*, vol. 15, 1985, pp. 167-87.
Decisions about whether experimental observations fit theories.

Pinch T.J. 'The externality and evidential significance of observational reports in physics'. *Social Studies in Science*, vol. 15, 1985, pp. 3-36.
Interpretative flexibility of experimental results. Example from solar-neutrino detection experiments.

Pinch T.J. *Confronting Nature: The Sociology of Solar-Neutrino Detection.* Reidel, Dordrecht, 1986.
Noteworthy for its attention to funding of experimental projects and the relationship between 'micro-sociological' and 'macro-sociological' factors.'

Ravetz J.R. *Scientific Knowledge and Its Social Problems.* Clarendon Press, Oxford, 1971, Part II.
Salutary stress on the craft nature of scientific activity.

Shapin S. 'History of science and its sociological reconstructions'. *History of Science*, vol. 20, 1982, pp. 157-211. Also in R.S. Cohen & T. Schnelle (eds). *Cognition and Fact: Materials on Ludwik Fleck*. Reidel, Dordrecht, 1986, pp. 325-86.
Survey of empirical literature in sociology of scientific knowledge. Several sections deal with contingency of experimental judgments.

Shapin S. & Schaffer S. *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life*. Princeton University Press, Princeton, NJ, 1985.

2

DEMARCATION

Prepared for the course team by David Turnbull

Demarcation

To seek it with thimbles, to seek it with care;
To pursue it with forks and hope;
To threaten its life with a railway-share;
To charm it with smiles and soap!

For the Snark's a peculiar creature, that won't
Be caught in a commonplace way.
Do all that you know, and try all that you don't:
Not a chance must be wasted today!

(Lewis Carroll, *The Hunting of the Snark*, Macmillan, London, 1876.)

Introduction

> We live in a society which sets great store by science. Scientific "experts" play a privileged role in many of our institutions, ranging from the courts of law to the corridors of power. At a still more fundamental level, most of us strive to shape our beliefs about the natural world in the 'scientific' image. If scientists say that continents move or that the universe is billions of years old, we generally believe them, however counter-intuitive and implausible their claims might appear to be. Equally, we tend to acquiesce in what scientists tell us not to believe. If, for instance, scientists say that Velikovsky was a crank, that the Biblical creation story is hokum, that UFO's do not exist, or that acupuncture is ineffective, we generally make the scientist's contempt for things our own, reserving for them those social sanctions and disapprobations which are the just deserts of quacks, charlatans and liars. In sum, much of our intellectual life, and increasingly large portions of our social life, rest on the assumption that we (or, if not ourselves, then someone whom we trust in these matters) can tell the difference between science and its counterfeit.
>
> For a variety of historical and logical reasons, some going back more than two millennia, that 'someone' to whom we turn to find out what the difference is usually happens to be the philosopher. Indeed, it would not be going too far to say that, for a very long time, philosophers have been regarded as the gatekeepers to the scientific estate. They are the ones who are supposed to be able to tell the difference between real science and pseudo-science. In the familiar academic scheme of things, it is specifically the theorists of knowledge and the philosophers of science who are charged with arbitrating and legitimating the claims of any sect to 'scientific' status. It is small wonder, under the circumstances, that the question of the nature of science has loomed very large in Western philosophy. From Plato to Popper, philosophers have sought to identify those epistemic features which demarcate or mark off science from other sorts of belief and activity. (L. Laudan, 'The demise of the demarcation problem', in R.S. Cohen & L. Laudan (eds), *Physics, Philosophy and Psychoanalysis*, Reidel, Dordrecht, 1983, p.111.)

Thus far in this course we have considered the problematic character of scientific knowledge and the way it is socially constructed, with most emphasis on the knowledge and on its scientificity. The question of what science is and how it differs from other forms of knowledge has been a problem almost as long as there has been philosophy. However, as we shall see it is not merely a philosophical issue but one with social and political ramifications.

Before you start work on this material, what are your own views on the issue?

Many of you will recall reading M. Charlesworth, *Science, Non-Science and Pseudo-Science,* (Deakin University, Vic., 1982). Write down a brief list of the characteristics of knowledge claims that in your opinion would warrant their being called scientific.

Do you think it is a clear-cut issue?

Is it essentially philosophical?

If not, in what way do you think it significant?

It would not be unreasonable for someone, unfamiliar with the complexities of the problem, and imbued with the conventional image of science to hold the following position — which might be called the 'standard view of science'. Science is a coherent, systematic body of knowledge, which is derived from the observation of facts using the principles of the scientific method consisting largely of the logical procedures of induction and deduction. Uniformities and regularities which are discerned in observed facts are called laws, and the explanations of those laws are called theories. Theories may require the postulation of entities which are not themselves directly observable — like force or electrons — but such entities are none the less derivable from the observed phenomena. The body of knowledge so obtained is worthy of the title scientific because it is **objective** — that is, independent of the opinions or wishes of individuals — and is based on facts which are simply characteristics of external reality. Moreover, compliance with the methods of science enables others to repeat experiments and observations and to verify their results which ultimately leads to a very high degree of consensus within science and a very high degree of coherence amongst various disciplines.

Read Reference 2.1: Popper, 'Science: conjectures and refutations'.

Note his criticisms of induction and verification and his proposed demarcation criterion.

Can you find anything to criticise in Popper's account of falsification?

Does it achieve its task in the case of astrology for example?

Does it seem to exclude anything you might expect to count as science?

In a given example of an apparent conflict between an observation and an hypothesis is it in practice possible to determine what has been falsified?

Keep your answers to these questions in mind as you read subsequent references many of which are critical of Popper's ideas.

Read Reference 2.2: Kuhn, 'Logic of discovery or psychology of research?'

Read Reference 2.3: Lakatos, 'Science and pseudo-science'.

Do you think that the distinction between progressive and degenerative research programs is an advance on Popper and Kuhn's treatment?

Is the distinction sufficiently sharp to act as a demarcation criteria?

Lakatos goes beyond Laudan's claim that characterising science has been of central importance in Western philosophy to the much stronger claim that far from being a merely philosophical problem 'it is of vital social and political relevance' (p.115). As you read further you should not only absorb the arguments for and against particular descriptions of science but also make notes on the social and political consequences of those descriptions. Paul Feyerabend is notoriously anarchistic in his approach and obviously views science in a highly political way.

Read Reference 2.4: Feyerabend, 'How to defend society against science'.

How well do Feyerabend's criticisms of Popper, Kuhn and Lakatos work?

How would you characterise his understanding of science?

What connections does he perceive between science and politics, what remedies does he advocate?

A much more moderate view is advocated by J.M. Ziman and probably comes close to the presently received view.

Read Reference 2.5: Ziman, 'What is science?'

Bringing to bear all that you have read so far, what, if anything, do you find to criticise in Ziman's position?

In ch. 2 of *Public Knowledge* (Cambridge University Press, Cambridge, 1968) Ziman argues that 'science is designed in striving for, and instituting, a consensus'.

What does he mean by consensus?

What are his reasons for advocating this view?

Note that this science is an inherently social activity, that Ziman, like many other philosophers, is ultimately compelled to consider the social character of science.

Is the consensus criteria adequate to distinguish science from pseudo-science as well as from non-science?

Is it an accurate depiction of science?

Is it desirable? How does it relate to the ideas of Lakatos and Kuhn?

In the light of the positions of Popper, Kuhn, Feyerabend and Ziman sketched above, would you characterise as scientific or non-scientific such knowledge claims as those of, Ptolemaic astronomy, history, parapsychology, Azande magic, economics, quantum mechanics, creationism, mathematics, Marxism, sociobiology, Newtonian mechanics or Micronesian navigation? What criterion of demarcation did you use? Why?

Now that you have examined some of the philosophical background of the demarcation problem let us turn to consider some of the ways in which we are driven back from an abstract analytic discussion to the social dimension. Popper, like Ziman, ultimately grounds his view of science in a number of agreements amongst participants.

> Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we decide to accept. If we do not come to any decision, and do not accept some basic statement or other, then the test will have led nowhere.
>
> ...
>
> Basic statements are accepted as the result of a decision or agreement; and to that extent they are conventions.
>
> ...
>
> The empirical basis of objective science has thus nothing absolute about it. Science does not rest on rock bottom. The bold structure of its theories rises as it were above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or given base and when we cease our attempts to drive our piles into a deeper level it is not because we have reached firm ground, we simply stop when we are satisfied that they are firm enough to carry the structure at least for the time being ... (K.R. Popper, *Logic of Scientific Discovery*, Hutchinson, London, 1959, pp.106-11.)

Thus, Popper holds that science is both fallible (subject to revision and correction over time) and also at root conventional (dependent on agreement of what is to count as a basic statement and what is to count as adequate support). The question is who constitutes the 'we' that makes those decisions? This question also emerges from the accounts of Kuhn, Lakatos, Feyerabend and Ziman, who all concur in the view that what counts as 'scientific', rational, progressive, puzzle-solving or whatever is a matter of conventional agreement.

Do you find the arguments for this conventionalist position persuasive? Why? If not, why not?

Popper and Lakatos, like Laudan, appear to argue that philosophers of science should be the ones who are responsible for laying down the demarcation criteria and evaluating scientific practice. Kuhn and Ziman appear to hold that it should be the practitioners. Feyerabend appears to hold a third position, that it is a matter of personal taste since there are no agreed criteria.

Do you find any of these alternatives acceptable? Why?

According to D.L. Phillips (*Wittgenstein and Scientific Knowledge: A Sociological Perspective*, Macmillan, London, 1977, p.167) the accounts of Kuhn, Popper and Kakatos makes science 'inescapably elitist'. Only Feyerabend is willing to 'leave decisions about scientific standards to the state or to laymen' (D.L. Phillips, *Wittgenstein*, p.167).

Do you agree with this assessment?

Do you think that science should be elitist and if you do not, what might be a reasonable alternative?

The social embeddedness of the issue is just as pervasive when attempting to demarcate good from bad science, which is according to Laudan (R. Laudan, 'The demarcation between science and pseudo-science', in R. Laudan (ed.), *Working Papers*, vol.2, no.1, Centre for the study of science in society, Virginia Tech., Blacksburg, Va., 1983, p.29) a much more significant question than distinguishing science from non-science. This is reflected in Westrum's discussion of the socially constructed character of 'competence' and 'reality'.

> By competence I mean proper use of that system of inquiry, experiment and reasoning which best reflects the state of the art of science. In school most of us were taught that there is a single 'scientific method' used in resolving scientific questions. Working scientists, however, understand and ethnographic studies of research have confirmed that exactly what constitutes 'scientific method' is a flexible matter depending on the problem at hand and the resources which can be brought to bear on it There appears to be a whole spectrum of procedures which are used by scientists to develop and test ideas about the way things are. Some of these operations and some of the practitioners are perceived as being 'better' than others. The highest level of practice is called the 'state of the art'. It is this socially constructed standard of competence in research which defines what is really 'scientific' and what is not.
>
> By reality I mean that state of affairs which a given social group agrees is the case. What is 'real', then, is something agreed upon by a given group and therefore may be different for different groups. This reality is constructed through mutual interaction, and more advanced societies tend to have highly specialised roles (such as that of scientist) whose maintenance of this reality is just as important as its construction, and a number of social institutions are likely to take part in it. Every such reality brings with it a set of events which do not fit within the framework, and therefore are "unreal". How experiences with such 'unreal' events are handled is obviously critical for the maintenance of the reality for that society. (R. Westrum, 'Crypto-science and social intelligence about anomalous events', in R. Laudan (ed.), *Working Papers*, vol.2, no.1, Centre for the Study of Science in Society, Virginia Tech. Blacksburg, Va., 1983, pp.151-2.)

Do you think that there are ways to show that a belief is well founded or heuristically fertile which are independent of socially constructed definitions of competence and reality?

To pursue this sociological and political perspective further:

Read Reference 2.6: Gieryn, 'Making the demarcation of science a sociological problem'.

How does Gieryn characterise the problem and what does he contend is at stake?

Gieryn's analysis of science's struggle for cognitive authority complements Randall Albury's analysis of the autonomy of science in terms of internal and external control of funding in the book, *Politics of Objectivity*, (Deakin University, Vic., 1982). Many of you who did the course *Knowledge and Power* (HUS 101/2) will be familiar with this work, but it may pay to reread it, especially ch. 3, 'Scientific objectivity and social objectives'.

> The fundamental importance, socially, philosophically and politically, of defining what counts as science in the actual practice of science has been captured by Pierre Bourdieu. In attempting to understand how the individual self-interest of scientists is effectively channelled into producing the progress of reason we found that the essence of scientific practice is the struggle for authority and 'what is at stake is in fact the power to impose the definition of science (i.e., the delimitation of the field of problems, methods and theories that may be regarded as scientific) best suited to his specific interests, i.e., the definition most likely to enable him to occupy the dominant position in full legitimacy, by attributing the highest position in the hierarchy of scientific values to the scientific capacities which he personally or institutionally possesses.' (P. Bourdieu, 'The specificity of the scientific field and the social conditions of the progress of reason', *Social Science Information*, vol.14, 1975, p.23.)

How does this discussion of the demarcation problem intersect with the approach taken in section 1, *Knowledge Making: An Introduction*?

How does it fit with your own position?

ANNOTATED BIBLIOGRAPHY

This bibliography is subdivided by topic, with entries listed alphabetically within each topic.

Recommended Reading

Chalmers, A. *What is This Thing Called Science?* 2nd edn, Queensland University Press, St Lucia, 1982.
A good basic introduction to contemporary philosophy of science, it is recommended that all students read this book.

Charlesworth, M. *Science, Non-Science and Pseudo-Science.* Deakin University, Vic., 1982.
A good introduction to Bacon, Popper, Lakatos, Kuhn and Feyerabend.

Feyerabend, P. *Against Method: Outline of an Anarchistic Theory of Knowledge.* New Left Books, London, 1975.

Feyerabend, P. *Science in a Free Society.* New Left Books, London, 1978.
Both these books are recommended for students who wish to explore a radical viewpoint.

Grove, J.W. 'Rationality at risk: Science against pseudoscience'. *Minerva*, vol.23, 1985, pp.216-40.

Hanen, M.P., Osler, M.J., & Weyant, R.G. (eds). *Science, Pseudoscience and Society.* Wilfrid Laurier University Press, Waterloo, Ontario, 1980.
A very useful collection of essays from across the spectrum on the question of demarcation.

Kuhn, T.S. *The Structure of Scientific Revolutions.* 2nd edn, University of Chicago Press, Chicago, Ill., 1970.
A seminal work which all students should read.

Lakatos I., & Musgrave A. (eds). *Criticism and the Growth of Knowledge.* University Press, London 1970.
A classic collection of essays, including Popper, Kuhn, Lakatos, Feyerabend and Toulmin. A useful companion to Chalmers (*What is This Thing Called Science?*).

Laudan, L. 'The demise of the demarcation problem'. In R.S. Cohen & L. Laudan (eds). *Physics Philosophy and Psychoanalysis.* Redidel, Dortrecht, 1983.

Laudan, R. (ed.). 'The demarcation between science and pseudo-science'. *Working Papers*, vol.2, no.1, Centre for the Study of Science in Society, Virginia Tech. Blacksburg, Va., 1983.
Particularly good papers by Laudan, Gieryn and Westrum; goes well with the Hanen, Osler and Weyant collection (*Science, Pseudoscience and Society*).

Phillips, D.L. *Wittgenstein and Scientific Knowledge: A Sociological Perspective.* Macmillan, London, 1977.
Ch.7 on the demarcation problem in science is of particular relevance.

Popper, K.R. *The Logic of Scientific Discovery.* Hutchinson, London 1979.

Popper, K.R. *Conjectures and Refutations: The Growth of Scientific Knowledge.* 4th edn, Routledge & Kegan Paul, London, 1972.
Though Popper has been much criticised both these works have had an enormous impact on the philosophy of science in general and demarcation in particular.

Suppe, F. (ed.). *The Structure of Scientific Theories.* 2nd edn, University of Illinois Press, Chicago, Ill., 1977.
A good overview of the technical literature for those who are interested in pursuing philosophical issues, but not recommended as an introductory text.

Ziman, J. *Public Knowledge: The Social Dimension of Science.* Cambridge University Press, London, 1968.
A readable account by a practising theoretical physicist.

Further Background Reading

Albury, R. *The Politics of Objectivity,* Deakin University, Vic., 1983.

Bourdieu, P. 'The specificity of the scientific field and the social conditions of the progress of reason'. *Social Science Information,* vol.14, 1975, pp.19-47.

Douglas, M. *Purity and Danger: An Analysis of Concepts of Pollution and Taboo.* Routledge & Kegan Paul, London, 1966.

Douglas, M. *Natural Symbols: Explorations in Cosmology.* Barrie & Jenkins, London, 1973.
Both these works provide an anthropological framework for exploring the general nature of boundary formation.

Mendelsohn, E., Weingart, P., & Whitely, R. (eds). *The Social Production of Scientific Knowledge.* Reidel, Dordrecht, 1977.
Provides some good social and historical background especially the essays by Mendelsohn and Van Den Daele.

On Psychoanalysis, Parapsychology and Astrology

Cioffi, F. 'Fraud and the idea of a pseudo-science'. In F. Cioffi, & R. Berger, (eds). *Explanation in the Behavioural Sciences.* Cambridge University Press, Cambridge, 1970.
A useful addition to Grunbaum ('How scientific is psychoanalysis?').

Collins H.M., & Pinch, T. *Frames of Meaning: The Social Construction of Extraordinary Science.* Routledge & Kegan Paul, London, 1982.

Grunbaum, A. 'How scientific is psychoanalysis'. In R. Stern, L.S. Morowitz, & J. Lyons (eds). *Science and Psychotherapy.* Haven, New York, 1977.

Mauskoff, S.M., & M.R. McVaugh. *The Elusive Science: Origins of Experimental Psychical Research.* Johns Hopkins University Press, Baltimore, Md., 1980.
For the student who wants to go further with parapsychology.

Mendelsohn E., & Elkana Y. (eds). *Sciences and Cultures.* Reidel, Dordrecht, 1981.

Pinch, T. 'Normal explanations of the paranormal: The demarcation problem and fraud in parapsychology'. *Social Studies of Science,* vol.9, no.1, 1979, pp.329-48.
Pinch and Collins (*Frames of Meaning*) provide the basic background for considering parapsychology.

Wallis, R. (ed.). *On the Margins of Science: The Social Construction of Rejected Knowledge.* Sociological Review Monographs, University of Keele, 1971.
This and the preceding book both contain relevant essays, especially those on astrology by Wright. See also the following title.

Wright, P. 'Astrology and science in seventeenth-century England'. *Social Studies of Science*, vol.5, 1975, pp.399-422.

On Velikovsky

Albury, R. *The Politics of Objectivity.* Deakin University, Vic., 1983.

de Grazia, A. (ed.). *The Velikovsky Affair*. Abacus, London, 1978.

Goldsmith, D. (ed.). *Scientists Confront Velikovsky.* Cornell University Press, Ithaca, NY, 1977.

Editors of Pensee, *Velikovsky Reconsidered*. Doubleday, Garden City, NY, 1976.

On Creationism

Kitcher, P. *Abusing Science: The Case Against Creationism.* MIT Press, Boston, Mass., 1982.

Nelkin, D. *Scientific Textbook Controversies and the Politics of Equal Trust*. MIT Press, Boston, Mass., 1977.

In the midst of the word he was trying to say,
In the midst of his laughter and glee,
He had softly and suddenly vanished away -
For the Snark was a Boojum, you see.

Source of poem and picture: L. Carroll, *The Hunting of the Snark*, Macmillan, London, 1935, pp.40, 41 & 43. Reproduced by permission.

3

SCIENTIFIC REVOLUTIONS

Prepared for the course team by Struan Jacobs

Scientific Revolutions

We have now to consider more especially a long and barren period, which intervened between the scientific activity of ancient Greece and that of modern Europe; and which we may, therefore, call the Stationary Period of Science ...

William Whewell

The mechanical and physical science of which modern times are justly proud unfolds, through an interrupted series of barely imperceptible improvements, from the doctrines taught in the medieval schools.

Pierre Duhem

The great merit of Professor Kuhn's insistence on the 'revolutionary' character of some changes in scientific theory is that it has compelled many people to face for the first time the full profundity of the conceptual transformations which have, at times, marked the historical development of scientific ideas.

Stephen Toulmin

The new paradigm brings a totally new rationality. There are no super-paradigmatic standards. The change is a bandwagon effect. Thus in Kuhn's view scientific revolution is irrational, a matter for mob psychology.

Imre Lakatos

I have not previously and do not now understand quite what my critics mean wher they employ terms like 'irrational' and 'irrationality' to characterize my views .... My difficulties in understanding are, however, even clearer and more acute when these terms are us not to criticize my position but in its defence. Obviously there is much in th last part of Feyerabend's paper with which I agree, but to describe the argument as a defence of irrationality in science seems to me not only absurd but vaguely obscene.

Thomas Kuhn

Introduction

The debate about scientific revolutions that has raged now for over twenty years (a debate presaged and encouraged by the French scholars Gaston Bachelard and Alexandre Koyre earlier this century) is critical to the understanding of scientific change, and particularly those upheavals we associate with 'heroic' figures such as Copernicus, Galileo, Newton, Lavoisier and Einstein. It is crucial too in calling into question the authority (legitimacy) of science. Those among the disputants who have now gained the upper hand and succeeded in establishing, to the satisfaction of most scholars, their 'new image of science' find it either qualifies or denies the objectivity and rationality of scientists' theory-choices and the possibility of progressive improvement in knowledge — ideals that have commonly supported the authority of science in the past. So let us ponder what it is that constitutes a scientific revolution. It is obviously change of some sort, but how deep, how broad and over what duration?

How often have scientific revloutions occurred, and what implications do they carry for our understanding of reality and rationality? In this section we want to try and indicate something of the range of answers that have been given to questions like these.

We begin with a concrete example. Changes wrought in physical theory during the sixteenth and seventeenth centuries have been conceived by scholars as constituting not just a revolution in science but 'the scientific revolution' *par excellence*.

Use the first three readings (Refs 3.1, 3.2 & 3.3) to answer the following questions:

Read Reference 3.1: Kearney, 'Science and change 1500-1700'.

Read Reference 3.2: Hall, 'Introduction to the scientific revolution 1500-1800'.

Read Reference 3.3: Krige, ''Revolution and discontinuity'.

Who were the leading participants in 'the scientific revolution'?

What elements constituted it?

Was it discontinuous with past knowledge or did it effect a progressive improvement in human knowledge?

Can one speak sensibly of a scientific revolution lasting as long as these people (Kearney, Hall and Krige) would have it when the analogue idea of political revolution is usually construed as a sudden and dramatic occurrence?

Was the scientific revolution unitary, or a number of separate smaller revolutions? Before this century the idea of such a protracted revolution (e.g. the title of A.R. Hall's study, 'The scientific revolution 1500-1750') would have been alien to most thinkers. Deriving from the Latin *revolutio*, which signifies the movement of objects from one place to another, the term revolution had no prominence in Western thought before the appearance in 1543 of Copernicus's seminal treatise, *De Revolutionibus Orbium Caelestium* (Revolutions of the Heavenly Spheres). English political thinkers then began gradually to draw on the astronomical idea of planets moving in fixed circles around the sun as a metaphor for describing the overthrow of governments, and the reversion of constitutions to earlier forms. Later, this sense was displaced by the idea of change of magnitude and suddenness — breaking from the past rather than returning to it. For Enlightenment thinkers such as d'Alembert, Diderot and Condillac, scientific revolutions represented subversions of traditional beliefs by new items of proven, authoritative scientific knowledge.

The conception of social and political revolutions as protracted ongoing processes is exemplified by the French and Industrial Revolutions and, more recently, by the Bolshevik Revolution whose apologists, I.B. Cohen tells us, reckoned 'the calendar in years of the Revolution, rather than years since the revolution; so that the revolution itself became an era' (I.B. Cohen, 'The eighteenth century origins of the concept of scientific revolution', *Journal of the History of Ideas*, vol.37, 1976, p.258). This is the generic model which historians draw on when describing developments from Copernicus to Newton as 'The Scientific Revolution'.

The Theme of Continuity

Pervasive in contemporary philosophy of science is the idea that science is not a process of accretion in which the stock of scientific knowledge has new items added by each generation of scientists. Theories are seen rather as often displacing and replacing predecessors, and it is these substitutions that are nowadays commonly designated as 'scientific revolutions'. The problem is how to characterise them. Crucially, are there continuities that permit scientists to make informed decisions about new theories being cognitively superior to predecessors — continuities in other words that are able to underwrite scientific progress? Karl Popper, Ernest Nagel and Carl Hempel believe it to be so as, in rather less definite ways, do Imre Lakatos and Larry Laudan.

Popper's canons for rational theory-choice are stated clearly by Krige:

> to be considered as a reasonable alternative to the prevailing view it is, minimally, demanded by Popper of a new theory 'that it solves those problems which its predecessor solved and those which it failed to solve'. But this is not all. There are two further so-called 'formal' requirements which it must meet: the requirement of simplicity, and the requirement of independent testability. By the first, Popper means that the new theory should unify two previously unrelated theories.... And, at its most straightforward, the requirement of independent testability amounts to a demand that new and unexpected predictions be derivable from the novel conjecture. Both of these conditions, if met, indicate that an increase in content has been achieved. (J. Krige, *Science, Revolution and Discontinuity*, Harvester Press, Brighton, Sussex, 1980, p.53.)

There is a definite echo of this in Lakatos, with his attempt at a philosophical synthesis of the (for him) part-truths of Kuhn and Popper. As described by Lakatos, the bulk of scientific activity proceeds within 'research programs', the principal components of which are metaphysical 'hard cores' and theories modified by auxiliary hypotheses. Essentially, he agrees with Kuhn's theory of paradigms and normal science, but looks on Kuhnian revolutions as 'irrational, a matter for mob psychology'. (I. Lakatos & A. Musgrave, eds, *Criticism and the Growth of Scientific Knowledge*, Cambridge University Press, London, 1974, p.178.) And so Lakatos seeks to develop:

> criteria of progress and stagnation within a programme and also rules for the 'elimination' of whole research programmes. A research programme is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is as long as it keeps predicting novel facts with some success ... it is stagnating if its theoretical growth lags behind its empirical growth, that is, as long as it gives only post hoc explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme ... If a research programme progressively explains more than a rival, it 'supersedes' it, and the rival can be eliminated (or, if you wish, 'shelved'). (I. Hacking, *Scientific Revolutions*, Oxford University Press, Oxford, 1981, p.117.)

Kuhn and Incommensurability

Let us turn to Kuhn, author of the most detailed and influential of all theories of scientific revolution. His patterning of science will be familiar to you: comparatively long epochs of normal science devoted to refining and extending knowledge within paradigm frameworks, and each epoch terminated by a revolution.

Read Reference 3.4: Kuhn, 'The structure of scientific revolutions'.

What for Kuhn are the components of scientific revolutions?

What elements, in other words, change during these revolutions?

Are competing paradigms amenable to systematic, logical discriminations?

What does Kuhn mean by incommensurability?

Are scientific revolutions irrational for him?

What objections do Toulmin and Greene raise against Kuhn's theory?

Read Reference 3.5: Toulmin, 'The problem of conceptual change'.

Read Reference 3.6: Greene, 'The Kuhnian paradigm and the Darwinian revolution in natural history'.

Barnes has identified important resemblances between those putative types which Kuhn wants to differentiate sharply as normal and revolutionary science.

Read Reference 3.7: Barnes, 'T.S. Kuhn and social science'.

Specify his reasons for saying that 'Kuhn's insistence upon the "necessity" of scientific revolutions is misplaced' (p.86).

What does he mean by 'judgements of sameness' (p.87) in science and their significance for it.

Does the normal-extraordinary division of science hold water for him?

Feyerabend and Uneven Development

Two important themes in Feyerabend's theory of scientific revolutions are that scientific frameworks are subject to 'uneven development' and are often incommensurable with one another.

The idea of uneven development is prominent in Marxism as Feyerabend notes. For Marx, '"secondary" parts of the social process, such as demand, artistic production may get ahead of material production and drag it along', and it also refers to the fact that 'capitalism has reached different stages in different countries, and even in different parts of the same country'. (P. Feyerabend, *Against Method*, New Left Books, London, 1975, p.147.) Feyerabend's point is that scientific theories commence in a parlous state; they clash with intuitions and observations, and are limited in what they explain. It takes time to resolve such problems, during which empirical discrepancies need to be ignored or concealed (Feyerabend, *Against Method*, p.143). In Feyerabend's view (unlike the suggestions of Kuhn and Popper) scientific revolutions are not dramatic episodes of short duration, but eras or epochs of constructing disciplines from the ground up.

Let the following quotation from *Against Method* attest its themes of uneven development and incommensurability respectively.

> Theories which effect the overthrow of a comprehensive and well-entrenched point of view, and take over after its demise, are initially restricted to a fairly narrow domain of facts, to a series of paradigmatic phenomena which lend them support, and they are only slowly extended to other areas. This can be seen from historical examples ... and it is also plausible on general grounds: trying to develop a new theory, we must first take a step back from the evidence and reconsider the problem of observation ... Later on, of course, the theory is extended to other domains; but the mode of extension is only rarely determined by the elements that constitute the content of its predecessors. The slowly emerging conseptual apparatus of the theory soon starts defining its own problems, and earlier problems, facts, and observations are either forgotten or pushed aside as irrelevant ... This is an entirely natural development and quite unobjectionable. (Feyerabend, *Against Method*, p.176)

On what are supporters of a new theory to base their allegiance? According to Feyerabend, support should be based chiefly on **passions** or **urges**, and with **faith** in its ability to surmount present difficulties. The resources of reason are of no real assistance; with all their deficiencies new theories cannot be **rationally preferable** to entrenched theories for which scientists have long been devising elaborations, refinements, auxiliary ideas and refashioning experience. Feyerabend supports this view in *Against Method* with an extended case study of Galileo's defence of the Copernican theory. The concept of incommensurability, is the subject of Reference 3.8.

Read Reference 3.8: Feyerabend, 'Against method'.

Construed as a thesis about different languages and about language-learning, what does Feyerabend mean by incommensurability?

How does Feyerabend answer Popper's objection 'that even totally different languages (like English and Hopi, or Chinese) are not untranslatable and that there are many Hopis and Chinese who have learnt to master English very well' (Popper quoted in Feyerabend, p.273).

According to Feyerabend, the suspension by one theoretical framework of the 'universal principles' of another 'establishes incommensurability' (p.277). Discuss this in relation to his comments concerning classical physics and relativity theory (p.275).

Discuss some of the conditions and qualifications attached by Feyerabend to incommensurability in science (e.g. p.279). Does he believe that all scientific theories are incommensurable (p.284)?

Does Feyerabend's doctrine on incommensurability mean that rational discrimination between scientific theories is impossible (pp.284-5)?

SELECT BIBLIOGRAPHY

Barnes, B. *T.S. Kuhn and Social Science*. Macmillan, London, 1982.

Barnes, B., & Dolby, R.G.A. 'The scientific ethos: A deviant viewpoint'. *Archives Europeennes De Sociologie*, vol.11, 1970, pp.3-25.

Barnes, B., & Edge, D. (eds). *Science in Context*. Open University Press, Milton Keynes, 1982.

Barnes, B., & Shapin, S. (eds). *Natural Order*. Sage, London, 1979.

Basalla, G. (ed.). *The Rise of Modern Science*. Heath, Lexington, 1968.

Bonelli, H., & Shea, W. (eds). *Reason, Experiment and Mysticism in the Scientific Revolution*. Science History Publications, New York, 1975.

Brown, H. *Perception, Theory and Commitment*. University of Chicago Press, Chicago, Ill., 1979.

Butterfield, H. *The Origins of Modern Science*. Bell, London, 1973.

Chalmers, A. *What is This Thing Called Science?* 2nd edn, University of Queensland Press, St Lucia, 1982.

Clagget, M. (ed.). *Critical Problems in the History of Science*. University of Wisconsin Press, Madison, WIS., 1959.

Cohen, I.B. 'Physical phenomenon'. *Social Studies of Science*, vol.11, 1981, pp.33-62.

Cohen, I.B. 'The eighteenth century origins of the concept of scientific revolution'. *Journal of the History of Ideas*, vol.37, 1976. pp.257-8.

Cohen, I.B. *Revolution in Science*. Harvard University Press, Cambridge, Mass., 1985.

Collins, H.M. 'Stages in the empirical programme of relativism'. *Social Studies of Science*, vol.11, 1981, (special issue), pp.3-10.

Collins, H.M. 'Son of seven sexes: The social destruction of a physical phenomenon' *Social Studies of Science*, vol.11,(special issue), 1981, pp.33-62 .

Collins, H.M. 'Knowledge and controversy: Studies of modern natural science'. *Social Studies of Science*, vol.11, (special issue), 1981.

Collins, H.M. 'Knowledge, norms and rules in sociology of science'. *Social Studies of Science*, vol.12, 1982, pp.299-308.

Collins, H.M. 'An empirical relativist programme in the sociology of scientific knowledge'. In K. Knorr-Cetina & M. Mulkay (eds). *Science Observed*. Sage, London, 1983.

Collins, H.M., & Pinch, T.J. *Frames of Meaning: The Social Construction of Extraordinary Science*. Routledge & Kegan Paul, London, 1982.

Feyerabend, P. *Against Method*. New Left Books, London, 1975.

Feyerabend, P. *Science in a Free Society*. New Left Books, London, 1978.

Frankel, E. 'Corpuscular optics and the wave theory of light: The science and politics of a revolution in physics'. *Social Studies of Science*, vol.6, 1976, pp.141-84.

Gilbert, F. 'Revolution'. In *Dictionary of the History of Ideas*. Scribners, New York,1973.

Gutting, G. (ed.). *Paradigms and Revolutions*. University of Notre Dame Press, Notre Dame, Ind.,1980.

Hacking, I. (ed.). *Scientific Revolutions*. Oxford University Press, Oxford, 1981.

Hall, A.R. *The Scientific Revolution 1500-1750*. Longmans, Harlow, 1983.

Hall, A.R. *From Galileo to Newton*. Dover, New York, 1981.

Hollis, M., & Lukes, S. (eds). *Rationality and Relativism*. Blackwell, Oxford, 1983.

Kearney, H. *Science and Change 1500-1750*. Weidenfeld & Nicholson, London, 1971.

Knorr-Cetina, K., & Mulkay, M. (eds). *Science Observed*. Sage, London, 1983.

Krige, J. *Science, Revolution and Discontinuity*. Harvester Press, Brighton, Sussex, 1980.

Kuhn, T. *The Structure of Scientific Revolutions*. 2nd edn, University of Chicago Press, Chicago, Ill., 1973.

Kuhn, T. *The Essential Tension*. University of Chicago Press, Chicago, Ill., 1977.

Lakatos, I., & Musgrave, A. (eds). *Criticism and the Growth of Scientific Knowledge*. Cambridge University Press, London, 1974.

Laudan, L. *Progress and its Problems*. Routledge & Kegan Paul, London, 1977.

Newton-Smith, W. *The Rationality of Science*. Routledge & Kegan Paul, London, 1981.

Nelkin, D. (ed.). *Controversy: Politics of Technical Decisions*. Sage, London, 1979.

Porter, R., & Teich, M. *Revolution in History*. Cambridge University Press, London, 1986.

Reingold, N. 'Through paradigm-land to a normal history of science'. *Social Studies of Science*, vol.10, 1980, pp.475-96.

Roller D.R. (ed.). *Perspectives in the History of Science and Technology*. University of Oklahoma Press, Norman, Okla., 1971.

Toulmin, S. *Human Understanding*. Clarendon Press, Oxford, 1972.

4

GALILEO AND THE CATHOLIC CHURCH

Prepared for the course team by J. Schuster

Galileo and the Catholic Church

> Many theories of knowledge are morality plays set in a Manichaean cosmos. The source of light is experience; its agent 'reason'. The source of darkness is culture; its agent authority. The remaining dramatis personae are garbed according to their origins. Truth, validity, rationality, objectivity are to be seen among the many white-apparelled children of the light; error and irrationality, custom, convention, dogma and many others are dressed in black. The moving principle of the drama is the unremitting conflict of the two opposed and irreconcilable forces.
>
> There is nothing to be said in favour of this Manichaean mythology. Culture and experience interact at all times as knowledge grows; they operate symbiotically, as it were, not in conflict. None the less the myth is widespread and significant, and the habits of thought it favours must be taken into account. In particular, the term, 'reason' is so widely understood as cognition and inference without a social component that there is little alternative but to give it this sense in what follows. The reader must simply understand that in asserting the insufficiency of 'reason' in science, the text will in no way imply that scientists are unreasonable men; rather it will oppose an intolerably individualistic conception of cognition and will inference. The argument will not be that something the opposite of what is reasonable or rational guides inference in science, but that the entire framework wherein the reasonable and the social stand in opposition must now be discarded. (B. Barnes, *T.S. Kuhn and Social Science*, Macmillan, London, 1983, p.22.)

In October 1632 Galilei Galilei, one of the greatest figures in the origins of modern science, was summoned to Rome from his home in Florence to appear before the Inquisition of the Roman Catholic Church. He was the most famous astronomer in Europe, sixty-nine years old, sick and soon to go blind. Earlier that year he had published his *Dialogue Concerning the Two Chief World Systems*, in which he had argued for the truth of the Copernican system of astronomy. Galileo was summoned because what he had taught, that Copernicanism was true, was a violation of Catholic doctrine. Sixteen years before, in 1616, the Congregation of the Holy Office had decreed that to hold the Copernican theory to be true was heresy, a violation of the literal word of the Bible on a matter pertaining to faith and morals. Galileo was now 'vehemently suspect of heresy'; he was tried, found guilty and made to abjure — that is, to deny his scientific beliefs. On 22 June 1633, he knelt before his judges and recanted:

> I, Galileo, ... arraigned personally before this tribunal and kneeling before you, Most Eminent and Lord Cardinals Inquisitors General ... having before my eyes and touching with my hands the Holy Gospels, swear that I have always believed, do believe, and with God's help will in future believe all that is held, preached and taught by the Holy Catholic and Apostolic Church ... I have been judged to be vehemently suspected of heresy, that is of having held and believed that the sun is the center of theworld and immobile and that the earth is not the center and moves.
>
> Therefore, ... with sincere heart and unpretended faith I abjure, curse, and detest the aforesaid errors and heresies ... and I swear that in future I will never again say or assert ... anything that might cause a similar suspicion toward me ... (G. De Santillana, *The Crime of Galileo*, University of Chicago Press, Chicago, Ill., 1955, p.312.)

What views about the relation between science and religion does the 'Galileo affair' suggest to you?

Was this a one-off unfortunate event, or is conflict inevitable and if so, why?

The classic statement of the thesis that there must inevitably be conflict between science and dogmatic Christian theology derives from two late-nineteenth-century US authors, John William Draper, a noted chemist, and Andrew Dickson White, the first President of Cornell University. Draper placed the 'anti-science' onus of religion squarely on the Roman Catholic Church; immersed in battles over the secularisation and modernisation of teaching and research in US colleges and universities, he castigated dogmatic theology as the culprit, whether found in Catholic or Protestant guise. Nevertheless, White's central thesis speaks for both sides:

> In all modern history, interference with science in the supposed interest of religion no matter how conscientious such interference may have been, has resulted in the direst evils both to religion and to science, and invariably: and,on the other hand, all untrammeled scientific investigation, no matter how dangerous to religion some of its stages may have seemed for the time to be, has invariably resulted in the highest good both of religion and of science. (A.D. White, *A History of the Warfare of Science with Theology in Christendom (1896)*, vol.1, Arco, London, 1955, p.viii.)

Read Reference 4.1: Draper, 'History of the conflict between religion and science'.

What image of scientific knowledge is implied by Draper? What image of religion?

What factors in the Galileo affair made for inevitable conflict in Draper's view?

The purpose of this section is to use the case of the Galileo affair to explore the thesis that the views about the content, methods and limits of scientific knowledge which prevail at any given time in any given social domain (including within the scientific community) are always the contingent outcome of historical processes of conflict and negotiation, coloured and conditioned by varied social interests, cultural and political priorities. That is, what for the time being is accepted as scientific truth, as acceptable scientific method and as the proper relation of scientific knowledge to other bodies of belief is the revisable and contingent upshot of processes of negotiation, interpretation and possibly conflict amongst 'interested' groups and individuals.

At first sight it might appear that the Galileo affair offers a straightforward illustration of the implausibility (and uselessness) of this thesis. White and Draper surely imply that there is no case worth discussing for the thesis: Galileo was essentially correct and correct because of his adequate use of the proper method(s) of science. The Church and its scholastic allies were defending a clearly false world view simply because it legitimated the social and political power of the Church and the interests of the upper echelons of its bureaucracy. Galileo's defence of science, its autonomy and its proper limits is *ipso facto* correct, and to challenge it is simply to misunderstand these matters, a misunderstanding charitably explicable by one's social interests getting in the way of a clear appreciation of the truth. We shall see, however, that although the conflict thesis lives on in various guises and can be refurbished to fit the political, cultural and philosophical concerns of the modern world, it cannot be sustained on historical grounds. As a consequence the Galileo affair takes on a new historical complexity. We are invited to see it not as a conflict between two knowledge systems and their social backgrounds, one system simply true, the other false; one background progressive and enlightened, the other reactionary and oppressive. Rather, we may come to see it as a more typical (if heated and explosive) example of a struggle among variously competing and overlapping groupings within one social system to impose for the time being a definition of scientific truth and of its proper relation to religion.

A consequence of this is that one might begin to see the conflict thesis as an attempt by scientifically and positivistically minded individuals and groups to constitute the official historical record in such a way as to reflect and support their views about what science is and what its relation to religion and other domains of culture should be. This in turn raises a further crucial point. If the conflict thesis view of the Galileo affair is an interested one, a value-laden construction of the past, then it is likely that the other interpretations one encounters are such as well. Interpretations of the Galileo affair can be construed as a series of attempts to win contemporary battles (for contemporary reasons and interests) over the definition of scientific knowledge, its limits and its social role. All depictions of the Galileo affair, all interpretations of its causes and delineations of its consequences are subject to the same stresses of value- and interest-laden negotiation; for to accept a particular depiction of the affair is to accept that depiction as 'evidence' for the view of science which went into its construction.

This does not necessarily mean that one can say literally anything in the historiographical debate about the Galileo affair. Historical accounts at the very least must 'take account' in some way of interpretations which support, contradict or modify their thesis. There is to some considerable degree a universe of historical discourse into which competing interpretations have to be launched. For example, no historian can claim 'divine revelation' as the basis for her or his interpretation of the affair, and those arguments which are proffered are subject to analysis according to the (largely tacit and open-endedly revisable) standards of adequate evidence and argument then prevailing in the field. Be that as it may (and it indicates that the problem of the nature of historical knowledge poses problems similar to those encountered in earlier sections about scientific knowledge), the important point is that historical interpretations are as much part of the problem — part of the debate over defining science and its limits — as they are solutions to the problems of 'what really happened' in the Galileo affair.

In recognition of these problems, and because of the lack of a suitable textbook for this section, the course team has prepared a taped lecture outlining the Galileo affair. This account does not claim objectivity in any ultimate sense. It does claim to be based on a synthesis and exploitation of some of the more recent perspectives on the Galileo affair, of which two need to be noted. First, the taped lecture accepts to a large degree the views of moderate Catholic apologists concerning Galileo's entry into the theological debate over Copernicanism. In particular it accepts their conclusions about Galileo's theological shortcomings and the negative effect upon the Catholic hierarchy (which was initially tolerant of the debate) of Galileo's pro-Copernican campaign of the years 1610–1615. Second, the lecture reflects a view of science and its history which in the light of earlier sections of this course may be termed 'Kuhnian'. This includes among other tenets the claims that traditions of scientific research are always loaded with scientific, metaphysical and value orientations which constrain and facilitate work within them; that there is no single, unique method for producing or evaluating scientific knowledge claims, since all living fields of research, dominated by particular 'paradigms', are characterised by *sui generis* 'methods' of problem solving; that whatever is accounted as scientific knowledge within such a tradition results from a social process of construction, interpretation, persuasion and negotiation carried out by experts within the relevant community, with the proviso that such communities are variously subject to, dependent upon or open to larger 'influences' of various types — economic, political, cultural, religious, etc. The lecture, therefore, is sceptical of claims that Galileo had grasped the truth, that his findings constituted proof of the Copernican theory or that he had a method for reliably producing such results. The lecture takes seriously the cognitive claims of theories which competed with Copernicanism, the astronomy of Tycho Brahe and the terrestrial physics of Aristotle. It also takes seriously the commitment of almost all of the historical figures involved to the proposition that theological arguments and methods have relevance(s) to the question of determining scientific truth. With these perspectives a different sort of story emerges which can serve as a *de facto* base line for the remainder of our case study.

Listen to the taped lecture, noting as many contrasts as possible with the account offered by Draper in Reference 4.1.

Despite the emergence of new perspectives in the history and philosophy of science which underlie the position taken in the taped lecture, the conflict thesis lives on in modern scholarship, assuming new and sophisticated forms as a result of being expressed in changing social and political contexts. Giorgio de Santillana's important work, *The Crime of Galileo*,(University of Chicago Press, Chicago, Ill., 1955) analyses the affair again on the basis of a much wider command of the documentary record than was available to Draper and White. In addition, it modulates the conflict thesis into a new key, one redolent of the atmosphere of the post-World War II United States, the Cold War and the 'liberal' reluctance of that period to attack religious belief as such, as opposed to the particular bureaucratic structures which uphold such beliefs and which might be open on independent argument to charges of propagating unenlightened or unprogressive social attitudes.

Read Rerference 4.2: de Santillana, 'The crime of Galileo'.

(Students who intend to write an essay from this section should read at least the whole of ch. VII, 'The how he shifts the terms of the conflict, blaming it upon the arbitrary and authoritarian political apparatus of the Church, duped by (its own) agents provocateurs into setting up and acting upon presumed 'reasons of state'.)

How convincing are de Santillana's parallels to the abuses of the Stalinist state?

How convincingly does he thereby separate the theocratic state (and its allies) from the religious belief system it supported and which legitimated it?

How do both of these moves reflect the Cold War US Liberalism of the 1950s?

Does de Santillana's last paragraph make sense, with its implication that Galileo had to act as he did because the issue of Copernicanism fell outside the category of 'slow change of opinion' concerning Nature which 'would eventually register' in the Church's doctrine?

De Santillana has little to say about the content and status of Galileo's scientific work, the conflict thesis being spun out through a detailed reinterpretation of the nature of the 'anti-science' grouping involved. An alternative way of reinterpreting the conflict thesis is to play upon the resources of modern scientism, in particular nineteenth-century positivism and its logical positivist and Instrumentalist offspring in this century, in order to recast the conflict by depicting Galileo as a hero and harbinger of the Enlightenment and its supposed culmination in the triumphs of nineteenth and twentieth-century 'scientific culture'. Ludovico Geymonat, a leading Italian historian and philosopher of science, of a positivist and instrumentalist persuasion, has written a scientific biography of Galileo in this vein.

Read Reference 4.3: Geymonat, 'Galileo Galilei'.

(Students who intend to write an essay from this section should read all of ch. 5, if not the whole book.)

Note Geymonat's attempt to delineate contending parties within the Church: progressive Jesuits and reactionary Dominicans, not to mention enlightened 'high prelates'.

How consistently does Geymonat's explanation work with these categories in his book, and in the end does he sustain the attempt to depict progressive and reactionary parties in the Church?

What philosophical and ideological implications are contained in Geymonat's easy equation of science–Enlightenment–'modern culture'.

The conflict thesis, even when modulated into new forms commensurate with contemporary historical standards and contemporary social, political and philosophical concerns, still leaves us with a Manichaean epistemology. Galileo possesses genuine scientific knowledge, properly and methodically obtained, and which moreover stands in its correct relation to theology (as defined by Galileo himself in his writings on theology). The world view espoused by the Church (or by triumphant reactionaries within it) is simply false, its view of the relation of theology to science is mistaken and retrograde. Any move away from the conflict thesis must involve a questioning of the allegedly privileged status of Galileo's scientific achievements, and an investigation of his presumably authoritative pronouncements on the proper relation between science and theology. Furthermore, any examination of these issues must be historiographically adequate: it must arguably reconstruct the situation as faced and understood by the relevant historical actors, and not as it appears in anachronistic retrospect. In addition, it will probably depend upon social, cultural and political interests at variance with those behind the various versions of the conflict thesis. We turn now to these issues and their implications for alternative reconstructions of the Galileo affair.

Galileo's *Letter to the Grand Duchess Christina* (1614) was written during the height of his early pro-Copernican campaign and in response to the rising level of Biblical objections to Copernican theory. It has often been lauded as a lucid and essentially correct definition of the relationship which an autonomous and objective science should bear to theology, and accordingly it has become standard grist for the conflict theorists' mill. In Reference 4.4 Jerome Langford, a Franciscan and extremely competent Galileo scholar, summarises the positive interpretation of the *Letter*.

Read Reference 4.4: Langford, 'Galileo, science and the Church (1)'.

What does Galileo hold to be the proper relation of science to theology? Why, according to Galileo, should science be granted a large degree of autonomy from theology?

Note Galileo's use of the expression 'sense experience and necessary demonstration' when speaking about the 'proof' of the Copernican system. His opponents also tended to accept such locutions. What would modern philosophy of science say about this notion of proof in physical matters?

How is the debate likely to have been complicated for both sides, given their acceptance of this notion of proof?

It is only superficially surprising that over time the Church itself has moved to endorse the sorts of principles of Biblical exegesis advocated by Galileo in his *Letter*. As early as 1893 Pope Leo XIII endorsed the type of approach to scriptures urged by Galileo. More recently, in 1983, Pope John Paul II, addressing a group of scientists and referring to his decision to launch an expert historical, philosophical and theological inquiry into the Galileo affair, put the following view of the relation of science to the Church.

Read Reference 4.5: Pope John Paul II, 'The responsibility of science'.

Note the parallels between the Pope's position on exegesis and that of Galileo.

How does the Pope explain the (contingent) fact of Galileo's difficulties with the Church of his day?

Why do you think that the Church has had to become so accommodating to Galileo's scientific claims and to his views on theology?

The question of whether it is wise, necessary or enlightening to conflate a desire to accept modern science with a desire to embrace Galileo's expressed views should be kept in mind during the remainder of this section.

One can seriously question whether the Church's apparent acceptance of Galileo's definition of the science–theology relationship is the last word in historical understanding of the Galileo affair. The portions of Galileo's *Letter* discussed thus far have been highly selected. A more comprehensive survey of the *Letter* suggests that Galileo's arguments are (and were) problematical, theologically dubious or at least ill advised in the context of his time, a context he was importantly influencing through his campaigning for the outright acceptance of Copernicanism as the truth. In Reference 4.6 Langford tries to demonstrate that an apologetic strategy need not surrender the field to Galileo, the theologian, and that such an alternative strategy arguably rests on a more convincing reading of the *Letter* than we have thus far seen.

Read Reference 4.6: Langford, 'Galileo, science and the Church (2)'.

What theological errors and scientific overstatements does Langford attribute to Galileo?

What was their likely effect on the relevant parties in his audience, for example, the progressives and reactionaries described by Geymonat in Reference 4.3. Langford, despite disclaimers in the 'Preface' to his book, is also an apologist of sorts (but unlike the traditional type).

How does his argument advance what can be seen as apologetic interests?

Differing cultural and political interests can sometimes lead in the same direction of historical interpretation. Galileo's theological errors and tactical weaknesses have also been canvassed by Arthur Koestler (*The Sleepwalkers: A History of Man's Changing Vision of the Universe*, Penguin, Harmondsworth,1964), an early harbinger in many ways of the new style of historiography of science. Koestler was no apologist for the Church, but he was an avowed critic of the scientific and creative stature of Galileo (compared with his own hero and exemplar of scientific creativity, Kepler).

Read Reference 4.7: Koestler,' A shifting of the burden'.

Note the similarity between Koestler's analysis of the Letter and that of Langford. But note also how Koestler raises in this context the issue of Galileo's scientific evidence and the question of his motives (and hence of the wisdom of his tactics).

Before turning to the question of the status of Galileo's scientific claims in the eyes of relevant contemporaries, we must briefly examine the drift of official Church thinking about Copernican theory and Biblical interpretation in the period immediately preceding the condemnation of 1616 — that is, in the wake of Galileo's pro-Copernican campaign of the years 1610 to 1615. The position of the higher echelons of the Church was perhaps best captured by a letter written in 1615 by the highly influential Cardinal Bellarmine. The letter has been quoted in part by Geymonat in Reference 4.3. In Reference 4.8 we cite it in full from Langford.

Read Reference 4.8: Langford, 'Scriptural objections'.

What do you think of Bellarmine's arguments?

What do you think he was signalling to Galileo and other Copernicans concerning Church policy on Copernicanism and the Biblical question

Do not be surprised if your own reading of Bellarmine's letter contains uncertainties and ambivalences. Such important texts do not speak for themselves — they must be viewed in context — but the reconstruction of the context (and the consequent interpretation of the text) is, of course, both coloured and made possible by the particular constellation of knowledge and interests which characterises the historian (or contemporary actor) in question. Documents only really become 'evidence' for historians or 'backing' for contemporary actors after such processes of reconstruction and interpretation have occurred. Langford, for example, continues on to a stinging critique of Bellarmine's theological arguments.

Read Reference 4.9: Langford, 'Scriptural objections'.

What errors are attributed to Bellarmine?

Do you think Langford means to imply a further critique of Bellarmine's intentions?

How does this reference serve and illustrate Langford's possible apologetic concerns?

Taking Langford's critique of both Galileo's and Bellarmine's letters as 'factual', what new interpretive light is cast on the conflict thesis?

Langford's argument that there are theological errors in Bellarmine's letter is fair textual comment from an expert in the field; it can certainly contribute toward a sophisticated apology for the events of 1616 and 1633. But in this procedure there lurks a potential pitfall well known to historians of science and of politics — the danger of 'Whiggish' interpretation. This is the evaluation of past events, actions and beliefs in terms of standards, values and beliefs held today and unavailable to the historical actors concerned within their own frames of meaning and value. To avoid the problems of Whiggishness historical evaluations must be made relative to some arguably adequate depiction of the belief system and structure of relevances of the actors concerned. With this in mind consider Koestler's account of the Bellarmine letter, its background and Galileo's response.

Read Reference 4.10: Koestler, 'The parting of the ways'.

Note Bellarmine's strategy according to Koestler.

Would you agree with Koestler's implication that it was reasonable and fair in terms of Church doctrine (as then understood) and in terms of the status of the scientific debate (as then understood)?

Note how Koestler tries to link an analysis of the letter to Galileo's tactics and behaviour in late 1615 and early 1616, the period leading up to the issuing of the condemnation.

It is a fact that Galileo and Bellarmine agreed on the necessity of 'proof' of Copernicanism (but not presumably on the probability of such proof ever appearing)! This fact and the scepticism of some historians and philosophers of science concerning the status of Galileo's scientific claims raises the crucial issue for those still committed to an historical and epistemological Manichaean dualism of science–anti-science. What was the status of Galileo's scientific claims and arguments in their contemporary context? This is the issue which conflict theorists, positivists and even most apologists have been unable or unwilling to frame, because the answer has seemed a foregone conclusion. As far as science goes, Galileo was clearly right, or so much closer to the truth than his opponents that the interpretation of the affair and the assignment of 'fault' essentially rests on this 'fact'. The standing of Galileo's achievements with the conflict theorists is indicated in Reference 4.11

from Andrew Dickson White, and in Reference 4.12 from Stillman Drake, a distinguished Galileo scholar of positivist learnings.

Read Reference 4.11: Dickson White, 'The war upon Galileo'.

Read Reference 4.12: Drake, 'Discoveries and opinions of Galileo'.

Recalling the taped lecture and your prior study of the philosophy of science, do you think Galileo's contemporaries had to be as convinced by Galileo's evidence and arguments as Galileo was, and his apologetic scholars are?

How convincing is Drake's appeal to Galileo's having discovered and used the proper method of science?

All the above questions take on an even more challenging demeanour when considered in relation to the work of the philosopher Paul Feyerabend. In his brilliant and controversial *Against Method* (New Left Books, London, 1975) he offered a detailed analysis of Galileo's evidence and arguments in defence of Copernicanism, an analysis based on a radical commitment to the principles of the new historiography of science mentioned earlier. (Students who intend to write an essay on this section should read at least chs 6 to 11 of *Against Method.*)

Feyerabend argues that Galileo recognised that the Copernican theory was in 'even deeper trouble' than the Ptolemaic with respect to the facts. There was not only the problem of the moving earth, but also the fact that, for example, Venus, to the naked eye, varied neither in brightness nor shape as required by Copernican theory. Galileo's contribution was of course his telescopic observations which showed among other things that Venus had phases like the moon and varied considerably in brightness. The problem that Feyerabend emphasises is the reliability of such observations. Galileo claimed that the telescope was 'a superior and better sense', that he succeeded in building it through 'a steep study of refraction' and that he had tested it 'a hundred times on a hundred thousand stars and other objects'. Feyerabend shows that he had neither a theory for the telescope nor empirical warrant. At the time celestial and terrestrial objects were thought to be different and would react differently to light. And indeed they are different in that we are familiar with seeing terrestrial objects and can use familiar clues to evaluate our impressions. We can also check them against our unaided vision by going closer. Even more problematic was the fact that the telescopes of the time often gave double images, coloured rings, and even made stars look square. Consequently, it was often impossible for Galileo to get groups of observers to agree on what they had seen. Thus, Feyerabend concludes that Galileo resorted to rhetoric and propaganda to achieve the acceptance of his theory which lacked the evidential support he claimed for it. Further, he argues that such tactics are justified and if any of the prescribed methodologies of philosophical orthodoxy, such as conjectures and refutations ,had prevailed the consequences would have been disastrous.

What would a reasoned defence of the Tychonic system have looked like in the light of Feyerabend's contentions and our earlier consideration of the state of the theological debate?

To be fair, Galileo did not rest his claim to proof on the telescopic observations *per se*. Rather, the evidential *pièce de résistance* was supposed to be his theory of the tides, backed by an argument premised on the fact that sun spots are carried along by the axial rotation of the sun. Koestler summarises these claims and their contemporary evaluation both in principle or in fact.

Read Reference 4.13: Koestler, 'The trial of Galileo'.

Feyerabend has neatly summarised the structure of Galileo's strategy.

Read Reference 4.14: Feyerabend, 'Against method'.

For Feyerabend, Galileo's strategies illustrate the point that no one formal methodology has ever been necessary or sufficient to produce or appraise scientific novelty. Innovators have had to proceed in the way Galileo is depicted as having proceeded: unmethodically and with unavoidable appeals to rhetoric and propaganda. Feyerabend, unlike Koestler, is far from debunking or discounting Galileo's genius or his contribution to science. Nevertheless, if we take Feyerabend's analysis seriously, the Manichaeanism common to Draper and White, de Santillana and Geymonat seems threatened with collapse, along with the apparently Galileo-worshipping tendency of Pope John-Paul II's apologetic.

Limitations of space preclude an examination of the content of Galileo's *Dialogue Concerning the Two Chief World Systems*, the curious history of its licensing by Church authorities, or the tortuous history of the trial itself. In conclusion it is worth noting that just as the conflict thesis founders on alternative (equally interested) accounts of the theological debate and the scientific state of play, so the image of a monolithic, self-serving and repressive theocratic bureaucracy softens when one considers the external political forces to which it itself was subject, as depicted by such conflict theorists as Geymonat and de Santillana.

Read Reference 4.15: Geymonat, 'The collapse of the Galilean program'.

Although de Santillana (who makes similar points) would no doubt class these matters under the heading of 'reasons of state' (see Ref. 4.2), they may indicate that the ultimate 'targeting' of Galileo was a contingent matter, more an issue of bad policy made in the light of a disintegrating larger political situation than the theological goal of a bureaucratically engineered vendetta. It would, therefore, be worth considering whether the Galileo affair was inevitable at all; whether Galileo might not have largely but contingently triggered the condemnation of 1616, and Urban VIII largely but contingently made him the scapegoat in 1633.

Appendix: Apologetic and the Construction of the past 1980s style

It might be thought that the Church's new interest in investigating the Galileo affair (see Ref. 4.5) would issue in a form of apologetic influenced:

1 by the new perspectives in the history and philosophy of science canvassed earlier in this section; and

2 by the re-evaluation of the theological debate offered in References 4.6 to 4.10.

This may still come to pass, but to judge from a recently -published study by a member of the Pope's inquiry, the strategy of historically based apologetic will run along somewhat different lines. Olaf Pedersen, a distinguished historian of astronomy, has recently published a study entitled 'Galileo and the Council of Trent: The Galileo Affair Revisited' (co-published in the Inquiry's *Studi Galileiana* and *The Journal of the History of Astronomy*, vol.14, 1983, pp.1-29). He eschews the issue of the standing of Galileo's claims, and, as was indicated by the Pope, he endorses Galileo's definition of the theological debate.

A close look at Pedersen's argument and its strategy will form a suitably instructive, if perhaps not quite fully intellectually satisfying conclusion to the section.

In history, as in science, the definition of one's problem is crucial. Pedersen states his thus:

> One of the more enigmatic features of this state of affairs is that the debate has gone on for 350 years without producing any real convergence of opinion which would once and for all settle the matter, by providing an answer to the fundamental question of why the ecclesiastical authorities of the seventeenth century were able to make a decision which was nothing but a serious blunder. (Pedersen, 'Galileo and the Council of Trent', p.1)

Galileo scholarship, Pedersen continues, has reached the point at which we can obtain a 'rather precise answer' to the first important issue: What happened? And the second key question: How did it happen? is also better understood than ever before. Thus it is the third and most important question: Why did it happen? upon which he will concentrate.

The course of 'what happened', in Pedersen's view, starts (rather late) in February 1615, when the Dominican Lorini denounced certain 'Galileisti' to Cardinal Millino, a member of the Holy Office. Lorini informed him that the Galileisti:

> Taught that the Earth was moving, and that Holy Scripture was concerned only with matters of faith ... but not with philosophical or astronomical questions ... although these men no doubt were both good men and good Christians. This moderate note did not deceive the Cardinal ... He accordingly informed (the Holy Office), an investigation was started, witnesses called from far and near, and at long last the Congregation was able to lay two questions before its 'qualificatores', or panel of theological experts. (Pedersen then reports the terms of the February 1616 condemnation.) (Pedersen, 'Galileo and the Council of Trent', p.2)

Thus, Pedersen telescopes a year of inquiry into a few sentences, a year of inquiries which some historians, moreover, consider to have been inconclusive and not essentially related to the summoning of the 'qualificatores' by Pope Paul V in February 1616 (see Ref. 4.10).

Turning to: How did it happen?, Pedersen recounts Galileo's astronomical discoveries, the opposition of various Aristotelian professors, the theological assault by Colombe and Caccini and the background to the composition of the *Letter to the Grand Duchess Christina* (see Ref. 4.7). The agitation of Galileo's opponents is described down to Lorini's denunciation to Rome of the 'Galileisti', the event with which he commenced the account of 'what happened'. Given this symmetrical if somewhat contrived and loaded packaging of the first two questions, Pedersen can proceed to the third.

He dismisses answers to the third question involving:

1 inevitable conflict between science and theology;

2 a Marxist version of (1); and

3 the view that the Church's opposition was based on a temporary judgment of pastoral necessity — to save souls from yet another revolutionary shock to their traditional world view, following hard upon the Reformation and the discovery of the New World.

This leaves a fourth option — to accept the series of events catalogued under 'how did it happen?' as the reason why. That is, one:

> tries to locate the reasons for the proceedings against Copernicanism in a string of accidental circumstances closely related to Galileo's person and scientific position in society (a position he attributes to de Santillana)! This approach has the advantage of being based on detailed investigation of the evidence and would accordingly appeal more to the historian than the three other attempts outlined above ... Nevertheless, this approach cannot provide the whole truth. If the Church was lured into action by the machinations of a more-or-less organised group or secret society, it may be exonerated

from the accusations of being hostile towards science as such, and also of having fundamentally changed its attitude. But this acquittal must, then, necessarily be followed by an accusation of stupidity since it implies that the Holy Office was unable to grasp the heart of the matter ...

However, everything considered, it is difficult to imagine that the extremely serious condemnation of Copernicanism would be the result of a series of petty disturbances ... Everything points to the conclusion that the grave step taken in February 1616 must have been motivated by equally grave reasons, at least seen from an ecclesiastical point of view. None of these possible reasons has been unveiled by any of the four types of explanation which were summarized above. Since most of these have approached the problem from the point of view of the history of science, or the history of society, the conclusion must be that we have to search elsewhere for an explanation which would make the seriousness of the cause comparable to that of the effect. (Pedersen, 'Galileo and the Council of Trent', p.11)

What is Pedersen's apparent strategy as revealed by the argument thus far?

How does he narrow the historical issue to one of whether or not to attribute 'stupidity' to the Holy Office?

What do you suppose will be the form of his explanation by non-attribution of stupidity. The options would seem to be:

1 **the 'grave reasons' given in the condemnation itself were essentially correct (not a likely tack);**
2 **deliberate bureaucratic oppression trumped up the 'grave reasons'; or**
3 **there was some sort of (historically forgivable) mistake about the 'grave reasons'.**

Pedersen next assesses Galileo's campaign for the truth of Copernicanism in 1615 in Reference 4.16.

Read Reference 4.16: Pedersen, 'Galileo and the Council of Trent: the Galileo affair revisited', pp.12-14.

Note with regard to Reference 4.10 (Koestler) how Pedersen tries to excuse Galileo from the charge of having been too insistent on having proof for Copernicanism by linking Bellarmine and Galileo as **both** being ignorant of the spurious nature of Osiander's 'Preface' to Copernicus' book. In addition, note Pedersen's wistful observation that Galileo really should have relied on Kepler's work, not on his own 'false' theory of the tides.

Is it likely Kepler's work constituted 'proof' in 1615, especially for Galileo's intended audience, or is it Pedersen who is 'Whiggishly' impressed?
Is this just an unfortunate failure on Galileo's part, which focused the issue on theology, or, following Reference 4.10 (and the tenor of the study questions posed after Refs 4.7, 4.10 & 4.11), was Galileo's insistence on truth, adherence to the theory of the tides and general strategy a (or the) crucial part of the story?

Turning to the theological question, Pedersen insists that Galileo had to enter the theological debate with the *Letter to the Grand Duchess Christina* for two reasons:

1 His earlier draft, the *Letter to Castelli*, had circulated freely and in some case in unauthorised and inaccurate copies.

2 Galileo was seeking support from the educated Italian elite who might understand his theological arguments but not his technical scientific ones. He therefore addressed his theology to them, in Italian, despite the fact that he was an amateur in the field and might attract the attention of expert theologians.

Nevertheless, Galileo decided to make an appeal to both groups by means of a single treatise in Italian, presumably fully aware of what he was up against (p.15).

Read Reference 4.17: Pedersen, 'Galileo and the Council of Trent: the Galileo affair revisited', pp.15-17.

Note how Pedersen must strive to save Bellarmine from the obvious charge of a momentarily self-serving fundamentalism (see Ref. 4.9), and hence how Pedersen conveys a sense of the 'rationality' and necessity of Galileo's theological campaign: Bellarmine was capable of being convinced, and as it turned out, Galileo produced rather good arguments along those lines but nobody in authority listened. (See study questions after Refs 4.6 & 4.7)

Not surprisingly, Pedersen finds the key portion of Galileo's *Letter* convincing and adequate to the demands set by official arbiters of theology, as represented by Bellarmine.

Read Reference 4.18: Pedersen, 'Galileo and the Council of Trent: the Galileo affair revisited', pp.20-3.

Compare Langford and Pedersen on the status of Galileo's arguments.

Compare Pedersen and Pope John-Paul II on the issue of correct principles of exegesis.

Having thus constructed the situation as having essentially turned on theological issues, and having lauded Galileo's performance by what he takes to have been the standards of evaluation of the time (i.e. his reading of Bellarmine's letter), Pedersen is ready for the closing stage of his argument.

Read Reference 4.19: Pedersen, 'Galileo and the Council of Trent: the Galileo affair revisited', pp.23-4.

Note how Pedersen finally apologises for the condemnation, (i.e. supplies a reason 'why'), having manoeuvered all the pieces into place previously to permit this construal.

Given our study to this point, has Pedersen adequately posed or answered the question: Why?

One might ponder the transformed question of whether there is an inevitable conflict between adequate historiography and the needs of apologetic.

ANNOTATED BIBLIOGRAPHY

This bibliography is subdivided by topic, with entries listed alphabetically within each topic.

Highly Recommended Reading

Drake, S. (ed.). *The Discoveries and Opinions of Galileo*. Doubleday, New York, 1957.
Translations of important works by Galileo with useful editorial explanations by Drake.

Feyerabend, P.K. *Against Method*. New Left Books, London, 1975.
A brilliant de-construction of Galileo's strategies by a radical representative of the new history and philosophy of science.

Koestler, A. *The Sleepwalkers: A History of Man's Changing Vision of the Universe*. Penguin, Harmondsworth, 1964.

Langford, J.J. *Galileo, Science and the Church*. University of Michigan Press, Ann Arbor, Mich., 1966.

Also

Galileo Galilei. *Dialogue Concerning the Two Chief World Systems (1632)*, of which there are two modern English translations:

Drake, S. (tr.). *Dialogue Concerning the Two Chief World Systems*. 2nd rev. edn, University of California Press, Berkeley, Calif., 1962, 1967.

De Santillana, G. (tr.), *Dialogue on the Great World Systems*. University of Chicago Press, Chicago, Ill., 1953.

Recommended Reading

Broderick, J. *Galileo, The Man, His Work and His Misfortunes*. Harper & Row, New York, 1964.

Drake, S. *Galileo at Work: His Scientific Biography*. University of Chicago, Chicago, Ill., 1978.

Geymonat, L. *Galileo Galilei: A Biography and Inquiry Into His Philosophy of Science*. translated by G. de Santillana. McGraw-Hill, London, 1965.

Gosselin E., & Lerner, L.S. 'Galileo and the long shadow of Bruno'. *Archives Internationales D'Histoire Des Sciences*, vol.25, 1975, pp.223-46.
A speculative reconstruction of the political issues and local contingencies motivating Urban VIII's turning against Galileo in the early 1630s.

Pedersen, O. 'Galileo and the Council of Trent: The Galileo affair revisited'. *Journal of the History of Astronomy*, vol.14, 1983, pp.1-29.

De Santillana, G. *The Crime of Galileo*. University of Chicago Press, Chicago, Ill., 1955.

Taylor, F.S. *Galileo and The Freedom of Thought*. Watts, London, 1938.

Background Reading on the Scientific and Astronomical Revolutions

Easlea, B. *Witch-hunting, Magic and the New Philosophy: An Introduction to Debates of the Scientific Revolution — 1450–1750.* Harvester Presss, Brighton, Sussex, 1980.

Kuhn, T.S. *The Copernican Revolution.* Harvard University Press, Cambridge, Mass., 1966.

Shea, W. *Galileo's Intellectual Revolution.* Macmillan, London, 1972.

Tychonic Astronomy

Kuhn, T.S. *The Copernican Revolution.* Harvard University Press, Cambridge, Mass., 1966.

Easlea, B. *Witch-hunting, Magic and the New Philosophy: An Introduction to Debates of the Scientific Revolution — 1450–1750.* Harvester Press, Brighton, Sussex, 1980.

Goodfield, J., & Toulmin, S. *The Fabric of the Heaven.* Hutchinson, London, 1961.

Brahe, T. 'On the most recent phenomena of the aetherial world'. In M.B. Hall (ed.). *Nature and Nature's Laws.* Harper & Row, New York, 1970.

Westman, R.S. 'Three responses to Copernican theory: Praetorius, Brahe and Maestlin'. In R.S. Westman (ed.). *The Copernican Achievement.* University of California Press, Berkeley, Calif., 1975.

5

SCIENCE AS TEXT

Prepared for the course team by Ian Reid and Terry Stokes

Science as Text

In this section we will be examining the scientific paper as a literary form. Though scientists are not usually thought of as writers, they are — and are frequently — very prolific. In the course of a career, a successful scientist will publish a great many short papers announcing the results of original research. Overwhelmingly, this is the predominant form in which scientists express themselves in print. Thus, for example, in modern science the monograph is almost never the mode in which research is first published. Of course, scientists do write books — but they are usually textbooks for students at one level or another. Similarly, they also undertake critical reviews of recent research in a specialty. These are intended for practitioners in different but related fields. It is successful scientists who are asked to write textbooks and literature reviews; however, their achievements are not measured in those terms. Scientists are judged by the quantity and quality of their short, original research papers.

A typical scientific article is from one to half a dozen pages long. By contrast, in the humanities the normal range is more like ten to thirty pages. Even the most important and famous scientific articles are often very short.

READ: Appendix 1 of Reference 5.3 which reproduces Watson and Crick's Nobel Prize winning announcement of their double helical structure for DNA. It is a 'letter' (short paper) to *Nature*. As you can see, it is barely more than a page long. Yet it is one of the seminal scientific documents of the twentieth century.

What would you expect to characterise scientific papers?

What do you think their function is?

Why do you think that scientific articles are comparatively short?

Does Watson and Crick's paper fit with your expectations? Or do you think it is a special case?

Scientific articles often include substantial amounts of compact, non-verbal material — mathematical formulae, tables, charts, photographs, and so on. The scientist usually writes for 'the moderate specialist' — as one journal advises — with considerable understatement. In fact, a great deal is assumed. Consequently, a reader must be familiar with current technical literature in the area. Moreover, research that an academic trained in a non-scientific field would publish as a single article, a scientist would more likely break into several. Finally, as an encouragement to brevity (and in order to raise funds), many scientific journals levy a substantial charge per page published on authors.

A further contrast between scientific and non-scientific writing lies in the typical number of authors per paper. Most articles in the humanities have only a single author. Many scientific papers which report observations or experimental results have multiple authorship. A single paper may list as many as half a dozen, or even more co-authors.

Day offers this definition: 'A scientific paper is a written and published report describing original research results'. (R.A. Day, *How to Write and Publish a*

Scientific Paper, 2nd edn, ISI Press, Philadelphia, Pa, 1983, p.1.) And so it is. But the same could be said of an historical article — by no means the same kind of thing. So, as Day says, that:

> short definition must be qualified ... by noting that a scientific paper must be written in a certain way and it must be published in a certain way, as defined by three centuries of developing tradition, editorial practice, scientific ethics, and the interplay of printing and publishing procedures. (Day, *How to Write*, p.1)

Indeed, there is more to be said by way of qualification than can possibly be discussed here, however briefly. Nevertheless, a good beginning may be made by dispelling some myths about what a scientific article is.

Read Reference 5.1: Medawar, 'Is the scientific paper a fraud?'

In the light of the Watson-Crick paper, how do you assess Medawar's argument?

Crick himself observed of the famous letter to *Nature*, 'the structure (for DNA) is produced like a rabbit out of a hat, with no indication of how we arrived at it'. (F. Crick, 'The double helix: A personal account of the discovery of DNA', in G.S. Stent (ed.), *The Double Helix: A Personal Account of the Discovery of DNA*, Norton, New York, 1980, p.139.) But, as O'Connor and Woodford advise novice scientific authors, 'a scientific paper should not be the history of an enquiry, but its outcome'. (M. O'Connor & F.P. Woodford, *Writing Scientific Papers in English*, Pitman Medical, Fearon, 1978, p.3.) By and large, that is what they are, and are taken to be — reports of the results of research, not slices of intellectual biography. Thus, the contentious question of whether the logic of scientific method is inductive or deductive is irrelevant. As for Medawar's characterisation of the results and discussion sections of a scientific paper, consider the advice O'Connor and Woodford (1978) offer:

> Make the results section comprehensible and coherent on its own. Even if you are planning to write a detailed discussion section later, do not merely describe here a series of experiments without any indication of their purpose. Allow yourself to make connections! (O'Connor & Woodford, *Writing Scientific Papers*, p.23)

Nevertheless, the structure for scientific papers sketched by Medawar is the archetypal form for reporting experimental work (the constraints are noticeably more lax for the rarer theoretical article). However, though universally endorsed by it, this pro forma is not learned from the 'How to Write a Scientific Paper' genre. Scientists learn to write as they learn to do scientific research — in the course of an apprenticeship in the laboratory as postgraduate students, and as journeymen postdoctoral fellows. Early in this period novice scientists first participate in the research for and writing-up of an article. To begin with, the papers will be written by more senior co-authors. But the apprentice scientists will gradually play a more active part in the process, eventually themselves writing up the papers which will form the core of a Ph.D. dissertation. Day describes as crucial two features of a scientific paper: namely, that it must be 'in a form whereby peers of the author can repeat the experiments and test the conclusions, and ... (published) in a journal ... readily available within the scientific community' (Day, *How to Write*, p.3).

Why do you think Day makes repeatability a crucial feature of scientific papers?

By whom do you think experiments are repeated and their conclusions tested?

Certainly not by those who read the paper — unless work they subsequently do and which is based upon its results fails to live up to expectations. If a paper is published, then it will be because the editor of the journal concerned has been advised of its acceptability by at least two specialists. The identity of these referees generally remains unknown to the authors of the paper — though they usually see the written comments made on their paper, especially if changes are demanded, as they often are. It is the function of these anonymous referees to certify the validity of results.

Whilst it is impossible to know how often referees actually do repeat experiments, it would not be expected that they do so unless they had serious doubts about results. Moreover, there is evidence to indicate that repetition may not be possible. Collins reports a scientist as adhering to the following strategy.

> What you publish in an article is always enough to show that you've done it, but never enough to enable anyone else to do it. If they can do it then they know as much as you do (H.M. Collins, 'Tacit Knowledge and Scientific Networks', in B. Barnes & D. Edge (eds), *Science in Context*, Open University Press, Milton Keynes, 1982, p.54.)

This reflects the ever-present tension in science between the desire to establish claims so as to obtain credit, and the need to maintain an edge over other workers in a highly competitive enterprise. There is no reward for being the second discoverer of something. (See also H.M. Collins, 'The Replication of Experiments in Physics', in B. Barnes & D. Edge (eds), *Science in Context*, Open University Press, Milton Keynes, 1982.)

What then is the role of referees in your opinion? In what sense do they certify results?

Day's requirement of publication in a 'journal ... readily available in the scientific community' suggests that articles are primarily a form of communication between scientists. Would you agree?

Crane observes that:

> The function of a scientific paper that has been refereed and published in a journal is only secondarily to convey information. Its primary function is to serve as a statement of knowledge that has been evaluated and declared acceptable by the scientist's peers. (D. Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities*, University of Chicago Press, Chicago, Ill. 1972, p.122.)

Indeed, unless the paper proves to be of quite exceptional importance, the only communication that takes place may be that between the co-authors, editor and referees. One measure of how widely a paper is read is how often it is cited in later work. The great majority of scientific articles receive only one or no such citation (see D. Price, 'Is technology independent of science? A study in statistical historiography', *Technology and Culture*, vol.6, 1965, pp.553-68.) As is sometimes said in the advertisements that corporations occasionally place for fully-subscribed debenture issues, scientific papers often — perhaps most often — 'appear only as a matter of record'.

This is true even for important and widely-cited papers — for the scientific paper is not the main form of scientific communication. In science, as Crane (*Invisible Colleges*) showed, information flows down the informal channels of 'invisible colleges'.
Pre-prints of papers submitted to, but not yet accepted by, journals are distributed by their authors to all those they know are working in the same field. Maybe more important still are direct personal contacts made at conferences, and during visits to other laboratories. Postgraduate students and postdoctoral fellows are often sent off specifically to learn new methods and techniques. More senior scientists use their sabbatical leave for the same purpose. (See H.M. Collins, 'Tacit knowledge', esp. pp.53-4.)

What is the purpose of citation?

Why is there such strong pressure to publish in learned journals as opposed to the popular media?
Are there occasions when the convention is broken? If so, why?

It would appear that Day is correct when he says, half jokingly: 'A scientific paper is not designed to be read. It is designed to be published' (*How to Write*, p.5). Be this as it may, he also holds:

> A scientific paper is not 'literature.' The preparer of a scientific paper is not really an author in the literary sense. In fact, I go so far as to say that, if the ingredients are properly organized, the paper will write itself ... Literary tricks, metaphors and the like, divert attention from the message to the style ... Justin Leonard ... once said: 'The PhD in science can make journal editors quite happy with plain, unadorned, eighth-grade level composition (BioScience, September 1966)' (Day, *How to Write*, pp.5-6)

Read Reference 5.2: Gusfield, 'The literary rhetoric of science: a comedy and pathos in drinking driver research'.

You will note that what Gusfield calls 'rhetoric' is not to be equated with elegant stylistic flourishes, fancy figures of speech and so forth. List the features of scientific writing (or at least of the example he discusses) that Gusfield does regard as closely comparable to the features of literary texts.

Gusfield's most general point is that the medium cannot be a transparent windowpane through which 'reality' is directly visible. While scientific papers are hardly ever 'literary' in the sense of drawing attention to the artfulness of their own language, nevertheless, they do not present a purely objective and comprehensive account of the phenomena they examine. A stance of neutrality — towards subject matter, audience and one's own role as a researcher — is illusory, and the very act of making this illusion persuasive involves the author of a scientific paper in a number of devices of selection. Thus, as analysts of science as text, we can legitimately ask about the way in which this selective process works in any given instance. By what means do authors establish their credentials for learning and knowing? From what position do they perceive, interpret, conceive and present knowledge? Who is the implied audience, and what position are their readers invited to adopt? What is emphasised, and what is omitted? These are the sorts of questions in which literary or rhetorical analysis specialises. Wayne Booth — who is cited by Gusfield — sums up the general view that critics have of the fiction writer's art when, referring to claims made by Flaubert, Chekhov and others, he says:

> We all know by now that a careful reading of any statement in defense of the artist's neutrality will reveal commitment; there is always some deeper value in relation to which neutrality is taken to be good (W. Booth, *The Rhetoric of Fiction*, University of Chicago Press, Chicago, Ill., 1961, p.68.)

This general question of the 'deeper value' underlying and motivating an apparently neutral report is discussed by Law and Williams. They remark that 'significance or meaning can only be allocated to an element by putting it next to, and seeing it in relation to, other elements'. (J. Law & R.J. Williams, 'Putting facts together: A study of scientific persuasion', *Social Studies of Science*, vol.12, 1982, p.539.) Thus, in the particular organisation of a scientific paper, the author's aim:

> Is to propose a value for the paper, a value for himself, and a value for the bits and pieces so juxtaposed. It proposes a reality in which events, facts and scientists have their place. Power in science, as elsewhere, comes from the successful capacity to create and impose value. And it is for this capacity that scientists struggle when they write a paper. (Law & Williams, 'Putting facts together', p.539)

Cameron argues that, so far from being 'a trivial linguistic embellishment', metaphor 'highlights and explains the core of scientific theorising'. (I. Cameron, *Metaphor in Science and Society*, SISCON, 1974, p.8.) Metaphor — the attribution of likenesses to the superficially dissimilar — is an enormously fruitful source of new scientific ideas, often suggesting a wide range of further possibilities (consider, for example, the wave theory of light). Thus, the 'language in which ... laws and theories are expressed is not fixed and determinate, but undergoes subtle restructuring in the course of theoretical elaboration' (Cameron, *Metaphor*, p.12). There is a symmetry between the creative function of metaphor and its rhetorical function: suggestive metaphors are, *ipso facto*, persuasive. (See M. Black, *Models and Metaphors: Studies in Language and Philosophy*, Cornell University Press, Ithaca, NY, 1962.)

With these observations in mind, turn again to Gusfield's argument (Ref. 5.2). Note that what he means by 'the literary genre of the scientific report' is not simply the 'traditional form' set out by Medawar (Ref. 5.1) — that is, not just the set of orthodox conventions by which a paper's general structure and details of presentation are shaped. Reread pp.19-25 especially, and jot down brief answers to these questions:

In what respects does Waller's paper resemble narrative and dramatic modes of writing?

What instances does Gusfield observe of non-neutral phrasing?

How, according to Gusfield, does a scientific paper establish relationships between author and audience, and between author and subject (which, in Waller's case, is certain human beings)?

What is the role of metaphor in Waller's paper? Does it differ from the use of metaphor in fiction?

How does a paper claim authoritative status for its findings?

The last question raises a cluster of issues concerning the authorisation of knowledge — how professional hierarchies and interest groups influence the nature of scholarly activity, how the domains of various specialities are circumscribed. (Cameron (1974) suggests it might be by acceptance of a common metaphor — contrast the wave with the particle theory of light.) But let us focus on the way authority operates within scientific papers themselves.

Read Reference 5.3: Bazerman, 'What written knowledge does: Three examples of academic discourse'.

You will note that whereas Gusfield's emphasis is on similarities between scientific writing and imaginative fiction, Bazerman indicates specific differences between three sorts of 'knowledge-bearing texts' — none of which is 'literary', in the sense of being primarily creative — though one of them does discuss a poem and goes in for 'verbal play' and 'reverberative density' of style itself. While Bazerman is careful to point out that his examples may not be representative of all papers in the natural sciences, social sciences and humanities, his interest is still in the distinctive ways in which knowledge tends to be constructed in separate traditions.

Are Bazerman's comments consonant or dissonant with Gusfield's?

Although their purposes and terms are different, Bazerman makes some of the same points as Gusfield — for instance, in remarking that it would be an error to regard language as providing unproblematic access to reality (p.364). He also echoes Medawar when he says that a scientific article reporting the results of a piece of research is a quite different thing from 'the full range of actual activity of the scientists' who carried out the research (p.365). Like Gusfield, Bazerman draws attention to the rhetorical importance of presentational conventions and superficially insignificant details of expression (pp.365-7).

Note the four 'contexts' that Bazerman sees being mediated through each text. It is by setting up some particular relationship between these contexts that any research report produces its own kind of knowledge. They are not discrete elements, but interlinked aspects. Thus, while we may notice certain things about the perspective of an authorial persona (e.g. the way Watson and Crick use the first person plural, Merton's adoption of a companionly tone, or Hartman's display of cleverness), these are inseparable from the attitude taken in each case towards accumulated work in the same field — that is, previous literature on the same subject or in the same 'tradition'.

The last point is worth dwelling on because it involves the important nexus between authorship and authority — cognate terms that should figure as prominently in the analysis of scientific research as they do in the analysis of imaginative works of fiction. Nowadays, in literary criticism one seldom meets the old notion that an individual author's 'originality' is the source of a novel, poem or play. Rather, attention is usually directed to the complex weft of artifices and allusions that constitute what is written. Increasingly current as an indicator of this is the term 'intertextuality', which is used:

> to signify the multiple ways in which any one literary text echoes, or is inescapably linked to, other texts, whether by open or covert citations and allusions, or by the assimilation of the features of an earlier text by a later text, or simply by participation in a common stock of literary codes and conventions. (M.H. Abrams, *A Glossary of Literary Terms*, 4th edn, Holt Rinehart & Winston, New York, 1981, p.200.)

From this perspective, the author no longer has a privileged place. Roland Barthes puts the point provocatively:

> We know now that a text is not a line of words releasing a single 'theological' meaning (the 'message' of the Author-God) but a multi-dimensional space in which a variety of writings, none of them original, blend and clash. The text is a tissue of quotations drawn from the innumerable centres of culture. (Abrams, *Glossary*, p.146)

Another influential contemporary French thinker, Michel Foucault, argued similarly that although a text may include grammatical references to an 'author-function' ('I'), yet 'neither the first person pronoun nor the present indicative refer exactly either to the writer or to the moment in which he writes, but rather to an alter ego whose distance from the author varies, often changing in the course of the work'. (M. Foucault, 'What is an author?', in J.V. Harari (ed.), *Textual Strategies*, Cornell University Press, Ithaca, NY, 1976, p.152.) Moreover, Foucault insisted, that it is not:

> a characteristic peculiar to novelistic or poetic discourse. The self that speaks in the preface to a treatise on mathematics - and that indicates the circumstances of the treatise's composition - is identical neither in its position nor in its functioning to the self that speaks in the course of a demonstration, and that appears in the form of 'I conclude' or 'I suppose'.
>
> In the first case, the 'I' refers to an individual without an equivalent who, in a determined place and time, completed a certain task; in the second, the 'I' indicates an instance and a level of demonstration which any individual could perform provided that he accepts the same system of symbols, play of axioms, and set of previous demonstrations. We could also, in the same treatise, locate a third self, one that speaks to tell the work's meaning, the obstacles encountered, the results obtained, and the remaining problems; this self is situated in the field of already existing or yet-to-appear mathematical discourses. (Foucault, 'What is an author?', p.152)

Probably few authors themselves would accept either Barthes's view of their role as merely instrumental — subordinate to a 'tissue of quotations' — or Foucault's view that the appropriate question is not: Who is speaking? but: How does this kind of speech circulate? Those who produce scientific papers are most unlikely to regard the act of writing as anything less than their very own work. But the question remains: Exactly how will a writer (whether poet, scientist or any other) manage to combine the claim to personal authority with the claim to be operating within that recognised tradition of 'previous literature of the subject'? We must also bear in mind the multiple authorship of many scientific papers: Who speaks?

Note what Bazerman says about the role of 'explicit citation and implicit knowledge', and compare this with Gusfield's remarks on 'the dilemma between personalizing and removing the agent'. Does the quotation from Abrams (*Glossary*) on intertextuality fit the case of a scientific paper?

Remember Medawar's description of the section on 'previous work' in an article as being where 'you concede, more or less graciously, that others have dimly groped towards the fundamental truths that you are now about to expound' (Ref. 5.1). A comparable point is made by Ross Chambers in his recent book. He suggests that a narrator's ostensibly respectful mention of earlier texts may sometimes carry the implication 'where X was only partly successful, I will do better'. (R. Chambers, *Story and Situation*, University of Minnesota Press, Minneapolis, Minn., 1984, p.215.) Can you find specific examples of this kind of intertextuality in any of the papers discussed by Gusfield and Bazerman?

SELECT BIBLIOGRAPHY

Abrams, M.H. *A Glossary of Literary Terms.* 4th edn, Holt, Rinehart & Winston, New York, 1981.

Barnes, B., & Edge, D. (eds). *Science in Context.* Open University Press, Milton Keynes, 1982.

Barthes, R. *Image-Music-Text.* translated by S. Heath. Hill & Wang, New York, 1977.

Black, M. *Models and Metaphors: Studies in Language and Philosophy.* Cornell University Press, Ithaca, NY, 1962.

Booth, W. *The Rhetoric of Fiction.* University of Chicago Press, Chicago, Ill., 1961.

Cameron, I. *Metaphor in Science and Society.* SISCON, 1974.

Chambers, R. *Story and Situation.* University of Minnesota Press, Minn., 1984.

Collins, H.M. 'Tacit knowledge and scientific networks'. In B. Barnes & D. Edge (eds). *Science in Context.* Open University Press, Milton Keynes, 1982.

Collins, H.M. 'The replication of experiments in physics'. In B. Barnes & D. Edge (eds). *Science in Context.* Open University Press, Milton Keynes, 1982.

Crane, D. *Invisible Colleges: Diffusion of Knowledge in Scientific Communities.* University of Chicago Press, Chicago, Ill., 1972.

Crick, F.H.C. 'The double helix: a personal view'. In G.D. Stent (ed.). *The Double Helix: A Personal Account of the Discovery of DNA.* Norton, New York, 1980.

Culler, J. *Pursuit of Signs.* Routledge & Kegan Paul, London, 1981.

Danto, A.C. *Analytical Philosophy of History.* Cambridge University Press, Cambridge, 1965.

Day, R.A. *How to Write and Publish a Scientific Paper.* 2nd edn, ISI Press, Philadelphia, Penn., 1983.

Foucault, M. 'What is an author?' In J.V. Harari (ed.). *Textual Strategies.* Cornell University Press, Ithaca, NY, 1976.

Gilbert, G.N. 'The transformation of research findings into scientific knowledge'. *Social Studies of Science*, vol.6, 1976, pp.113-22.

Gilbert, G.N., & Mulkay, M. *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse.* Cambridge University Press, London, 1984.

Harari, J.V. (ed.). *Textual Strategies.* Cornell University Press, Ithaca, NY, 1979.

Hesse, M. 'The explanatory function of metaphor'. *Revolutions and Reconstructions in the Philosophy Science.* Harvester Press, Brighton, Sussex, 1980.

Knorr-Cetina, K.D. *The Manufacture of Knowledge: An Essay on the Contextual Nature of Science.* Pergamon, Oxford, 1981.

Knorr, K.D., Krohn, R., & Whitley, R. (eds). *The Social Process of Scientific Investigation: Sociology of the Sciences Yearbook.* vol.4, Reidel, Dordrecht, 1980.

Knorr-Cetina, K.D., & Mulkay, M. *Science Observed: Perspectives on the Social Study of Science.* Sage, London, 1983.

Law, J., & Williams, R.J. 'Putting facts together: A study of scientific persuasion'. *Social Studies of Science*, vol.12, 1982, pp.535-58.

Martin, B. *The Bias of Science*. Society for Social Responsibility in Science, Canberra, 1979.

Mulkay, M. *The Word and the World: Explorations in the Form of Sociological Analysis*. George Allen & Unwin, London, 1985.

O'Connor, M., & Woodford, F.P. *Writing Scientific Papers in English*. Pitman Medical, Tunbridge Wells, 1978.

Price, D.J. 'Is technology historically independent of science? A study in statistical historiography'. *Technology and Culture,* vol.6, 1965, pp.553-68.

Stent, G.S. (ed.). *The Double Helix: A Personal Account of the Discovery of DNA*. Norton, New York, 1980.

Woolgar, S. 'Discovery: logic and sequence in a scientific text'. In K.D. Knorr, R. Krohn, & R. Whiteley, (eds). *The Social Process of Scientific Investigation: Sociology of Science Year Book.* Vol.4, Reidel, Dordrecht, 1980.

Science and the Media

Most people learn about contemporary developments in science through what they read in newspapers and magazines, what they see on television or what they hear on the radio. The average layperson is unlikely to have direct access to the scientific community, or the skills to interpret specialist scientific literature. For those who have received no formal education in science at all, the mass media is their sole source of information. Others may have acquired a basic knowledge of scientific subjects at school. However, once they have left the education system, their information about science also tends to come through one or more channels of the mass media. Indeed, science itself evolves so quickly that the ideas taught in school will often be significantly modified by later developments reported in the media.

Given the importance of science in the modern world — not least being the profound impact which scientific knowledge may come to have on the life of the individual — many argue that the information provided in this way is vital for developing the type of public awareness about scientific and technical issues desirable in a modern democracy. A recent report (1985) published by the Royal Society in London entitled *The Public Understanding of Science*, for example, takes as its starting point the 'basic thesis' that:

> better public understanding of science can be a major element in promoting nationa prosperity, in raising the quality of public and private decision-making and enriching the life of the individual. (*The Public Understanding of Science*, Royal Society, London, 1985, p.9.)

What do you think the Royal Society means by 'understanding'?

What do you think it should mean?

Discuss the ways in which public understanding of science might:

1 promote national prosperity;

2 raise the quality of decision-making; and

3 enrich life.

One of the report's main recommendations on how the level of public understanding should be raised is to increase the amount of attention paid to science in the mass media, particularly in daily newspapers. Yet, the reporting of science is not an entirely neutral affair. A news story or a television program about a scientific subject may appear to be concerned solely with providing information; indeed, many scientists like to think of the mass media as performing little more than a 'conveyor belt' function, moving scientific facts from the laboratory into the public arena (even if these facts are subsequently embellished with a discussion of their social implications). But this is seldom the whole story. Just as in any other field, coverage of science in the mass media contains both explicit and implicit messages. Even the most innocent-looking scientific story not only conveys information, but also helps modify our attitudes towards, and ideas about science and scientists. Perhaps it does this by reinforcing the cultural status of the 'scientific' over other forms of knowledge, or by emphasising one 'school' of scientific thought to the exclusion of another, or by creating an arbitrary and unreal distinction between the facts of a case and its implicit values. Such a distinction allows scientists (who claim ultimate authority over the factual domain) the power to adjudicate matters in which facts and values are inextricably intermingled.

What kind of images of science would you expect to find in the media?

Do you think it is portrayed in the same way in all media?

One of the aims of this section — which concentrates primarily on the print media, especially national newspapers, but whose arguments can equally be applied to television and radio — is to explore the linkage between the two functions of science reporting in the mass media, namely between the communication of information about science (the explicit message) and attitudes towards science (the implicit message).

Read Reference 8.1: Shen, 'Science literacy'.

Consider the validity of the three types of scientific literacy which Shen proposes. Do you think the 'scientifically literate layman' can learn to 'separate the nontechnical from the technical, the subjective from the objective' (p.266)?

What do you think about Shen's example of the Green Revolution being dependent on scientific literacy?

Is the kind of literacy he calls for the same as the Royal Society's 'understanding'?

Is there a role for critical science literacy?

Read Reference 8.2: Haldane, 'How to write a popular scientific article'.

Haldane's advice to writers of popular science focuses on questions of style and organisation in the presentation of technical information. He seems to imply that 'references to Engels', and so on, are nothing more than 'padding'. Yet, almost all his own popular writings are suffused with his social concerns and political ideology.

Consider his outline for an article on the manufacture of cheese.

Might journalists of different ideological commitments contrive completely different stories while remaining faithful to Haldane's outline?

The primary function of any scientific news story or feature article, of course, is to convey information. This information can be derived directly from the results of a particular research project; science journalists working for national newspapers frequently base their stories on papers that have just appeared in leading scientific journals — in particular *Science* and *Nature* — or on research results that are presented at scientific conferences. Alternatively, an article may summarise important new ideas that are emerging in a discipline such as high-energy physics or molecular biology.

When reporting on science, the subjects selected by the media for presentation to the public, therefore, tend to coincide with those considered to be significant by the scientific community itself. There is not always agreement, however, on the **way** that science is presented by the mass media. Many scientists frequently criticise what they regard as the 'trivialisation' of scientific knowledge when complex ideas or theories, that may have taken years to develop, are reduced to the pithy prose of the popular newspaper, or — even worse — to the three or four word attention-grabbing headline. Most would prefer all stories about science to be written in the way described by biologist J.B.S. Haldane in his essay 'How to write a popular scientific article' which you have just read and which first appeared over forty years ago, but still remains topical today.

Haldane accepts, as many journalists would, that the purpose of a popular article about science should be to interest and excite the reader — even if this means excluding some information which a professional scientist might consider highly relevant. He also suggests that this can often be done most effectively by explaining the links between the subject under discussion and the everyday experience of the average reader, another common journalistic practice. But Haldane also argues that such an article should place demands on both the writer and the reader, for example, complex arguments should be described one step at a time, and the journalist should be in complete mastery of his or her subject before sitting down to the typewriter. 'It may take you twelve hours' reading to produce an intellectually honest article of a thousand words', says Haldane (p.29).

What are the main criteria that Haldane considers essential for a good, popular article on a scientific subject?

Are there other criteria that you would like to add?

How often do the articles that you have read about science conform to this ideal pattern?

Does Haldane consider the problems encountered when controversial topics are treated? For example, does he offer any advice on how a journalist should proceed when scientists disagree?

In practice, of course, science reporting frequently fails to meet the high standards suggested by Haldane. A variety of pressures operate on the science journalist which condition the way he or she is able to work including, in particular, what happens to the words produced after they have left the typewriter or computer terminal. One is the pressure of space; a journalist who has four hundred words to summarise both the main elements and the full significance of a scientific discovery — say the discovery of a new form of quasar or elementary particle — has no room to sketch out an elaborate sequence of logical steps and must, therefore, resort to what Haldane dismisses as the 'hat-and-rabbit method'. A second pressure is time. Those working on daily newspapers cannot afford the luxury of twelve hours in a library when a story 'breaks' — say the award of a Nobel prize to three scientists working in a relatively obscure field of organic chemistry — with only two hours to a deadline. Third, there is the fact that the science journalist is required to cover a wide range of scientific disciplines, and thus cannot expect to possess a detailed knowledge of more than one or two of them at most. Finally, there is the pressure of language and educational level. Journalists can only assume a minimum knowledge of technical terms in their audience, and no one reads a newspaper with a scientific dictionary in one hand. The result is that the science journalist frequently has to translate a scientific event into terms that a layperson will readily understand, even at the risk (in many scientists' eyes) of over simplifying the subject matter. Bryan Silcock, the science correspondent of the London *Sunday Times*, writes that:

> A newspaper or TV report cannot afford to have a didactic air if it is to command any attention, and in any case there is not the space for long explanations. So reporters are forced to resort to fudges, short-cuts and minor inaccuracies; calling particle accelerators atom smashers, for example, because they think their readers probably have not heard of particle accelerators but that they might have heard of atom smashers. (Brian Silcock, 'Review of Barbara Gastel, *Presenting Science to the Public*', *Nature*, vol.308, 1984, p.297.)

Many leading science journalists have no formal science training. What factors do you feel may give them an advantage over those who are scientifically trained?

What might science journalists gain from a study of the 'social studies of science' (including such areas as the history and philosophy of science and technology; the sociology of knowledge; the ethics of scientific practice; the political economy of technology; etc.)?

Read Reference 8.3: 'Anatomy of a science story'.

In this article various US science journalists discuss the constraints under which they have to work.

List the factors they raise which make it difficult for journalists to write the ideal article described by Haldane.

The preceding constraints result directly from the way in which newspapers work. They are constraints that apply to a news story about any subject, from nuclear weapons talks to football hooliganism. Scientists who, in the past, would have deplored the resulting distortion (using as their norm for communicating research results either a conventional scientific paper or the type of article described by Haldane) have increasingly come to realise that they must learn to live with and accept such limitations, however much they may dislike them, if their work is to reach the general public through science being treated as news. For example, the Royal Society report says that, in order to fulfil a responsibility to communicate with the public, 'all scientists need ... to learn about the media and their constraints and learn how to explain science simply, without jargon and without being condescending' (*Public Understanding of Science*, p.6).

At the same time, however, as the statements made by different journalists in Reference 8.3 illustrate, there are a number of areas in which tensions arise between scientists and the mass media because of the different operating principles which both communities adopt. Many scientists feel, for example, that research results should not be presented in public before they have been formally approved by the scientific community through subjection to peer review and subsequent publication in a scientific journal (the philosophy behind the so-called 'Inglefinger rule' described by David Perlman in Ref. 8.3). Journalists, in contrast, may claim that they have the right to write about any research they can find out about — particularly if it is being paid for by public funds — or if they have learned about it in advance of their professional colleagues. The pressures to disregard the scientists' demands that results should appear in the professional literature first are intensified when the results in question have important social implications.

Commenting on the rule of the *New England Journal of Medicine* not to publish research that has already been discussed in public, Hill Williams says that 'at the basis, it's a self-serving rule. The journal is doing it to build its own prestige' (p.5).

Do you agree?

What compromise does Perlman suggest, and why?

Finally, with regard to Reference 8.3, what do you think the participants mean when they use the phrase 'science story'? Their discussion is confined almost entirely to the reporting of new scientific 'discoveries' — that is, laboratory-based experimental work. Yet much, perhaps most, of the technical reportage in the mass media relates to the scientific dimensions of pressing social, political or environmental problems. List some of the constraints which journalists encounter when writing this very different sort of 'science story'.

Why do you think the whole of natural history (as in the tradition of the television documentary) was neglected in this discussion?

What does this say about the participant's ideas about what constitutes science?

Tensions between scientists and the media have traditionally led scientists to develop a substantial mistrust of journalists. Many claim that the reporting of scientific results, particularly in the daily press, is frequently distorted and inaccurate. Certainly there are plenty of individual examples where scientific news stories have given a false impression of the scientific work that they were supposed to be describing. However, is science journalism in general as inaccurate as scientists often feel it to be? Or are charges of inaccuracy more a measure of mistrust towards the practice of journalism, and the challenges that this can present towards the traditional behaviour of scientists, for example, the extent to which their control over science is being questioned?

Read Reference 8.4: Dunwoody, 'A question of accuracy'.

How does Dunwoody distinguish 'objective' from 'subjective' inaccuracies?

Which does she consider to be the more frequently referred to?

Dunwoody claims that:

> many engineers and scientists use the same criteria to evaluate a media story that they apply to their own research reports ... And since the journalistic account is dramatically different from the scientific one, the media version comes off worse for the comparison (p.197).

She notes the 'general finding that scientists may be more critical of media science coverage in general than they are of stories about themselves' (p.196), but points out that complaints about inaccuracy usually refer to the omission of information — the result of conflict between the scientist's conception and the journalist's conception of what information is or may be relevant — rather than mistaken information.

Dunwoody suggests it is also important to distinguish the direct control which the scientist can exercise over the precise information contained in a scientific paper to the lack of control which a journalist has over his or her copy.

Dunwoody ends her paper with the conclusion that 'the greatest cause of the confusion and hostility that sometimes permeate relationships between scientists or engineers and journalists is ignorance' (p.199). The Royal Society report, *The Public Understanding of Science*, suggests similarly that there is a 'strong case for ... improving the contact between scientists and journalists as a whole' (p.6).

But is a lack of mutual comprehension the whole story? Or can we not go further?

For example, what does the scientist's conception — and use — of scientific journalism tell us about science itself as a social practice (e.g. about how scientists would like to be perceived by the general public)?

To what extent does conflict arise when journalists, for reasons of their own social practice, refuse to perform the role that scientists would like them to?

If science was no more than an intellectual pursuit, it could afford to remain indifferent to outside opinion (provided this was not overtly hostile). But even if this may have been true in previous centuries, it no longer remains the case. Scientific research has, particularly since the Second World War, become a large-scale social enterprise. The continued expansion of this enterprise depends on at least two types of support from the outside community: financial backing and social status. The community's willingness, in turn, to provide both of these depends heavily on the image attributed to science by the mass media. The public will only support expanded research budgets if they have been persuaded that science is a desirable activity, and scientists are working towards worthwhile objectives. The same factors will also help to recruit students into science and technology courses in both school and university. And a positive image of science can also be used as a defence against those who have in recent years been strongly criticising the way that the scientific community traditionally runs its affairs.

For all of the preceding reasons, the scientific community has a clear self-interest in encouraging a particular image of itself — and in persuading the mass media to adopt and propagate this image. One of the most obvious ways of doing this is to adopt the standard public relations techniques used by any private company seeking good publicity for itself and its products. The past few years have seen an increasing number of scientists holding press conferences — often timed to coincide with the appearance of a scientific paper — to present their work to the press. Indeed, in the United States many universities and research institutions have full-time science writers on their staff, producing a steady stream of news releases and feature articles which often find their way, almost unaltered, into newspapers and magazines.

Such activities tend to be a more effective way of obtaining positive press coverage of a particular scientific news story than leaving it to the initiative of the individual science writer or news editor. At the same time, however, they are almost invariably concerned with more than just conveying information in a neutral way. In the most general sense, scientists are being increasingly encouraged to tell the public about their work in order to boost the image of science — and, indirectly, to justify demands to politicians to raise the funding of research institutions. Another reason for holding a press conference may be to present scientific results relevant to a particular public controversy, for example, on the dangers of 'nuclear winter' or of a suspected chemical carcinogen, with the deliberate aim of stimulating some form of action.

In both of the above cases, the desire for publicity is explicitly political. But this desire can also be more directly self-serving. One group of scientists may call a press conference to talk about their work shortly before their research grant comes up for renewal, hoping that the publicity will increase their chances of a favourable review. Another may do so to draw attention to the generosity of the sponsor of the research. A recent phenomenon has been the use of media publicity to increase commercial interest in a particular line of research. One prominent US molecular biologist, for example, held a highly publicised press conference at Harvard University to announce some important new research findings shortly before negotiating the final stage of a lucrative contract between the small biotechnology company which he headed (and which owned the patent rights to the research) and a large pharmaceutical company.

Each of these practices has contributed towards what the public relations officer of Stanford University Medical School has criticised as 'gene-cloning by press conference' (S. Andreopoulos, 'Gene-cloning by press conference', *New England Journal of Medicine*, vol.302, 1980, p.745). They rarely result in major distortions of the scientific work being promoted, but they do illustrate some of the factors which can motivate a scientist to seek publicity, in the hope that the journalist will report in a relatively uncritical way what he or she has been told. Providing that there is a 'good story' involved, most journalists will usually oblige. By fulfilling the function hoped for by the scientist in this way, the journalist is not merely helping to achieve the short-term aim of the publicity, but at the same time reinforcing in the public's mind a particular image of the research being described, an image that can have just as important a message as the science itself.

Read Reference 8.5: 'When is a scientific fact not a scientific fact?'

This item neatly illustrates the problem of interpretation in assessing accuracy.

Who do you think is right? The editors of *New Scientist*, the Scotsman's journalist, the Coal Board's press officer, the scientists from the Institute of Terrestrial Ecology?

Read Reference 8.6: Nelkin, 'Science and media: Uneasy relationship'.

Nelkin's article suggests the following question: Should science journalists be as concerned about the accuracy of the image of science they project as they are about the accuracy of the content of science they report?

Nelkin's judgments are severe. Others might argue that she takes an excessively critical view of both journalists and scientists. Many of the former do not always accept what they are told by scientists at face value and have played an important role in demasking the 'official version' of scientific events, (e.g. reporters on the London *Sunday Times* spent several years putting together the full scientific story of thalidomide, and several more fighting for the legal right to publish what they had discovered). Many scientists would agree that science should be presented by the mass media as a process with its own internal conflicts, uncertainties and social values, rather than merely as a highly polished product. The Royal Society report, for example, argues that 'everybody ... needs some understanding of science, its accomplishments and its limitations' (p.6). A far less critical view of the relationship between scientists and journalists than Nelkin's is expressed by June Goodfield in her book *Science and the Media*, which explicitly discussed the thalidomide enquiry as an example of responsible journalism.

Nevertheless, Nelkin's analysis provides an important insight. For it helps to show how, in addition to the conflicts that can arise between the practice of journalism and the practice of science, there is another level at which the two support each other in maintaining a relatively narrow definition of science as a social practice. Just as scientists may be reluctant to get drawn into a discussion of the social and political implications of their research — arguing that these are political, rather than scientific issues — so science journalists, whether by professional convention, editorial pressure or personal choice, often tend to concentrate on the technical aspects of scientific stories. Indeed even raising political issues, in the same way as a reporter of social or economic affairs might do without thinking, can be seen as a deviant lapse of objectivity by a science journalist.

Scientists may also have reasons of their own for restricting journalistic coverage of their work, as expressed in an editorial in the journal *Nature*, entitled 'Understanding begins at home'. The journal claims that the analysis of the Royal Society report is 'over-timorous' and 'may also be over-flattering to the scientific community everywhere' by failing to comment more critically on the scientific community's desire, for reasons of both social prestige and internal cohesiveness, to present itself in a good light. In particular, *Nature* attacks the reluctance of scientists to conduct debates over their intellectual differences in the public arena, as happened more frequently in the past. 'And the root cause of that development is the convention of self-certitude that has been taken up by academics, both in relation to students and, more alarmingly, among each other'. ('Understanding begins at home', *Nature*, vol.317, 1985, p.97.)

Scientists and science journalists, therefore, each for their separate reasons, can find themselves conspiring to present a particular image of science that may not always reflect the full reality. We can illustrate this further by referring to the idea of 'news as myth' suggested by Keith Windschuttle in his book *The Media*. Windschuttle uses myth in the anthropologist's sense as 'a story, factually based or fictional, a literary theme or character type that appeals to the consciousness of a social group by embodying its ideals for itself or by giving expression to deep, commonly felt emotions'. (K. Windschuttle, *The Media*, Penguin, Melbourne, 1984, p.279.) He is not concerned whether a myth is right or wrong, rather in the way in which it creates a framework of ideas through which the individual interprets his or her daily experience — in other words, a way of looking at the world. He goes on to suggest that 'what happens when a journalist recognizes a "good news story" is that he or she is bringing his own humanity and socialization to bear on a particular set of events, and picking up, instinctively, the mythical elements of his own culture' (K. Windschuttle, *The Media*, p.280).

Windschuttle uses this concept primarily to analyse the way newspapers report phenomena such as political celebrities, natural hazards — he cites the annual 'shark-infested beaches' and 'killer-spider' stories that predictably appear on the front pages of mass-circulation dailies every summer — and general forms of social deviance as news (see p.287). But there are many myths about science which tend to receive similar treatment. These include the idea that technical experts should be treated as dominant authorities, that all scientific research is both socially and politically neutral, and that scientific and technological developments inevitably lead to social progress.

Each of these ideas is firmly implanted in modern Western culture. Indeed, there are those who claim that science has become the dominant source of myths — perhaps more frequently described as the dominant 'ideology' — of our time. Reporting scientific events in a way that supports many of these myths tends to be seen as 'good journalism'. But this merely reflects the extent to which journalists, for good or ill, are engaged in reproducing the dominant mythologies of their social environment. Sociologists Jay Blumler and Michael Gurevich have remarked that journalists and those they are reporting on participate in a 'shared culture', each agreeing to keep to implicit ground rules. Dunwoody comments that one advantage of the shared culture, particularly between journalists and scientists, is that 'it minimises conflict and allows both sides to get their own jobs done efficiently'. (Sharon Dunwoody, 'The scientist as source', in S. Friedman, S. Dunwoody & C. Rogers (eds), *Scientists and Journalists*, Macmillan, New York, 1986.) The other side of the coin, however, is that this shared culture can also play a significant role in the conceptions of science that are built up in society — an act which, itself, can be seen as political, since it helps to legitimise conventional ideas about access to, and control over, scientific knowledge.

For a detailed description of the way that this process works in television, see Roger Silverstone, *Framing Science: The Making of a TV Documentary,* (British Film Institute, London, 1984) . Silverstone talks about television as myth making. He also describes how both the practical constraints and the professional conventions of television film making influenced the ways in which a television documentary on the 'New Green Revolution' was able to shift from scientific and technical to social, economic and political issues.

What type of 'myths' about science and scientists do children learn from reading comic books or watching television cartoons.

How much do you think that these myths later influence the way that they approach stories about science which appear in newspapers or television documentaries?

Read Reference 8.7: Schibeci, 'Students, science and the media in Australia'.

Schibeci reviews the literature on student's images of science and comes to the conclusion that such images are generally (1) stereotyped, (2) unrepresentative and (3) negative. This forms a vivid contrast with the view expressed by Nelkin that 'the images of science that appear in the press are usually optimistic, positive, even promotional'.

Both writers call for more realistic and more representative reporting of the actual day-by-day conduct of science. Yet their assumptions about the nature and aims of such reporting, differ markedly.

Compare and contrast Nelkin's and Schibeci's prescriptions for improving science reporting in the media.

Goodell comments that 'ironically, it is those who already know something about science, and have favourable attitudes toward science, who are most likely to read science news'. (R. Goodell, *The Visible Scientists*, Little Brown, Boston, Mass., 1975, p.135.) What are the main reasons you can think of for this?

Read Reference 8.8: Beckwith, 'How magazines cover sex difference research'.

We can see this process of myth -reinforcement at work in the analysis of research on the genetic basis of gender differences described by Barbara Beckwith. Beckwith bases her analysis on a range of articles appearing in the popular US press, and concludes that 'such coverage panders to conventional sex-role prejudices by telling readers "science" supports their biases', thus providing justification for 'keeping things as they are' (p.18). She shows how the tone in which the message is put across varies; *Playboy* and *Cosmopolitan* take a more sensationalist approach, while *Mademoiselle* and *Ladies Home Journal* adopt a gentler stance. In each case, however, the message is the same, that sexual roles — and sometimes even sexual violence — are part of our 'natural' biological inheritance, and thus something that cannot be changed. Critics of this position are quoted, but seldom with the same authority as proponents. Notice in this article that Beckwith is not addressing whether the science is correct (although it is clear that she does not believe it is); rather, she is describing how the way in which it is reported reflects and reinforces broader social values, namely the dominant 'mythology' of sex differences.

The same procedures operate even when newspapers report on topics involving the social applications of scientific knowledge that they acknowledge to be controversial. Here the mythology argues that objective 'fact' should be given priority over subjective 'values', or scientific 'reason' over political 'emotion', ignoring the extent to which such 'facts' or 'reasoning' are themselves the result of social processes. Many examples of this can be found in the media coverage of controversies over the safety of nuclear power. The critics of nuclear power frequently exploit the scientific uncertainties that exist — for example, over the effects of long-term exposure to low levels of radiation — arguing that a lack of certainty implies the need for the highest caution. Furthermore, their concerns, particularly when voiced by an eminent physicist whose authority on 'all things nuclear' tends to be part of the dominant mythology, are readily reported in newspapers, legitimised on the grounds of social interest and/or concern.

Read Reference 8.9: Welch, 'Deception on nuclear power risks'.

Welch does not comment on the role of the media in the deception on nuclear power risks, none the less it does occur through the media.

Should the media merely report the claims of the scientists?

Can they do anything else?

In contrast, supporters of nuclear power — often calling on their own white-coated experts — reject such criticisms by arguing that they lack a 'firm factual basis', and that stringent restrictions should not be placed on nuclear power, or indeed any new technology, without 'scientific evidence' that such restrictions are necessary. 'Facts seem to have fallen from fashion' (V. Godin, 'The media are the medium', *AECL Ascent*, Spring 1985, p.20) is a typical complaint from senior executives in a large Canadian nuclear company. Both sides want to claim 'the facts' as support for their views.

This would appear to leave accuracy of reporting as very insecure ground for the media to base themselves on. What position do you think is possible?

Read Reference 8.10: Godin, 'The media are the medium'.

Note that Godin argues that the 'only significance' of the Three Mile Island accident was that the nuclear industry has been unaware that the mass media has moved away from the post-war, pro-technology consensus; he complains that journalists are not interested in technical information, but only in 'intrigues, secret deals and international pressures'.

How do Godin's criticisms of the press reflect the perspectives on nuclear power that an industry executive might be expected to hold that differ from those of the general public?

To understand the way that science is presented in the media, it is necessary to do more than analyse the constraints that newspapers place on the way that 'scientific news' is handled — or conversely, the way that this coincides or conflicts with how the scientific community itself handles scientific information. We have first to look at both science and journalism as parallel social processes, with their own, frequently complementary, systems of legitimation. For, despite the differences in content, the form of scientific research and journalism are in many ways very similar. Both seek what is described as a form of truth by revealing the patterns of significance that lie behind apparently random events, the one looking at the natural world, the other at the social world. Or as David Crisp summed it up:

> Good scientists and good journalists have a great deal in common: an ingrained scepticism toward established dogma, conventional wisdom and the tyranny of commonsense; an eye for accuracy and detail; the ability to deal impartially with facts that don't fit the theory; and contempt for the pernicious theory that all passionately held ideas are of equal value. (David Crisp, 'Scientists and the local press', in Friedman et al. *Scientists and Journalists,* p.72).

For a scientist, these patterns of significance become the hypotheses (or paradigms) which are subsequently used to interpret the events being studied; for the journalist, the pattern of significance is 'the story', the dominant idea — usually expressed in the first or second paragraph — setting the framework by which individual facts and phenomena described in the rest of the article are held together. In both cases, a degree of judgment is inevitably involved in selecting the more significant events and the most appropriate paradigm — or story — into which they fit. Furthermore, an important component of this judgment is socially determined. Both science and journalism, however, seek to maintain an impression of objectivity; scientists and journalists alike frequently describe their work as a disinterested search for truth. Rather than taking such claims at face value, however, we can better understand both activities, as well as the relationships between them, if we see these descriptions as legitimations of the respective practices.

However, we have also to recognise that both science and journalism occur in social and political contexts which go beyond their specific practices. The media are largely owned practices controlled by the same groups that fund and control science. At this level it is necessary to consider how and why science journalism serves to perpetuate a mythology about science. A mythology that deliberately disguises those whose interests are being served by science.

It may be that by dispensing with that mythology and recognising, as Godin claims, that science is indeed a human experience, an experience that is as culturally embedded as art, religion or politics, we will be able to embrace the inherent tension between knowledge and criticism involved in understanding. If more science journalists and

popularisers worked towards this end, the result would not only be increased public awareness of the real nature of science, but possibly also an improvement in the practice of science itself.

SELECT BIBLIOGRAPHY

Andreopoulos, S. 'Gene-cloning by press conference'. *New England Journal of Medicine*, vol.302, 1980, pp.743-6.

Blumler, J., & Gurevitch, M. 'Politicians and the press: An essay on role relationships'. In D. Nimmo, & K.R. Sanders, (eds). *Handbook of Political Communicaiton.* Sage, Beverley Hills, Calif., 1981.

Dunwoody, S. 'The scientist as source'. In S.M. Friedman, S. Dunwoody, & C. Rogers, (eds). *Scientists and Journalists.* Macmillan, New York, 1986.

Farago, P. *Science and the Media.* Oxford University Press, Oxford, 1976.

Gardner, C., & Young, R. 'Science on television: A critique'. In T. Bennet, S. Boyd-Bowman, C. Mercer ,& J. Wodlacott (eds). *Popular Television and Film.* British Film Institute, London, 1981.

Gastel, B. *Presenting Science to the Public.* ISI Press, Philadelphia, Pa.,1983.

Goodell, J. *Science and the Media.* American Association for the Advancement of Science, Washington DC, 1981.

Goodell, R. *The Visible Scientists.* Little Brown, Boston, 1977.

Jones, G., Connell, I. & Meadows, J. *The Presentation of Science by the Media.* Primary Communications Research Centre, Leicester University, 1977.

Krieghbaum, H. *Science and the Mass Media.* New York University Press, New York, 1967.

La Follette, M. 'Science on television, influences and strategies'. *Daedelus*, vol.III, no.4, pp.183-97.

Mazur, A. 'Media coverage and public opinion on scientific controversies'. *Journal of Communication*, vol.31, 1981, pp.106-115.

Oppenheimer, J. R. 'Communication and comprehension of scientific knowledge'. *Science*, vol.142, 1963, p.1144.

The Public Understanding of Science. Report of a Royal Society ad hoc group, Royal Society, London, 1985.

Ryan, M. 'Attitudes of scientists and journalists toward media coverage of science news'. *Journalism Quarterly*, vol.56, 1979, pp.18-26.

Science and the Media. British Association for the Advancement of Science, London, 1976.

'Science: News, controversy, drama'. *Journal of Communication*, vol.31, 1981, pp.84-189.

Silock, B. 'Telling stories'. *Nature*, vol.308, 1984.

Silverstone, R. *Framing Science.* British Film Institute, London, 1984.

Sun, M. 'News of bone research causes fracture'. *Science*, vol.212, 1981, pp.906-7.

Television Evening News Covers Nuclear Energy: A Ten Year Perspective. The Media Institute, Washington DC.

'Understanding begins at home'. *Nature*, vol.317, 1985, p.97.

Welch, B. 'Deception on nuclear power risks: A call for action'. *Bulletin of the Atomic Scientists*, September 1980, pp.50-4.

Wertheim, M. *Beyond 2000.* Intercontinental, Hong Kong, 1986.

Windschuttle, K. *The Media*. Penguin, Melbourne, 1984.

READINGS

1

KNOWLEDGE MAKING:

AN INTRODUCTION

Steven Shapin

HISTORY OF SCIENCE AND ITS SOCIOLOGICAL RECONSTRUCTIONS

II. PROFESSIONAL VESTED INTERESTS AND SOCIOLOGICAL EXPLANATION 164

Within the scientific community, and within any given specialty or discipline, there will typically exist a distribution of different skills and technical competences. For example, some scientists will be more skilled than others in mathematical demonstration; some biologists will be more adept at morphological studies of animals and others will be highly skilled in biochemical analyses; within a scientific subculture there may also frequently be a division between theoreticians and experimentalists. These technical abilities and competences will have been acquired through processes of socialization; they will have represented a considerable investment on the part of the scientist, and he will naturally tend to deploy them, to show their value in scientific work and to extend the possible range of their application. Such skills and technical competences therefore represent a set of vested social interests *within* the scientific community. There is every reason why a scientist should wish to display the value and scope of what he can do, even to the extent of 165
criticizing the value and scope of others' acquired skills and competences. In the process of defending these professional vested interests conflicts may arise within the scientific community over the nature of phenomena. If nature is constituted in one way, then its investigation may best proceed through the application of one set of competences; if it is constituted differently, then perhaps another set of technical competences are called for. In this way, professional vested interests may form the middle link which connects, on the one hand, controversies about the nature of phenomena and, on the other, conflict over the availability of resources or the securing of credibility for scientists' work. The analysis in terms of socially acquired technical competences may even be extended to encompass scientists' investments in the practical or interpretive line of their previous work. If a group of scientists have accomplished a body of publicly available research in which they argue for a given point of view, theory or interpretation, they may well wish to defend that position from attack and display its value and scope over other positions—even if they are technically able to work from another cognitive or practical orientation. Naturally, there is no coercive force involved and scientists may readily shift their positions, seek to acquire other competences, or see the advisability of terminating a controversy to further shared interests. What is involved is a strategy for defending and furthering interests, based on complex calculations about the consequences of various courses of action.

There is a substantial body of history of science literature that shows the explanatory value of attending to professional vested interests. The significance of this perspective may best be shown by proceeding from the smaller to the larger scale of such interests. In November 1974 two new and unusual elementary particles (named 'J-psi' and 'psi-prime') were discovered by a group of high-energy physicists. Theorists in the community were faced with the problems of explaining the new particles' properties and of situating them within a coherent framework that also dealt with existing particles. Andrew Pickering's study of the controversy between advocates of the "charm" and "colour" models, and the quick resolution of that controversy, is built upon sensitivity to the pre-existing distribution of interests among specialist groups within high-energy physics [40]. Without entering into the technical details of each model, charm's proponents were so successful in vanquishing their colour model rivals that within eight or nine months a solid consensus in favour of charm had developed; within two years colour's advocates had been effectively isolated and the charm model had been solidly established. What was the basis of charm's success and colour's failure? Pickering demonstrates that the charm model intersected with, and could be readily integrated into, a range of existing bodies of theoretical practice in high-energy physics. For example, charm generated a puzzle — to do with rules for interpreting the longevity of certain particles employed by hadrodynamicists; it offered a solution to this puzzle which provided a programme of work for experimenters and hadron spectroscopists; and it gave support to, and generated support from, a group of important 'gauge theorists' who saw ways by which the success of the charm model could give additional credibility to the 'gauge theory revolution' in quantum mechanics. Moreover, the charm model used conceptual resources that were very widely distributed in physics; as Pickering says, "whenever the model encountered mismatches with reality the resources were available to essentially anyone to attempt to fix it up, and for others to appreciate such work" [40, p. 125]. By contrast, very few bodies of practice in theoretical physics incorporated resources associated with the colour model. Charm succeeded insofar as it was successfully insinuated into a range of bodies of practice; the greater and the more consequential the extent of that integration into practice, the more charm appeared as a fact of nature rather than a human contrivance.

In this particular case charm theorists *could have done* colour theorizing and *vice versa*; each group had acquired through their socialization into the high-energy physics subculture the competences to do both charm and colour theorizing. It was not a question of being unable either to do or to see the point of the other's theorizing. Nevertheless, given the pre-existing distribution of theoretical practices, each made the evaluation that gave most promise of validating and extending the range of applicability of its practices. Again, no coercion was evidently involved.

Let us move to consider competences within a scientific community that are not so readily acquired or discarded. One example is John Dean's examination of a series of controversies among twentieth century botanists over the correct classification of plants [31]. One group of practitioners has maintained that species are to be delineated on the basis of their morphology while the other group has claimed that experimental techniques of various kinds (including transplantation studies, cytological and biochemical work, and measures of genetic exchange) are required for a correct classification to result. These disputes have been going on since at least the 1920s and are still unresolved today. Each group is quite capable of generating its own taxonomy employing its preferred techniques. As might be expected, sometimes the taxonomies render given bits of botanical reality differently. In the case of the *Gilia inconspicua*-complex experimentalist taxonomists, using cytological findings, discern five species, while morphological taxonomists identify just one. In another *Gilia* complex (*tenuiflora-latiflora*) the situation is reversed; morphological criteria identify four distinct species while information about gene exchange points to just one. Each set of species criteria, so to speak, works, and each can be used to further the practical concerns of the classifying communities. So each group of scientists construes botanical reality differently. Each group is also distinguished by its members' acquired technical competences and, to a large extent, by the institutions in which it works. The more traditional taxonomists have been trained in morphological techniques deploying the existing Linnaean system of nomenclature and identification. Many of them work in herbaria producing monographs and flora which aim to provide clear-cut means of distinguishing taxonomic groups on grounds of their gross appearance. Botanists who have vigorously criticized the herbaria taxonomists (the experimentalists or "biosystematists") tend to have been trained in genetics, cytology, ecology and related disciplines and to work in university research departments. Thus the criteria each group advances as the basis for a proper classification act to defend and further its investments in socially acquired technical competences. The groups have on occasion competed for resources, but in the main they have worked out a *modus vivendi*, with the result that alternative techniques for classification also stably co-exist in the botanical community. 167

• • •

V. FULL CIRCLE: CONTINGENCY AND WIDER SOCIAL INTERESTS

For purely conventional reasons this paper has so far considered the role of a variety of social interests as if they were distinct and manifested themselves in separate bodies of knowledge. The time has come to correct any such impression and to attempt to put together a number of historiographic orientations which are often seen as incompatible. One traditional source of difficulty in sustaining a sociological approach to scientific knowledge comes from the view that the power and validity attributed to science is guaranteed by its freedom from 'social influences'. In this account social considerations can only work to corrupt proper science; the scholar convinced of the value of science and concerned to defend it from attack must therefore take great care before showing the presence of social interests in scientific activity. Writers in this tradition tend to read sociological accounts of scientific knowledge as aspersions, however great the pains taken by sociological writers to state otherwise. By now this particular battle has been fought so many times that it is pointless to do more than reiterate: sociological accounts have no bearing upon whatever evaluations one may wish to put upon science; indeed, the major reason why such accounts are frequently self-described as 'naturalistic' is simply that they have no evaluative axe to grind. 186 187

A related source of misunderstanding seems to stem from within certain strands of sociological thinking, namely a tendency to regard bodies of knowledge as the manifestations of single types of social interest. Knowledge used, for example, to legitimate structures in the wider society is considered to be different in kind from knowledge used for the 'prediction and control' of phenomena.[28] Since these interests are thought to be incompatible, so must the types of culture they produce. In the history of science such an impression may be reinforced by the purely conventional fact that empirical studies of particular bits of science tend to fall into distinct genres: there are studies which treat Newtonian natural philosophy as informed by technical interests in prediction and control and which situate it in a cultural tradition, and there are studies which assess the same body of culture as a legitimating resource deployed in the wider society. The historian may feel he is being asked implicitly to choose between incompatible approaches. Of course, 'choice' is not necessary, and there are already several empirical studies which explicitly make this point [104, pp. 124-31].

In a particularly concise example Lawrence has studied conceptions of the human nervous system and its functioning in eighteenth century Scotland [117]. The major empirical concern of his paper is to show that theories of nervous 'sensibility' and 'sympathy' in that setting were evaluated according to their use in justifying the cultural and social leadership of Lowland intelligentsia and their allies. But Lawrence goes on to argue that these conceptions functioned in both 'scientific' and apologetic contexts; they were, as he says, "multifunctional", and, while he does not himself show their detailed usage in medical and physiological settings, he points to a body of historical work which does display such a role. Similarly, Wynne's account of aetherial and energetic conceptions of matter in late Victorian Cambridge argues the importance of their use as an anti-naturalist, anti-professionalizing strategy, strongly linked to psychical research, without in any way claiming that such ideas did not also inform much technical work in the study of radiation [122]. But to make the point about multifunctionality [103] totally convincing, and to advance our understanding of interests and scientific knowledge generally, it is best to turn to empirical studies which themselves document the role of a variety of interests in the development of knowledge.

Let us return to the cluster of sub-cultures which encompassed the study of evolutionary mechanisms (biometry and Mendelism), the biological understanding and manipulation of social structure (eugenics), and the techniques thought requisite to eugenic theory and practical programmes. In briefly considering some empirical literature dealing with biometry and Mendelism I pointed to the possible explanatory role of the distribution of scientific competences and skills (see Sect. II above). However, some of the writers on these episodes explicitly state that such considerations are insufficient to explain the controversies: Pearson apparently possessed the competences to have embraced Mendelism if he so chose; Bateson's attachment to Mendelism cannot satisfactorily be explained by his particular experimental skills; and proposals to reconcile the two orientations were systematically rejected or ignored for quite some time. MacKenzie and Barnes [36; 37; also 35, ch. 6] thus turn to factors operating in the general cultural, social and political milieu of late nineteenth and early twentieth century Britain, that is to factors generally identified as 'external' to the natural scientific culture of that setting. If one proceeds in a traditional historical manner and concentrates on key individual actors, one can discover interesting differences in their social and political views. Karl Pearson, the major British biometrician, came from a dissenting middle-class background [119; also 35, ch. 4], and had pronounced anti-clerical, anti-*laissez-faire*, social imperialist views close to those of many Fabians. His belief in biological gradualism was paralleled by his strong commitment to progressive and gradualist social change; indeed, he believed in the application of the results of scientific investigation to social problems. The display of progressive continuity in nature underwrote a commitment to progressive and continuous social change. A commitment to continuity thus ran through Pearson's evolutionary views and his

highly-developed social philosophy. By contrast, his opponent William Bateson was connected to traditional academic elites and was deeply mistrustful of the effects of industrialization, and the ideologies of utilit- 189
arianism and evolutionary social progress. As Coleman has shown [124], Bateson was, in Mannheim's sense, an essentially conservative thinker.[19] He thought that both science and society ought to 'treasure exceptions'; just as evolution depended, in Bateson's view, upon the exceptional discontinuity, so social progress depended upon the uncontrôllable appearance of rare individual genius.

So preferences for biometrical *versus* Mendelian explanation appear to proceed from divergent social orientations; preferences for continuity theories versus discontinuity theories in the natural sciences were structured in part by conflicting interests in the wider society. These divergences also manifested themselves in attitudes towards potential courses of practical social action, particularly towards eugenics. Pearson, like many biometricians, was a committed eugenist, while Bateson, like other opponents of biometry, was deeply suspicious of eugenics. This is an association which becomes more understandable when one recognizes the extent to which biometry was developed with a view to coping with the problems posed by a eugenic view of society and by practical eugenic programmes of action [35, chs 5-6; 120]. Insofar as eugenics was the strategy of a particular interest group in British society, the biometry-Mendelism controversy was sustained by conflicting interests in the distribution of rewards, rights and privileges in the wider society. Of course, recognizing these features of the controversy supplements rather than diminishes the significance of professional competences and skills. A range of social interests, including those usually considered 'internal' and 'external' to the scientific culture, needs to be considered in order satisfactorily to explain this particular episode.

This methodological point becomes especially important when one considers some of the mathematical tools developed within this cluster of sub-cultures. For example, Ruth Cowan showed some years ago that Francis Galton's statistical theory was informed by his eugenic commitment [113; also 112; 114; 115]. She took the view that the significance of eugenics was that it provided the motivation which turned Galton towards particular statistical questions the content of which was, presumably, not dependent upon Galton's eugenic purposes. Recently, however, MacKenzie has pressed the point that "the needs of eugenics in large part determined the content of Galton's statistical theory" and has produced detailed demonstrations of this in his account of the differences between Galton's work and that of the error theorists, and in his discussions of Galton's work on regression, correlation, and the bivariate normal distribution [35, ch. 3]. However, given our present concern with the range of social interests involved in sustaining scientific knowledge, it is MacKenzie's study of statistical controversy between Pearson and G. U. 190
Yule which provides better illustrative material [35, ch. 7; 111; 118]. This was a highly esoteric controversy within early twentieth century British statistics dealing with the correct way to measure the association of data arranged in contingency tables. The controversy overlapped with the most intense phase of the biometry-Mendelism conflict and involved some of the same major actors. By 1900 there was general agreement on how to measure the correlation of normally-distributed interval variables, but it was uncertain how best to deal with nominal variables, i.e., those for which no unit of measurement existed such as 'alive' or 'dead', 'vaccinated' or 'unvaccinated'. From 1900 Pearson's approach was to

treat nominal variables in the contingency tables *as if* they were produced by an underlying bivariate normal distribution.

Pearson was aware that this was often an untestable assumption, but he nevertheless regarded his measure of correlation as the correct one; others were indeed possible, but these were treated as approximations to the tetrachoric coefficient. By contrast, Yule did not make the assumption of underlying normal distribution, and by 1905 he openly attacked Pearson's work, especially the assumptions underlying the tetrachoric. The controversy continued for a decade, involving a good part of the small British statistical community.

MacKenzie stresses that Pearson (and his followers) and Yule (and his followers) had different goals in statistical theory: the former wishing to maximize the analogy between the treatment of interval variables and nominal variables, while the latter wanted to treat nominal data *sui generis*. These differing goals MacKenzie terms divergent "cognitive interests". The two positions were incommensurable by virtue of Pearson's and Yule's differing goals in statistical theory [111]. But MacKenzie goes on to try to explain why it was that differing cognitive interests were so distributed. MacKenzie's analysis is too subtle to be briefly summarized; however, he connects the two sides' conflicting views on association with their divergent positions on eugenics. Pearson, MacKenzie shows, developed his statistical theory in the manner he did because of the requirements of the eugenic programme to which he was committed. Yule, on the other hand, had no commitment to eugenics and developed his statistical work in this area differently. And, as MacKenzie has already shown, commitment to eugenics is itself to be referred to wider social interests, such as those affecting the professional middle classes, on the one side, and traditional elite groups, on the other.

Thus esoteric work in mathematical statistics is explained by referring different views to divergent purposes within the statistical community, and also to diverging goals in the wider society. Historical work of this sort

therefore illustrates two points of relevance here: firstly, it shows beyond any doubt that the explanation of even the most technical and esoteric scientific activities *may* need to be referred to wider social interests. In this respect it has long seemed that the history of mathematics is an unusually tough nut for the sociology of knowledge to crack, but cracks have indeed begun to appear recently.[30] Secondly, MacKenzie's work, and other studies to be discussed shortly, erodes any tendency to think of wider social interests as affecting, as it were, the 'outside' of scientific knowledge (models, metaphysics and metaphors) while the esoteric core is generated solely through disinterested contemplation of reality. Any such 'two-tier' model of the sociology of knowledge gets no support here. And finally, such work reinforces the point made by studies discussed above: that institutionalized bodies of scientific knowledge may typically be sustained by a variety of social interests, and these may cross-cut historians' conventional categories of 'internal' and 'external' considerations.

By connecting interests in the wider society to judgments of the adequacy and validity of esoteric mathematical formulations we have come close to completing the methodological circle. We started by considering historical studies which showed the contingency of scientific judgments about experimental findings and matters of fact, and we have now reached the point at which we can begin to see that such judgments may well be structured by wider social interests. Let us amplify and refine this point by briefly examining two final historical studies: one dealing with the notion of a 'competent experiment' and another concerning observation reports. Farley and Geison have studied nineteenth century French controversies over the spontaneous generation of life [116]. In the late 1850s the Rouen naturalist Felix Pouchet pronounced himself convinced of the reality of spontaneous generation and published what he took to be experimental proof of the phenomenon: micro-organisms appeared in boiled hay infusions under mercury after they had been exposed to artificially generated air or oxygen. These experiments elicited immediate critical comment from Louis Pasteur: he suggested to Pouchet that his experiments had been improperly performed; contaminated air had almost certainly been introduced and this error of procedure, rather than spontaneous generation, was responsible for the appearance of life in the flasks. Immediately thereafter Pasteur undertook his own series of experiments. He took a set of flasks high on a glacier in the French Alps, exposed them to the rarified (and presumably uncontaminated) air and showed that only one developed signs of life. This one Pasteur regarded as an anomalous result—the series definitively proving that competently performed experiments refuted spontaneous generation. Pouchet felt obliged to replicate Pasteur's experiments (although the replication was not exact), and he went to the Pyrenees with his flasks, where all showed signs of life when briefly exposed. This Pouchet took to be proof that competently performed experiments established the fact of spontaneous generation—all that was needed to make life appear in an organic infusion was oxygen. Since Pasteur and Pouchet could not agree between themselves about criteria for a 'competently performed experiment', Pouchet issued a challenge which resulted in the appointment of an adjudicating commission by the Paris *Académie des Sciences*. In the event, that commission was heavily stacked in favour of those already convinced of the impossibility of spontaneous generation; some of the members announced against Pouchet even before the experiments were examined, and so Pouchet withdrew, leaving the prize to Pasteur.

Farley and Geison thus describe a situation already familiar to us from the sociological work of writers like Collins and Pickering [e.g., 4; 6; 7; 16]. There are no transcendent criteria by which the competence of experimental procedures may be judged; prior commitment to the existence or non-existence of the phenomenon in question necessarily enters into a judgment about whether or not relevant experiments have been competently performed. But Farley and Geison do not seek merely to establish the contingency of judgments about experimental procedure; they seek to identify particular contingent social considerations which structured differing judgments. As it happens, those considerations relate scientific judgments to concerns in the wider political and moral setting of mid-to-late nineteenth century France. The issue was materialism, and the consequences perceived to flow from materialism in that context. Belief in spontaneous generation seemed to imply the self-organizing capacities of matter and (echoing seventeenth century hylozoist themes) therefore to threaten the existence and role of the external spiritual agencies upon which the authority of the Church rested. While Pouchet stipulated the religious orthodoxy of his particular version of spontaneous generation, Pasteur's forces insisted upon identifying their opponent's views as heterodox and dangerous to public morality. Farley and Geison conclude that "members of the French scientific community may have chosen Pasteur over Pouchet" (and therefore have judged the competence of their respective experiments) "at least in part for socio-political reasons" [116, p. 183].[31]

In Farley's and Geison's example we see how wider social interests bore upon evaluations of experimental competence, and, therefore, upon the truth-status of experimental findings of fact. The micro-sociological focus of writers like Collins and Pickering is supplemented here by a
193 macro-sociological analysis: the relevant social factors in this case happened to include both features internal to the sub-culture of French science and considerations linking science to religious, moral and, ultimately, political discourse. It is a contingent association: one which there is no reason to expect operates in all controversies over experimental competence. One might, for example, be surprised to find views for or against the authority of religious institutions figuring in the gravity radiation controversies (but perhaps less startled to find that similar considerations might prove relevant to explaining the present-day disputes over species change). The appropriate methodological strategy derives from the historical circumstances, not from the level of culture the historian seeks to explain. This is best demonstrated by bringing our methodological excursion full circle; let us consider an episode in which actors disagreed about visual observations.

The setting is one which we have already introduced: the disputes over the validity of phrenology in early nineteenth century Edinburgh [102; 103]. We have seen that phrenology was the argumentative strategy of groups in that society which were concerned to erode the authority of existing academic and spiritual elites and to substitute for it a proto-naturalist, participatory science of man as natural object. The local controversies thus tended to array disaffected and iconoclastic bourgeois groups against traditional elites and their intellectual spokesmen; it is no exaggeration therefore to see the Edinburgh phrenology disputes in terms of the macro-sociological category of social class. Nevertheless, these controversies also involved a series of esoteric issues in cerebral and neuro-anatomy; there were disputes over the exact contours of the cranial bones, the patterns of the cerebral convolutions, the exact fibrous constitution of the hemispheres, and the fine structure of the cerebellum and the fibres connecting it with other parts of the brain and the spinal cord [121]. Participants in these disputes violently disagreed about what could be seen when one looked at these structures. There might be a temptation to separate the controversies into cosmological and methodological components, on the one hand, and esoteric, technical and 'scientific' matters, on the other. It might be thought that the former could be referred to macro-sociological considerations, while the latter must pertain solely to concerns within the sub-culture of anatomy. This proves, on inspection, not to be the case. Anti-phrenologists' insistence that cranial bones in the region of the frontal sinuses were not parallel was explicitly connected to their claim that phrenological character diagnosis was impossible; phrenologists' assertion that the cerebral convolutions might show standard pattern and morphological differentiation was explicitly related to their view that mental faculties
194 were subserved by distinct cerebral areas. Similarly, disputes over the fibrous nature of the brain mass were closely associated with conflicting views of the differentiation of cerebral organs and of mental function. As the anatomical disputes, so to speak, pushed deeper into the brain and into esoteric anatomical issues, the participants themselves seemed less able convincingly to assign social interests to their opponents' claims. Indeed, it was the public appearance of disinterestedness upon which rested, in that setting as in many others, the credibility of the claims

advanced. Nevertheless, close observation of the disputed phenomena did not lead to a convergence of claims, for observation was, apparently, a thoroughly political process and the political ends of the parties involved differed. In this case, as in the micro-sociological studies discussed in the first section, natural reality did not possess the coercive force with which actors' discourse often imbued it. Reality seems capable of sustaining more than one account given of it, depending upon the goals of those who engage with it; and in this instance at least those goals included considerations in the wider society such as the redistribution of rights and resources among social classes. . . .

For many writers a sociological approach meshes with some routine 196
practices in the history of ideas; for example, the state of knowledge at any given moment is matter-of-factly treated as the base point for cultural change. The cultural heritage is socially transmitted; no man invents his own language, and, just as the speaking of English in seventeenth century England was socially transmitted, so the heritage of, say, natural philosophical concepts and practices was socially transmitted to individual men of science, whatever innovations they then wrought on this legacy. Thus variability in concepts and practices in different settings and among different groups is frequently referred to patterns in the social agencies that transmit knowledge: schools, universities, churches, and the like. Of course, the resources that are available to solve scientific problems may not be the monopoly of formal educational and cultural institutions. People may deploy the resources provided by the experiences of living and operating in society, although, as we have seen, it is an entirely contingent matter whether scientists do so or not. Much of this is generally regarded as uncontentious, although many historians of ideas still treat contributions to culture as if they were generated *in vacuo* by atomistic individuals, and some of them continue to view the source of cultural materials used in science as a matter of moral concern.

In a sociological approach to knowledge-making, people produce knowledge against the background of their culture's inherited knowledge, their collectively situated purposes, and the information they receive from natural reality. Perhaps the most puzzling charge sometimes laid against relativist sociology of knowledge is that it neglects the role played by sensory input.[33] On the contrary, the empirical literature employing this perspective shows scientists making knowledge 'with their eyes wide open to the world'. If anything, writers such as Collins, Farley and Geison, Kohler and Pickering have been more intensely concerned with how scientists conduct experiments, focus on reality, and come to terms with the sensory information channelled by experiment than many more 'traditional' historians and philosophers from whom such criticisms often come.[34] Both in this empirical literature and in the theoretical sociology of knowledge corpus there is no question of denying the causal role of the *unverbalized reality* upon which given scientific beliefs focus.[35] What is perhaps at issue here is whether a specific *verbal formulation* of reality is
to be *privileged* in sociological and historical explanation. The historian 197
may indeed have little choice but to 'lay a bet' on the physical reality which impinged on actors, and in 'laying that bet' he might well opt for a modern text-book account.[36] However, he must remain on his guard against using that account as a *sufficient* explanation of beliefs that accord with it. If the historian succumbs to this temptation, he will indeed talk about 'natural reality' as a 'constraint' upon what is said about it. But whatever appeal this procedure may have to rationalist and

realist writers, historians ought to be aware of what is involved: for it may be nothing less than the very Whiggism and 'presentism' that historians have so generally agreed to abominate. To reject privileging specific verbal formulations of reality is not to reject the role of sensory input: it is to write more sensitive history. It is the opponent of relativist sociology of knowledge who would make the actor a "judgmental dope".[37] In this case, it is 'reality' which is said to coerce the actor.

This leads us on to what may be the most central aspect of the largely implicit 'instrumental model': the generation and evaluation of knowledge is treated as goal-directed. Knowledge is not regarded in this literature as contemplatively produced by isolated individuals; it is produced and judged to further particular collectively sustained goals. Knowledge, in this perspective, is always tailored to doing things. It is in the course of doing things with knowledge that its meaning is produced; thus, the notions of use and meaning are intertwined. We have seen this instrumentalist perspective at work both in the study of past science and its wider social relations and in the explanation of scientific controversies in present-day science. The purposes for which knowledge is produced and according to which it is evaluated may vary very widely: they may include the legitimation or criticism of tendencies in the wider society, or they may encompass goals generated exclusively within the technical culture of science. And, as we have seen, there are many instances in which both sorts of instrumental goals bear upon the production and evaluation of culture. Typically, usage and meaning will be embedded within a complex social network of calculations, such that possible connections always exist between considerations in all parts of the net.[38] As MacKenzie and Barnes conclude from their interpretive study of the biometrician-Mendelian controversies, "The general point is not that the goal-directed character of scientific judgment implies its relationship to any particular contingency, or to external factors, or political interests; what is implied is that any such contingency *may* have a bearing on judgment and that contingent sociological factors of some kind *must* have" [37, p. 205].

Finally, this brief exposition of a working instrumentalist model in the history of science allows us to reflect back upon certain aspects of the 'coercive model' that have hindered appreciation of the actual role of the social. In that model, the social was routinely contrasted to the 'rational'. However, in the empirical literature we see no such contrast. Actors are all treated as if their 'cognitive wiring' was in proper working order: that is to say, they are all possessed of 'natural rationality'.[39] That rationality is expressed in the instrumental character of their behaviour. Their calculations may, as a matter of fact, take into consideration goals pertaining to the wider society or they may not. Actors' judgments which are informed by wider social interests seem no less intelligible and competent than those which do not. Given that this is so, the only conceivable purpose to be served by equating the social with the 'irrational' is stipulating which sorts of considerations the ideal type of the modern scientist *should* take into account. Few historians will see this as an essential and proper part of their activity. It is this patently normative attitude towards 'rationality' which appears to inform the 'coercive model's' view of determination and the social. We are invited to conceive of 'social determination' as if it were a sort of mugging. But which model is it really that makes out actors as "judgmental dopes"? In an instrumentalist perspective actors are seen to produce and evaluate knowledge against the background of socially transmitted knowledge and according to their goals. The role of the social, in this view, is to prestructure choice, not to preclude choice.

I have attempted to show here that the sociology of knowledge is more than a set of theoretical and programmatic reflections upon what might be done; it is also a body of practical achievements. While there is every reason for satisfaction about the state of the empirical literature, there is no justification for complacency. Empirical writings are attended with problems as well as advantages, and it is highly desirable that practically minded historians should become more aware of the interpretive purport of their achievements: both for their own concerns with the handling of concrete materials and for the clarification of more general issues in the theory of knowledge. Given proper awareness of this work, there might also be no more talk of "the widely acknowledged failure of cognitive sociology to explain any interesting scientific episodes".[40]

A slightly different version of this paper was read to a Colloquium on Ludwik Fleck and the sociology of knowledge at Haus Rissen, Hamburg, in September 1981, and will be published in German in the proceedings of that colloquium. Part of this work was done with the support of a Fellowship from the John Simon Guggenheim Memorial Foundation.

REFERENCES

1. Joseph Ben-David, *The scientist's role in society: A comparative study* (Englewood Cliffs, N.J., 1971), 7-13.
2. A. Rupert Hall, "Microscopic analysis and the general picture", *Times literary supplement* (26 April 1974), 437-8, p. 438.
3. Larry Laudan, *Progress and its problems: Towards a theory of scientific growth* (London, 1977), ch. 7, esp. 204, 209, 219, 243 n. 10. For programmatic criticisms of Laudan's strictures: Barry Barnes, "Vicissitudes of belief", *Social studies of science*, ix (1979), 247-62; David Bloor, "The strengths of the strong programme", *Philosophy of the social sciences*, xi (1981), 199-213.
4. Especially Barry Barnes, *Scientific knowledge and sociological theory* (London, 1974); *idem, Interests and the growth of knowledge* (London, 1977); David Bloor, *Knowledge and social imagery* (London, 1976); Michael Mulkay, *Science and the sociology of knowledge* (London, 1979).
5. Nor will I treat the history of the social sciences, although I do not accept that these materials present an 'easier case' for the sociology of knowledge in anything but a persuasive sense. Perhaps the greatest losses resulting from my selective criteria are: (i) an excellent literature dealing with the cognitive foundations of research schools and disciplines; and (ii) some attempts to operationalize Mary Douglas's 'grid-group' schema. Some selected references are included in the Bibliography, Section VI (c) and (d).
6. Obviously, I cannot and do not claim scholarly competence in all the relevant areas; therefore I cannot 'vouch for' the factual accuracy of much empirical work I treat. Nevertheless, I see no major problem in presenting empirical achievements as, so to speak, 'state of the art'. Very little of this work has been challenged in print, but, where such challenges do exist and may bear upon the adequacy of interpretive perspectives, I shall make every effort to point this out in references. I must also stress that summarizing empirical studies always results in loss of detail and therefore of persuasive power. The brief sketches I provide should be regarded more as *guides* to reading empirical work, than as substitutes for reading it.
7. A positivistic sociology of the *dynamics* of science and its foci of interest appeared in R. K. Merton, *Science, technology and society in seventeenth-century England* (new ed., New York, 1970; orig. publ. in *Osiris*, iv (1938), 360-632).
8. Rudwick is preparing a full-length study of the Devonian controversy.

9. The argument establishing that in principle all experimental conclusions can be challenged was stated by P. Duhem in *The aim and structure of physical theory* (Princeton, 1954), ch. 6. If an experiment produces unexpected results or appears to refute a hypothesis, it is always possible to lay the blame on a subsidiary assumption in the test procedure. Using the usual notation of symbolic logic: if $A \cdot H \longrightarrow O$ and $\sim O$, then all that can be concluded is $\sim H$ *or* $\sim A$ where H = hypothesis; A = background assumption; O = observation. A decisive refutation would require a proof that there does not exist an alternative A, say A^*, such that $A^* \cdot H$ produces an 'acceptable' observational outcome. Since proofs of the *non*-existence of a suitable A are never available in practice, neither is a decisive or crucial experiment. These themes have been taken up by W. V. O. Quine, "Two dogmas of empiricism", in his *From a logical point of view* (2nd ed., Cambridge, MA, 1964), esp. p. 43. In Pickering's usage a 'closed' experimental system would be one in which all variables were perfectly understood and controlled, and all findings deriving from such a system would command universal assent. An 'open' system would be one which was imperfectly understood, measurements upon which would be open to a variety of interpretations. Scientists sometimes behave as if their experimental findings should be incontestable, although Pickering doubts whether such a thing as a 'closed' system exists in reality [16, p. 218].
10. An interesting study of Dirac and the monopole concept, providing background to the episode discussed by Pickering, is Helge Kragh, "The concept of the monopole: A historical and analytic case-study", *Studies in history and philosophy of science*, xii (1981), 141-72.
11. *Cf.* Karin Knorr-Cetina, "Relativism — What now?", *Social studies of science*, xii (1982), 133-6.
12. Quite recently there has appeared a programme devoted solely to analysing scientists' 'discourse': Michael Mulkay, "Action and belief or scientific discourse?", *Philosophy of the social sciences*, xi (1981), 163-71; Nigel Gilbert and Michael Mulkay, "Contexts of scientific discourse: Social accounting in experimental papers", in Knorr *et al.*, eds [9], 269-94; and a series of forthcoming papers by Mulkay and Gilbert. This programme is advanced as a way out of a "current analytic impasse" in the descriptive and explanatory project, viz. most of the empirical work discussed in this paper. We should try to analyse *how* scientists talk rather than *what* their talk is about: "It is simply impossible", according to Mulkay, "to produce definitive versions of scientists' actions and beliefs" (p. 169). There are many problems with this position, not least that relating to the claim that the discourse analyst "is no longer required to go beyond the data". It will be for others to judge whether the 'discourse project' should count as a contribution to the sociology of knowledge.
13. Recently, some aspects of Allen's work have been criticized by Jane Maienschein, Ronald Rainger and Keith Benson in *Journal of the history of biology*, xiv (1981), 83-158. Their diverse objections seem to centre upon (i) the rapidity of the shift to experimental techniques (which is not an issue in the present context), and (ii) the extent of polarization between morphological and experimental methods; the dichotomy is accepted by Allen's critics, although they wish to stress the complexity of the situation.
14. In this connection Morrell and Thackray [38, pp. 461-5] provide valuable institutional background to Rudwick's account of the Devonian controversy [22], pointing to the explanatory role of the control of resources in geology. Their study of the British Association for the Advancement of Science also offers institutional considerations relevant to explaining early nineteenth century controversies over wave versus corpuscular theories of light and differing views of the adequacy of mathematical methods in physics [38, pp. 466-84]. In this instance different evaluations were rooted in contrasted Cambridge and Edinburgh pedagogical traditions, as well as in conflicting English and Scottish conceptions of the social and cultural position of science. At the most vulgar level the disputes involved competition for students and alternative schemata for the social support of the man of science. For analyses (mostly pitched at a far less vulgar level) of these episodes: G. N. Cantor, "The reception of the wave theory of light in Britain: A case study illustrating the role of methodology in scientific debate", *Historical studies in the physical sciences*, vi (1975), 109-32, and David P. Miller, "The Royal Society of London, 1800-1835: A study in the cultural politics of scientific organization" (unpubl. Ph.D. thesis, University of Pennsylvania, 1981), ch. 3.

28. Jürgen Habermas, *Knowledge and human interests* (Boston, 1971). See discussions of this perspective in Barnes, *Interests and the growth of knowledge* (see ref. 4), ch. 1, and Shapin [108, pp. 63-65]. **202**

29. The contrast between "conservative" and "natural law" styles of thought is set out in Karl Mannheim, "Conservative thought", in *Essays in sociology and social psychology* (London, 1953), 74-164. For empirical studies utilizing Mannheim's categories, see Bibliography, Section VI (a).

30. See some selected references in Bibliography, Section VI (b).

31. It is true that some of the vocabulary Farley and Geison use in their paper invites a psychological reading of their argument: The "influence" of "external factors upon Pouchet is made to hinge upon his "sincerity" in insisting upon his orthodoxy (p. 184); we are obliged to choose whether Pasteur "allowed 'external' factors" to "influence" him "consciously" or "unconsciously" (pp. 196-7). It would seem however, that this individualism and psychologism does not sit easily with the main strands of the paper's argument, which is pitched at a sociological level

    Interestingly, a critical assessment of this paper has picked upon the psychologism and exploited its weakness· Nils Roll-Hansen, "Experimental method and spontaneous generation: The controversy between Pasteur and Pouchet, 1859-64", *Journal of the history of medicine*, xxxiv (1979), 273-92. **203**

32. The 'coercive model' (not so labelled) is most explicitly set forth in Laudan, *Progress and its problems* (see ref. 3), ch. 7, where the empirical failures of this approach are given as reasons for rejecting the sociology of knowledge.

33. There are many sources for this line of attack; perhaps the most explicit is A. G. N. Flew, "Is the scientific enterprise self-refuting?", *Proceedings of the Eighth International Conference on the Unity of the Sciences: Los Angeles, 1979* (New York, 1980), i, 347-60.

34. It is remarkable how little attention the 'Great Tradition' in the history of science has actually paid to experimental practice. Two recent major studies go some way to remedying this neglect; both point out how problematic is the connection between that practice and the theoretical culture that has been the major focus of historical interest: R. G. Frank, Jr, *Harvey and the Oxford physiologists: A study of scientific ideas* (Berkeley, 1980), and, especially, John L. Heilbron, *Electricity in the 17th & 18th centuries: A study of early modern physics* (Berkeley, 1979).

35. See, for example, Barnes, *Scientific knowledge and sociological theory* (ref. 4), esp. ch. 1; *idem, Interests and the growth of knowledge* (ref. 4), esp. ch. 1; Bloor, *Knowledge and social imagery* (ref. 4), chs 2, 8; Barry Barnes and David Bloor, "Relativism, rationalism and the sociology of knowledge", in S. Lukes and M. Hollis, eds, *Relativism and rationality* (Oxford, 1982), in the press, and Barnes papers in ref. 18.

36. See Shapin [121] for the notion of actors 'laying bets' on representations of perceived reality. In this episode the actors themselves privileged their preferred representations and provided psychological and sociological explanations of their opponents' 'erroneous' accounts.

37. Harold Garfinkel, *Studies in ethnomethodology* (Englewood Cliffs, N.J., 1967), 66ff.

38. For the 'network model': Mary Hesse, *The structure of scientific inference* (London, 1974); its sociological significance and implications for history of science have been developed in David Bloor, "Klassifikation und Wissenssoziologie: Durkheim und Mauss neu betrachtet", *Kölner Zeitschrift für Soziologie und Sozialpsychologie*, Sonderheft xxii (1980), 20-51 (an English version will shortly be appearing in *Studies in history and philosophy of science* under the title "Durkheim and Mauss revisited: Classification and the sociology of knowledge").

39. Barry Barnes, "Natural rationality: A neglected concept in the social sciences", *Philosophy of the social sciences*, vi (1976), 115-26.

40. Laudan, *Progress and its problems* (ref. 3), 219.

History of science, vol. xx, 1982, pp. 164-167, 186-203 (excerpts).

1.7

Barry Barnes
and David Bloor

RELATIVISM, RATIONALISM AND THE SOCIOLOGY OF KNOWLEDGE

In the academic world relativism is everywhere abominated. Critics feel free to describe it by words such as 'pernicious'[1] or portray it as a 'threatening tide'.[2] On the political Right relativism is held to destroy the defences against Marxism and Totalitarianism. If knowledge is said to be relative to persons and places, culture or history, then is it not but a small step to concepts like 'Jewish physics'?[3] On the Left, relativism is held to sap commitment, and the strength needed to overthrow the defences of the established order. How can the distorted vision of bourgeois science be denounced without a standpoint which is itself special and secure?[4]

The majority of critics of relativism subscribe to some version of *rationalism* and portray relativism as a threat to rational, scientific standards. It is, however, a convention of academic discourse that might is not right. Numbers may favour the opposite position, but we shall show that the balance of argument favours a relativist theory of knowledge. Far from being a threat to the scientific understanding of forms of knowledge, relativism is required by it. Our claim is that relativism is essential to all those disciplines such as anthropology, 22 sociology, the history of institutions and ideas, and even cognitive psychology, which account for the diversity of systems of knowledge, their distribution and the manner of their change. It is those who oppose relativism, and who grant certain forms of knowledge a privileged status, who pose the real threat to a scientific understanding of knowledge and cognition.[5]

[1] E. Vivas, 'Reiteration and second thoughts on cultural relativism' in H. Schoek and J. Wiggins (eds), *Relativism and the Study of Man* (Van Nostrand, Princeton, N.J., 1961).

[2] A. Musgrave, 'The objectivism of Popper's epistemology' in P.A. Schilpp (ed.), *The Philosophy of Karl Popper* (Open Court, La Salle, Ill., 1974), ch. 15, p. 588.

[3] K.R. Popper, *The Open Society and its Enemies* (Routledge & Kegan Paul, London), vol. 2, 1966, p. 393; H.R. Post, *Against Ideologies* (Inaugural lecture, Chelsea College, University of London, 28 Nov 1974), p. 2, for Jewish physics. Vivas also invokes the image of Belsen.

[4] S. Rose and H. Rose (eds), *The Radicalisation of Science* (Macmillan, London, 1977).

[5] We refer to any collectively accepted system of belief as 'knowledge'. Philosophers usually adopt a different terminological convention confining 'knowledge' to justified true belief. The reason for our preference should become clear in the course of the paper. For a full account of the ideas that form the background of this paper and a description of their implications for the sociology of knowledge, see B. Barnes, *Scientific Knowledge and Sociological Theory* (Routledge & Kegan Paul, London, 1974); B. Barnes, *Interests and the Growth of Knowledge* (Routledge & Kegan Paul, London, 1977); D. Bloor, *Knowledge and Social Imagery* (Routledge & Kegan Paul, London, 1976).

I

There are many forms of relativism and it is essential to make clear the precise form in which we advocate it. The simple starting-point of relativist doctrines is (i) the observation that beliefs on a certain topic vary, and (ii) the conviction that which of these beliefs is found in a given context depends on, or is relative to, the circumstances of the users. But there is always a third feature of relativism. It requires what may be called a 'symmetry' or an 'equivalence' postulate. For instance, it may be claimed that general conceptions of the natural order, whether the Aristotelean world view, the cosmology of a primitive people, or the cosmology of an Einstein, are all alike in being false, or are all equally true. These alternative equivalence postulates lead to two varieties of relativism; and in general it is the nature of the equivalence postulate which defines a specific form of relativism.

The form of relativism that we shall defend employs neither of the equivalence postulates just mentioned, both of which run into technical difficulties. To say that all beliefs are equally true encounters the problem of how to handle beliefs which contradict one another. If one belief denies what the other asserts, how can they both be true? Similarly, to say that all beliefs are equally false poses the problem of the status of the relativist's own claims. He would seem to be pulling
23 the rug from beneath his own feet.[6]

Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility. It is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic. The position we shall defend is that the incidence of all beliefs without exception calls for empirical investigation and must be accounted for by finding the specific, local causes of this credibility. This means that regardless of whether the sociologist evaluates a belief as true or rational, or as false and irrational, he must search for the causes of its credibility. In all cases he will ask, for instance, if a belief is part of the routine cognitive and technical competences handed down from generation to generation. Is it enjoined by the authorities of the society? Is it transmitted by established institutions of socialization or supported by accepted agencies of social control? Is it bound up with patterns of vested interest? Does it have a role in furthering shared goals, whether political or technical, or both? What are the practical and immediate consequences of particular judgements that are made with respect to the belief? All of these questions can, and should, be answered without regard to the status of the belief as it is judged and evaluated by the sociologist's own standards.

A large number of examples could be provided from recent work by historians, sociologists and anthropologists which conform to the requirements of our equivalence postulate. For example, many excellent historical studies of scientific knowledge and evaluation now proceed without concern for the epistemological status of the cases being addressed. They simply investigate the contingent determinants of belief and reasoning without regard to whether the beliefs are

[6] These are the kinds of relativism that Popper identifies as his target on p. 387 and p. 388 of his *Open Society*, vol. 2. The claim that relativism is 'self-refuting' is thoroughly discussed and thoroughly demolished in Mary Hesse, 'The strong thesis of sociology of science', ch. 2 of her *Revolutions and Reconstructions in the Philosophy of Science* (Harvester Press, Brighton, 1980).

true or the inferences rational. They exhibit the same degree and kind of curiosity in both cases.[7] Anthropologists too are increasingly
accounting for systems of commonsense knowledge and pre-literate 24
cosmologies in the same way.[8]

On the level of empirical investigation – and concentrating on the practice of investigators rather than the theoretical commentary they may provide – there is more evidence to be cited for relativism than
against it. It is mainly on the programmatic level that the determined 25
opposition to relativism is to be found. Since instances of the empirical material have been marshalled and discussed elsewhere[9] the issues that will be addressed here will be of a more methodological and philosophical character.

[7] As a selection of such work, see:
A. Brannigan, 'The reification of Mendel', *Social Studies of Science,* 9 (1979), pp. 423–54; T.M. Brown, 'From mechanism to vitalism in eighteenth-century English physiology', *Journal of the History of Biology,* 7 (1974), pp. 179–216. K.L. Caneva, 'From galvanism to electrodynamics: the transformation of German physics and its social context', *Historical Studies in the Physical Sciences,* 9 (1978), pp. 63–159; R.S. Cowan, 'Francis Galton's statistical ideas: the influence of eugenics', *Isis,* 63 (1972), pp. 509–28; A.J. Desmond, 'Designing the dinosaur: Richard Owen's response to Robert Edmond Grant', *Isis,* 70 (1979), pp. 224–34; J. Farley, *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore, 1977); J. Farley and G.L. Geison, 'Science, politics and spontaneous generation in nineteenth-century France: the Pasteur-Pouchet debate', *Bulletin of the History of Medicine,* 48 (1974), pp. 161–98; P. Forman, 'Weimar culture, causality, and quantum theory, 1918–1927: adaptation by German physicists and mathematicians to a hostile intellectual environment', *Historical Studies in the Physical Sciences,* 3 (1971), pp. 1–115; E. Frankel, 'Corpuscular optics and the wave theory of light: the science and politics of a revolution in physics', *Social Studies of Science,* 6 (1976), pp. 141–84; M.C. Jacob, *The Newtonians and the English Revolution, 1689–1720* (Ithaca, 1976); J.R. Jacob, 'Boyle's atomism and the Restoration assault on pagan naturalism', *Social Studies of Science,* 8 (1978), pp. 211–33; J.R. Jacob, *Robert Boyle and the English Revolution. A Study in Social and Intellectual Change* (New York, 1977); J.R. Jacob, 'The ideological origins of Robert Boyle's natural philosophy', *Journal of European Studies* 2 (1972), pp. 1–21; D. MacKenzie, 'Statistical theory and social interests: a case study', *Social Studies of Science,* 8 (1978), pp. 35–83; D. MacKenzie, 'Eugenics in Britain', *Social Studies of Science,* 6 (1976), pp. 499–532; D. MacKenzie, *Statistics in Britain 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh University Press, 1981); D. MacKenzie, S.B. Barnes, 'Biometriker versus Mendelianer: Eine Kontroverse und ihre Erklärung', *Kölner Zeitschrift für Soziologie,* special edition 18 (1975), pp. 165–96; D. Ospovat, 'Perfect adaptation and teleological explanation: approaches to the problem of the history of life in the mid-nineteenth century', *Studies in History of Biology,* 2 (1978), pp. 33–56; W. Provine, 'Geneticists and the biology of race crossing', *Science,* 182 (1973), pp. 790–6; M.J.S. Rudwick, 'The Devonian: a system born in conflict' in M.R. House *et al.* (eds), *The Devonian System* (London, 1979); S. Shapin, 'The politics of observation: cerebral anatomy and social interests in the Edinburgh phrenology disputes' in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge* (Sociological Review Monographs 27, Keele, 1979), pp. 139–78; R.S. Turner, 'The growth of professorial research in Prussia, 1818–1848: causes and contexts', *Historical Studies in the Physical Sciences,* 3 (1971), pp. 137–82; R.S. Turner, 'University reformers and professorial scholarship in Germany, 1760–1806' in L. Stone (ed.), *The University in Society* (Oxford, 1975), vol. II, pp. 495–531; Mary Winsor, *Starfish, Jellyfish and the Order of Life: Issues in Nineteenth-Century Science*

II

If the relativist places all beliefs on a par with one another for the purposes of explanation, then we can say that he is advocating a form of *monism*. He is stressing the essential identity of things that others would hold separate. Conversely, rationalists who reject relativism typically do so by insisting on a form of *dualism*. They hold on to the distinctions between true and false, rational and irrational belief and insist that these cases are vitally different from one another. They try to give the distinction a role in the conduct of the sociology of knowledge or anthropology or history, by saying that the explanations to be offered in the two cases are to be of a different kind. In particular, many of the critics of relativism implicitly reject our equivalence postulate by saying that rational beliefs must be explained wholly or partly by the fact that they *are* rational, whilst irrational beliefs call for no more than a causal, socio-psychological or 'external' explanation. For example, Hollis has recently insisted that 'true and rational beliefs need one sort of explanation, false and irrational beliefs another'.[10] Imre Lakatos was one of the most strident advocates of a structurally similar view. He equated rational procedures in science with those that accord with some preferred philosophy of science. Exhibiting the cases which appear to conform to the preferred philosophy is called 'internal history' or 'rational reconstruction'. He then asserts that 'the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery'. All the rest, which is not fully accounted for, is handed over to the sociologist for non-rational, causal explanation.[11] A version of this theory is endorsed by Laudan.[12] Even the sociologist

(New Haven, 1976); B. Wynne, 'C.G. Barkla and the J Phenomenon: a case study in the treatment of deviance in physics', *Social Studies of Science*, 6 (1976) pp. 304–47; B. Wynne, 'Physics and psychics; science, symbolic action and social control in late Victorian England' in B. Barnes and S. Shapin (eds), *Natural Order: Historical Studies of Scientific Culture* (Sage, London, 1979) ch. 7. A valuable review and discussion of this and other material in S. Shapin, 'History of science and its sociological reconstruction', *History of Science*, 20, Sep 1982 (forthcoming).

[8] M. Douglas, *Implicit Meanings* (Routledge, London, 1975); see also her 'Cultural Bias', Occasional Paper 34, Royal Anthropological Institute (London, 1978); cf. also M. Cole, J. Gay, J. Glick, D. Sharp, *The Cultural Context of Learning and Thinking* (Basic Books, New York, 1969) and R. Horton, and R. Finnegan (eds), *Modes of Thought* (Faber, London, 1973).

[9] See B. Barnes, and S. Shapin, (eds), *Natural Order: Historical Studies of Scientific Culture* (Sage, London, 1979); D. Bloor, 'The sociology of [scientific] knowledge' in W. Bynum, E.J. Browne and R. Porter (eds), *Dictionary of the History of Science* (Macmillan, London, 1981); S. Shapin, 'Social uses of science' in G.S. Rousseau and R.S. Porter (eds), *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-century Science* (Cambridge University Press, Cambridge, 1981) pp. 93–139.

[10] M. Hollis, 'The social destruction of reality', this volume p. 75.

[11] I. Lakatos, 'History of science and its rational reconstructions' in R. Buck and R. Cohen (eds), *Boston Studies in the Philosophy of Science*, vol. 8 (Reidel, Dordrecht, 1971) p. 106.

[12] L. Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (Routledge & Kegan Paul, London, 1977). For a critical review, see B. Barnes, 'Vicissitudes of belief', *Social Studies of Science*, 9 (1979), pp. 247–63.

Karl Mannheim adopted this dualist and rationalist view when he contrasted the 'existential determination of thought' by 'extra-theoretical factors' with development according to 'immanent laws' derived from the 'nature of things' of 'pure logical possibilities'. This is why he exempted the physical sciences and mathematics from his sociology of knowledge.[13]

As the first step in the examination of the rationalist case let us consider a charge that is sometimes made against the relativist. It is said, for example by Lukes, that the relativist has undermined his own right to use words like 'true' or 'false'.[14] Answering this charge is not a difficult task, and it will help to bring the character of relativism, and the shortcomings of rationalism, into sharp focus.

Consider the members of two tribes, T1 and T2, whose cultures are both primitive but otherwise very different from one another. Within each tribe some beliefs will be preferred to others and some reasons accepted as more cogent than others. Each tribe will have a vocabulary for expressing these preferences. Faced with a choice between the beliefs of his own tribe and those of the other, each individual would typically prefer those of his own culture. He would have available to him a number of locally acceptable standards to use in order to assess beliefs and justify his preferences.

What a relativist says about himself is just what he would say about the tribesman. The relativist, like everyone else, is under the necessity to sort out beliefs, accepting some and rejecting others. He will naturally have preferences and these will typically coincide with those of others in his locality. The words 'true' and 'false' provide the idiom in which those evaluations are expressed, and the words 'rational' and 'irrational' will have a similar function. When confronted with an alien culture he, too, will probably prefer his own familiar and accepted beliefs and his local culture will furnish norms and standards which can be used to justify such preferences if it becomes necessary to do so.

The crucial point is that a relativist accepts that his preferences and evaluations are as context-bound as those of the tribes T1 and T2. Similarly he accepts that none of the justifications of his preferences can be formulated in absolute or context-independent terms. In the last analysis, he acknowledges that his justifications will stop at some principle or alleged matter of fact that only has local credibility. The only alternative is that justifications will begin to run in a circle and assume what they were meant to justify.[15]

[13] K. Mannheim, *Ideology and Utopia* (Routledge & Kegan Paul, London, 1936), p. 239. For a reply to Mannheim, see D. Bloor, 'Wittgenstein and Mannheim on the sociology of mathematics', *Studies in History and Philosophy of Science*, 4 (1973), pp. 173–91.

[14] See, for instance, Lukes' critical reply to D. Bloor, 'Durkheim and Mauss revisited: classification and the sociology of knowledge' in *Studies in History and Philosophy of Science* (forthcoming).

[15] It may be objected that the present argument would only apply in a world where people were divided into relatively isolated social groups and would fail in proportion to the degree that cosmopolitan uniformity prevailed – or when what Durkheim called an 'international life' emerged. This is one of the objections E. Gellner presses against Winch in 'The new idealism – cause and meaning in the social sciences' in I. Lakatos and A. Musgrave (eds), *Problems in the Philosophy of Science* (North Holland, Amsterdam, 1968), pp. 377–406, esp. p. 397. In fact in our argument the picture of the isolated tribes T1 and T2 is merely expository and not a necessary feature of the argument. The size of the context and the actual presence of alternatives is entirely contingent. The same point would apply even if there happened to be just one, homogeneous, international community.

For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such. Because he thinks that there are no context-free or super-cultural norms of rationality he does not see rationally and
28 irrationally held beliefs as making up two distinct and qualitatively different classes of thing. They do not fall into two different natural kinds which make different sorts of appeal to the human mind, or stand in a different relationship to reality, or depend for their credibility on different patterns of social organization. Hence the relativist conclusion that they are to be explained in the same way.

III

A typical move at this point in the argument is to try to contain and limit the significance of the sociology of knowledge by declaring that because it is merely the study of *credibility* it can have no implications for *validity*. Validity, say the critics, is a question to be settled directly by appeal to evidence and reason and is quite separate from the contingencies of actual belief. As Professor Flew has put it, 'an account of the sufficiently good reasons' for a belief must be distinguished from 'an account of the psychological, physiological or sociological causes of inclinations to utter words expressing this belief when appropriately stimulated'.[16] The question of the reasons for a belief and the question of its causes are quite separate sorts of issue. But having separated these two issues this critic then proceeds to shunt the sociologist and psychologist into the sidings where they can be forgotten. The rationalist is now free to operate in the realm of reason and make out its function and workings to be whatever he wishes. This is why we are told so emphatically that the sociologist of knowledge 'must be concerned with causes of belief *rather than* with whatever evidencing reasons there may be for cherishing them'.[17]

Unfortunately for the rationalist the freedom which this convenient division of labour would give him cannot be granted: the distinctions upon which it is based will not stand examination. The reason is that it would be difficult to find a commodity more contingent and more socially variable than Flew's 'evidencing reasons'. What counts as an 'evidencing reason' for a belief in one context will be seen as evidence for quite a different conclusion in another context. For example, was
29 the fact that living matter appeared in Pouchet's laboratory preparations evidence for the spontaneous generation of life, or evidence of the incompetence of the experimenter, as Pasteur maintained? As historians of science have shown, different scientists drew different conclusions and took the evidence to point in different directions. This was possible because something is only evidence for something else when set in the context of assumptions which give it meaning – assumptions, for instance, about what is *a priori* probable or improbable. If, on religious and political grounds, there is a desire to maintain a sharp and symbolically useful distinction between matter and life, then Pouchet must have blundered rather than have made a fascinating discovery. These were indeed the factors that conditioned the

[16] A.G.N. Flew, 'Is the scientific enterprise self-refuting?', *Proceedings of the Eighth International Conference on the Unity of the Sciences, Los Angeles, 1979* (New York, 1980) vol. 1, pp.34–60.
[17] Ibid.

reception of his work in the conservative France of the Second Empire.[18] 'Evidencing reasons', then, are a prime target for sociological enquiry and explanation. There is no question of the sociology of knowledge being confined to causes *rather than* 'evidencing reasons'. Its concern is precisely with causes *as* 'evidencing reasons'.

IV

Obviously, it would be possible for the rationalist to counterattack. He could say that the above argument only applies to what are *taken* to be reasons, rather than to what *really are* reasons. Once again, the charge would be that the sociologists had conflated validity and credibility. But if a rationalist really were to insist on a total distinction between credibility and validity he would simply leave the field of discourse altogether. Validity totally detached from credibility is nothing. The sociologist of knowledge with his relativism and his monism would win by default: his theory would meet no opposition. It is because of the rationalist's desire to avoid this consequence that sooner or later, overtly or covertly, he will fuse validity and credibility. He too will treat validity and credibility as one thing by finding a certain class of reasons that are alleged to carry their own credibility with them: they will be visible because they glow by their own light.

To see how this comes about consider again the two tribes T1 and T2. For a member of T1 examining what is to him a peculiar belief
from the culture of T2, there is a clear point to the distinction between 30
the validity and the credibility of a belief. He will say that just because the misguided members of T2 believe something, that doesn't make it true. Its rightness and wrongness, he may add, must be established independently of belief. But, of course, what he will mean by 'independently of belief' is independently of the belief of others, such as the members of T2. For his own part, he has no option but to use the accepted methods and assumptions of his own group. In practice this is what 'directly' ascertaining truth or falsity comes down to.

The simple structure of the example makes it easy to see what is happening. The distinction between validity and credibility is sound enough in this case, but its real point, its scope and its focus, is entirely local. As the relativist would expect, it is not an absolute distinction, but one whose employment depends upon a taken-for-granted background. It is a move within a game, and it is with regard to the background knowledge, assumed by the move, that validity and credibility are tacitly brought together. Without this, the distinction itself could never be put to use, or its contrast be given an application.

If our imaginary tribesman was dialectically sophisticated he might realize that he is open to the charge of special pleading, and that he had, in his own case, collapsed the distinction upon which he had been insisting. How could he reply to the accusation that he had equated the validity and credibility of his own beliefs? As a more careful statement of his position he might claim that not even the fact that his own tribe believes something is, *in itself*, sufficient to make it true. But he would then have to mend the damage of this admission by adding that it just was a fact that what his tribe believed *was* true. A kindly providence, perhaps, had here united these two essentially different things.

[18] Farley and Geison, 'Science, politics and spontaneous generation'.

For the sociologist of knowledge these refinements change nothing. They do not remove the special pleading, they simply elaborate upon it. But they remind us that we need to locate the point at which the rationalists of our culture make the same move. We must examine the rationalist case to find the point at which reasons are said to become visible by their own light and Reason in Action transcends the operation of causal processes and social conditions.

V

A familiar candidate to invoke for the role of Reason in Action is the class of beliefs which are supposed to be directly and immediately
31 apprehended by experience. It may be said that some knowledge claims can be sustained, and can attract credibility purely in virtue of their correspondence with reality – a correspondence which any reasonable agent can recognize. Some things we just *know* by experience and no contingent factors, such as their support by authority or their coherence with the overall pattern of culture, are necessary for their maintenance.

Such a theory is easily recognized as a species of naîve empiricism and its weaknesses are well known. Nevertheless similar assumptions can emerge in a disguised form. For example Flew has argued that 'when it is a question of accounting for beliefs about matters of everyday use and observation, then there is nothing like so much room for sophisticated social and historical causes'.[19] The polemical force of this appeal to everyday use and observation may be gathered from the following claim which we shall assess in some detail:

> The cause of our belief that the ferry canoe is where it is on the Zaire River does not lie in the social structure of our tribe. It is to be found, instead, in certain intrusive non-social facts: that when we turn our eyes towards the right bit of the river the canoe causes appropriate sensory impressions; and that those heedlessly placing themselves in the water rather than the canoe are incontinently eaten by crocodiles.[20]

It would be possible to take exception to this passage by stressing how much more is involved in the identification of an object as 'the Zaire river ferry canoe' than turning our eyes in the right direction. All the socially sustained classifications that are involved in the process have been simply left out of account. But though such criticisms would be correct and well deserved, they would not fully meet the point being made. What is really at issue here is the status of certain skills such as our ability to navigate ourselves around our environment; avoiding falling in rivers; and remembering the location of medium-scale physical objects. The question is: how do these skills relate to a relativist sociology of knowledge? Do they provide any basis for criticism, and hence comfort for the rationalist?

The first point to notice is that the facts to be attended to are skills that individuals share with non-linguistic animals. They are a real and important part of our mentality and, as Flew has pointed out, they are not greatly illuminated by sociologists or historians. Indeed, they are

[19] Flew, 'Is the scientific enterprise self-refuting?'
[20] Ibid.

taken for granted by these disciplines. This is because they belong to the province of the biologist and the learning theorist. It is no surprise that different aspects of knowledge are divided out amongst different scientific disciplines. But it is surely no skin off the sociologist's nose that he cannot explain how a dog retrieves its buried bone.

The important point is that none of the work in cognitive psychology which might explain this order of fact is going to be sufficient to account for the problems addressed by the sociologist. These concern variations in institutionalized patterns of knowledge. The difference between knowledge as it concerns the sociologist and the kind of knowing used in the objection may be represented by an analogy: it is the difference between a *map* and an individual organism's working knowledge of a terrain. There is a qualitative difference between these two things: one is a collective, the other an individual, representation. A map is an impersonal document, not a state of mind; it is a cultural product which requires conventions of representation. (And, of course, there are an indefinitely large number of different conventions which may be agreed upon.) Information about the psychological capacities which permit individual navigation won't add up to competent answers to questions, say, about the creation, maintenance, and change of cartographic norms.

That features of animal navigation should be seen as *criticism* of the sociology of knowledge simply reveals an individualistic bias in the way that the word 'knowledge' is being construed. As an objection it trades on a muddle between social and individual accomplishments. Furthermore, the kinds of individual cognitive skills that are in question are increasingly coming under the scope of the *causal* theories produced by physiologists and psychologists. They are showing themselves to be amenable to precisely the type of explanation that, as a good rationalist, Flew was at pains to *contrast* with the operation of reason. This hardly makes them fit candidates for the role of Reason in Action which was the use to which our rationalist critic was putting them. Some of the facts about everyday use and observation may indeed provide little room for sophisticated sociological or historical explanation, but that is because they provide room for sophisticated psychological explanations. While these can happily *co-exist* with the sociology of knowledge, they directly *contradict* the claims of the rationalist critics of that discipline.[21]

[21] Of course it would be possible for the same argument, with the same monist and dualist alternatives, to be repeated on the level of psychological explanation. It has been known for philosophers to insist that, in psychology, causal accounts are only appropriate for pathological phenomena, e.g. saying that, while error and illusion might be causally explicable, normal or correct perception is not a fit subject for empirical investigation and causal explanation. See D. Hamlyn, *The Psychology of Perception* (Routledge & Kegan Paul, London, 1969), ch. 2, pp. 11–13; and G. Ryle *The Concept of Mind* (Hutchinson, London, 1949), p. 326.

In contrast, for fruitful and fascinating attempts to give good causal explanations of both successful and erroneous perception, see R.L. Gregory, *Eye and Brain* (Weidenfeld & Nicolson, London, 1966). Perhaps one day the dualist account of Ryle and Hamlyn will be developed into its ultimate form, and we will be told that the operations of adding machines are causally determined only when erroneous results are produced, and that at other times such machines operate rationally in ways which require no explanation.

33 There is no need for a relativist sociology of knowledge to take anything other than a completely open and matter-of-fact stance towards the role of sensory stimulation. The same applies to any other of the physical, genetic or psychological and non-social causes that must eventually find a place in an overall account of knowledge. The stimulation caused by material objects when the eye is turned in a given direction is indeed a causal factor in knowledge and its role is to be understood by seeing how this cause interacts with other causes. There is no question of denying the effect on belief of the facts – that is, of the segment of *unverbalized reality* that is the focus of the beliefs in question. All that need be insisted upon is that when due allowance is made for the effect of 'the facts' it is made in accordance with the equivalence postulate. This means that the effect of 'the facts' on a believer plays the same general role whether the belief that results is a true one or a false one.

To show what is meant by this let us look at a simple, real-life case where, it may be considered, reality impinges in the same causal way on those who held true and false beliefs about it. Consider the eighteenth-century chemists Priestley and Lavoisier who gave diverging accounts of what happens during combustion and calcination. For simplicity we may say that Priestley's phlogiston theory was false and Lavoisier's oxygen theory was true. Both Priestley and Lavoisier were looking at samples of (what we would call) lead oxide and mercuric oxide. They both arranged pieces of apparatus so that they could heat
34 these substances. They then observed what happened, and recorded the behaviour of various volumes of gas given off and absorbed.

Nevertheless Priestley and Lavoisier believed totally different things: they gave sharply conflicting accounts of the nature of the substances they observed and their properties and behaviour. Indeed they asserted that quite different substances were present in the events they witnessed. Lavoisier denied that there was such a substance as phlogiston and postulated the existence of something called 'oxygen'. Priestley took exactly the opposite view. He insisted on the existence of phlogiston, identifying it with certain samples of gas agreed by both to be present in the experiment. Furthermore Priestley denied Lavoisier's 'oxygen' and characterized the gas so labelled – which he had himself discovered – by means of his own theory.[22] Clearly the effect of 'the facts' is neither simple nor sufficient to explain what needs explaining, viz. the theoretical divergence. It is because the effect of 'the facts' is so different that the sociology of knowledge has a task.

There were, indeed, some occasions when for a while the experimenters observed different things from one another, e.g. when one came across a phenomenon that the other had not yet heard about. Furthermore, it is clear that when either of them observed something new in their apparatus it evoked a response. Thus Priestley spotted the appearance of water when, as we would say, he heated lead oxide in

[22] J.B. Conant, 'The overthrow of phlogiston theory', in J.B. Conant and K.K. Nash (eds), *Harvard Case Histories in Experimental Science*, vol. I (Harvard University Press, Cambridge, Mass., 1966). Lavoisier's 'oxygen' gas was conceived by him to be the principle of acidity plus caloric – the heat fluid. Caloric has now gone the same way as phlogiston and has been rejected as a theoretical entity, and it was later discovered that Lavoisier's 'principle of acidity' was not present in hydrochloric acid.

hydrogen. (For Priestley this was 'minium' in 'phlogiston'.) But what the new observation did was to prompt the elaboration of his existing approach. Similarly, the *differential* exposure to facts merely resulted, for a while, in a slightly different degree of elaboration of their respective systems of thought.

The general conclusion is that reality is, after all, a common factor in all the vastly different cognitive responses that men produce to it. Being a common factor it is not a promising candidate to field as an explanation of that variation. Certainly any differences in the sampling of experience, and any differential exposure to reality must be allowed
for. But that is in perfect accord with our equivalence postulate which 35
enjoins the sociologist to investigate whatever local causes of credibility operate in each case. There is nothing in any of this to give comfort to the rationalist, or trouble to the relativist.

VII

Another important line of attack directed against relativism appears in a well-known sequence of papers by Martin Hollis and Steven Lukes.[23] They hold that all cultures share a common core of true beliefs and rationally-justified patterns of inference. This core is made up of statements which rational men 'cannot fail to believe in simple perceptual situations' and 'rules of coherent judgement, which rational men cannot fail to subscribe to'.[24] Elsewhere these cultural universals are described as 'material object perception beliefs' and 'simple inferences, relying, say, on the law of non-contradiction'.[25] According to Hollis and Lukes the truth of the statements in this core, and the validity of the inferences therein, are everywhere acknowledged because there are universal, context-independent criteria of truth and rationality, which all men recognize and are disposed to conform to. Without such universal criteria there would be no common core.

Clearly if there is indeed such a core, and it is sustained by context-independent criteria of truth and rationality, then relativism is confounded. But why must we accept that it exists? Interestingly, Lukes
and Hollis make no serious attempt to describe the common core, or to 36
mark its boundaries. Rather, they seek to show that it *must* exist, or at least that its existence must be assumed *a priori* if the possibility of communication and understanding between distinct cultures is admitted. We are asked to consider the problems facing say, an English-speaking anthropological field-worker, seeking to understand an alien culture. Such an individual must grasp the meaning of the alien

[23] M. Hollis, 'The limits of irrationality', *European Journal of Sociology*, 7 (1967), pp. 265–71; and 'Reason and ritual', *Philosophy*, 43 (1967), pp. 231–47; and also this volume. See also S. Lukes, 'Some problems about rationality', *European Journal of Sociology*, 7 (1967), pp. 247–64; 'On the social determination of truth' in Horton and Finnegan, *Modes of Thought;* 'Relativism, cognitive and moral', *Supplementary Proceedings of the Aristotelian Society*, 68 (1974), pp. 165–89; 'Rationality and the explanation of belief', paper given at the Colloquium on 'Irrationality: Explanation and Understanding', Maison des Sciences de l'Homme, Paris, 7–9 Jan 1980.
Note: Hollis', 'The limits of rationality' and 'Reason and ritual' and Lukes' 'Some problems about rationality' have all been reprinted in B.R. Wilson (ed.), *Rationality* (Blackwell, 1970), chs. 9, 10 and 11. Page references will be to the Wilson volume, unless otherwise stated.

[24] Hollis, this volume, p. 74.

[25] Lukes, 'Rationality and the explanation of belief', p. 8.

concepts and beliefs, and this, we are told, requires him to *translate* them into English. This is where the common core comes in: it serves as the 'rational bridgehead' which makes translation possible. It is the basis upon which simple equivalences between two languages can be initially established so that the enterprise of translation can get off the ground. By assuming that in 'simple perceptual situations' the aliens perceive much as we do, infer much as we do, and say more or less what we would say, we can 'define standard meanings for native terms'. This then 'makes it possible to identify utterances used in more ambiguous situations', lying outside the bridgehead, in which 'supernatural' or 'metaphysical' or 'ritual' beliefs are expressed.[26] The basic point, however, is that without the rational bridgehead we would be caught in a circle. We need to translate 'native' utterances in order to know what beliefs they express, while at the same time we need to know what is believed in order to know what is being said. Without an assumed bridgehead of shared beliefs there 'would be no way into the circle', for there is, says Hollis, 'no more direct attack on meaning available'.[27]

Stated in abstract terms this argument has a certain plausibility. And if it were to prove correct it would certainly bolster the rationalist case and run counter to our equivalence postulate. The beliefs belonging to the rational bridgehead will be those whose enduring presence is explicable simply in virtue of the untrammelled operation of universal reason. Their credibility will be of a different sort from the diversity of beliefs that are peculiar to different cultures. The credibility of this latter class will have to be explained by special local causes, whilst the former 'simply are rational'.[28] The fact is, however, that the bridghead argument fails as soon as it is measured against the realities of language learning and anthropological practice.

37 Notice how the whole argument hinges on the supposed role of translation: there is 'no more direct attack on meaning available'. But the fact is that translation is *not* the most direct attack on meaning that is available. It was not available, nor did it play any part at all, in the first and major attack that any of us made upon meaning when we acquired language in childhood. First language acquisition is not a translation process, and nothing that is absent here can be a necessary ingredient in subsequent learning. To understand an alien culture the anthropologist can proceed in the way that native speakers do. Any difficulties in achieving this stance will be pragmatic rather than *a priori*. There is, for instance, no necessity for the learner to assume shared concepts. Such an assumption would be false and would have nothing but nuisance value.

To see why this is so consider what is involved when a child learns an elementary concept like 'bird'. Such learning needs the continuing assistance of culturally competent adults. A teacher may gesture towards something in the sky and say 'bird'. Given the well-known indefiniteness of ostension a child would probably glean very little information from this: is it the object or the setting that is intended? But after a few acts of ostension, to different birds in different settings, he would begin to become competent in distinguishing 'birds' from 'non-birds', and might perhaps himself tentatively point out and label putative birds.

[26] Hollis, 'The limits of irrationality', pp. 215, 216, and 'Reason and ritual', p. 221.
[27] Hollis, 'The limits of irrationality', p. 208.
[28] Lukes, 'Some problems about rationality', p. 208.

Suppose now that the child labels a passing aeroplane a 'bird'. This would be a perfectly reasonably thing to do given the points of resemblance between aeroplanes and birds. Of course, there are noticeable differences too, but there are such differences between every successive instance of what are *properly* called 'birds'. All the instances of empirical concepts differ in detail from one another and we can never apply such concepts on the basis of perfect identity rather than resemblance. What the child is doing, in effect, is judging the resemblances between the aeroplane and the previous instances of 'bird' to be more significant than the differences. The general form of his judgement, with its balancing of similarities and differences, is identical to those which lead to proper or accepted usage. It is only his knowledge of custom which is defective.

What happens in the case of the child is that he is overruled. 'No, that is an aeroplane.' This correction is at once an act of social control and of cultural transmission. It helps him to learn which of the possible judgements of sameness are accepted by his society as relevant to the
use of 'bird'. In this way the particulars of experience are ordered into 38
clusters and patterns *specific to a culture*.

The significance of this point becomes even more clear when we see how the things we call 'birds' are dealt with in other cultures. When the anthropologist Bulmer visited the Karam of New Guinea he found that many of the instances of what we would call 'bird' were referred to as 'yakt'. He also found that instances of bats were included amongst the 'yakt', while instances of cassowaries were scrupulously denied admittance to the taxon. Objects were clustered in different ways, and the analogies that it is possible to discern amongst phenomena were channelled along different paths. Nevertheless, it was not too difficult to learn 'yakt': the task simply involved noting what the Karam pointed out as 'yakt' until it was possible to pick them out as well as the Karam did.[29]

What these examples show is that even empirical terms like 'bird' do not constitute a special core of concepts whose application depends only upon an unconditioned reason. Learning even the most elementary of terms is a slow process that involves the acquisition from the culture of specific *conventions*. This makes apparently simple empirical words no different from others that are perhaps more obviously culturally influenced. There are no privileged occasions for the use of terms – no 'simple perceptual situations' – which provide the researcher with 'standard meanings' uncomplicated by cultural variables. In short, there is no bridgehead in Hollis' sense.[30]

Because there are no 'standard meanings' there is no question of using them to provide a secure base from which to advance towards more ambiguous cases whose operation is to be understood in a qualitatively different and derivative way. All concepts and all usages

[29] R. Bulmer, 'Why is the cassowary not a bird?', *Man*, n.s., 2 (1967), pp. 5–25.

[30] For a fuller development of these points, see M. Hesse, *The Structure of Scientific Inference* (Macmillan, London, 1974), chs 1 and 2. Their sociological significance is explored in B. Barnes, 'On the conventional character of knowledge and cognition', *Philosophy of the Social Sciences*, 11 (1981), pp. 303–33. D. Bloor, 'Durkheim and Mauss revisited: classification and the sociology of knowledge', *Studies in History and Philosophy of Science* (forthcoming) – a German version of this paper appeared in *Kölner Zeitschrift für Soziologie*, special issue, 23 (1980), pp. 20–51.

stand on a par: none are intrinsically 'unambiguous' or intrinsically 'ambiguous' any more than some are intrinsically 'literal' or intrinsically 'metaphorical'. Furthermore, there is no telling in advance
39 which are the 'problematic' cases where an alien culture will deviate from ours. For example, Bulmer could not have predicted in advance what the Karam would call bats or cassowaries, simply because of the initial identity of usage he discerned between the Karam 'yakt' and our 'bird'. Similarly, no one could predict on the basis of past usage what the Karam would do with a hitherto unknown case such as, say, a barn-owl. Existing usage is only a precedent defined over a finite number of particular instances. It does not fix the proper handling of new cases in advance. Diverse developments are possible, and even where cultural diversity is not present it could emerge at any moment by a revision of the existing sequence of judgements of sameness and difference.

It might be objected, none the less, that the 'rational bridgehead' was invoked to account for the possibility of translation, and translation is a *possible* mode of understanding alien culture, even if it is not a *necessary* mode. How is translation possible? Might not an anti-relativist argument be based simply upon the possibility of successful translation?

The way to proceed here is to assume nothing about translation in advance, least of all that it is successfully carried out. Instead we should ask what is implied for translation by the little empirical knowledge we possess of the simpler aspects of semantics and language learning. One clear implication arises from the character of concepts as arrays of judgements of sameness. Every such array, being the product of a unique sequence of judgements, is itself unique. No array in one culture can be unproblematically set into an identity with an array from another culture. Hence perfect translation cannot exist: there can only be translation acceptable for practical purposes, as judged by contingent, local standards. And this is a conclusion which fits well with what we know of the extremely complex procedures and activities which constitute translation as an empirical phenomenon.

Thus the rational bridgehead, the alleged common core of belief shared by all cultures, turns out to be a purely imaginary construct with no empirical basis at all. It is not difficult, however, to perceive its origins in the received culture of epistemologists. It is an old philosophical dualism dressed in a new garb. The distinction between the parts of a culture that belong to the rational core, and the parts that are specific and variable is just another version of the idea that observational predicates are qualitatively different from theoretical predicates. The bridgehead argument is a plea for a single pure
40 observation language. Of all the dualisms of epistemology this must be the most discredited.[31] Surely, we now all recognize that although we may well all share the same unverbalized environment, there are any number of equally reasonable ways of speaking of it.

VIII

Hollis' and Lukes' argument includes the claim that there are simple forms of inference which all rational men find compelling. Among the instances offered here is 'p, p implies q, therefore q'. Hollis introduces this under the logicians' name of *modus ponens* and represents it by

[31] Cf. Hesse, *The Structure of Scientific Inference*.

using the usual symbol for material implication, $(p.\,(p \rightarrow q)) \rightarrow q$.[32] It is noticeable, however, that Hollis and Lukes do not even begin to make their case. In particular they offer no relevant empirical evidence for their claim. None the less it is interesting to explore what follows if men do indeed evince some general disposition to conform to *modus ponens* and to other simple patterns of inference. It then becomes necessary to ask *why* men are disposed in favour of these forms of inference. What might account for the existence of such alleged universals of reason?

According to the rationalists there are two distinct issues here, and two ways of approaching the question. We can either search for the causes of the phenomenon, or we can seek to furnish reasons for it. Naturally a rationalist will want to provide the sufficiently good reasons that are at work, and hence show that deductive intuitions are explicable in rational terms. The aim will be to show that deductive forms of inference can be shown to be rationally justified in an absolute and context-free sense. Unfortunately for the rationalist there is little that he can offer by way of reasoned argument in favour of adherence to deductive inference forms. We have reached the end-point at which justification goes in a circle.

The predicament is neatly captured in Lewis Carroll's story of what the Tortoise said to Achilles. Presented by Achilles with premises of the form '$p \rightarrow q$' and 'p' the tortoise refuses to draw the conclusion 'q' until the step has been justified. Achilles obliges by formulating the rule according to which the tortoise is to proceed. The rule makes clear the grounds upon which the step to 'q' may be taken. Given the rule 'when you have "$p \rightarrow q$" and "p" conclude "q" ' and given both '$p \rightarrow q$' and 'p', will you *now* conclude 'q'? he asks. Unfortunately the tortoise is able to point out that when the justifying premise has been added the new inference is again dependent on a step of the type that has been called into question: so he asks for yet another premise to be formulated, and so on. The attempt at justification therefore fails, and Achilles finds that he cannot use logic to force the tortoise to draw the desired conclusion.[33]

The basic point is that justifications of deduction themselves presuppose deduction. They are circular because they appeal to the very principles of inference that are in question.[34] In this respect the justification of deduction is in the same predicament as the justifications of induction which tacitly make inductive moves by appealing to

[32] Hollis, 'Reason and ritual', p. 232.

[33] L. Carroll, 'What the Tortoise said to Achilles', *Mind*, n.s., 4 (1895), pp. 278–80. Carroll, of course, does not use bare *p*s and *q*s but begins with a simple proposition from Euclid.

[34] It has been argued that there are technical defects in Carroll's paper and that the tortoise shifts his ground with regard to what has to be justified at different stages in the regress. See J. Thomson, 'What Achilles should have said to the Tortoise', *Ratio*, 3 (1960), pp. 95–105. Nevertheless the basic thrust of Carroll's argument is correct. Circularity emerges whenever an attempt is made to ground our most general notions of validity. See W. Quine, 'Truth by convention' in his *Ways of Paradox* (Random House, New York, 1966). See also J. McKinsey, *Journal of Symbolic Logic*, 13 (1948) pp. 114–15; and S. Kleene, ibid., pp. 173–4. These points are fully discussed in Susan Haack, 'The justification of deduction', *Mind*, 85 (1976), pp. 112–19. In particular Haack shows that appeals to the truth table definition of material implication in order to justify *modus ponens* itself uses that principle (p. 114).

the fact that induction 'works'. Our two basic modes of reasoning are in an equally hopeless state with regard to their rational justification.[35]

42 As with induction a variety of attempts have been made to evade the circularity of justification.[36] Perhaps the most fully developed attempt at justification has been to say that the validity of inferences derives simply from the meaning of the formal signs or logical words used in them. For instance, the meaning of '→' is given by 'truth table' definitions or the rules of inference of the logical system of which it is a part, and the validity of the inferences in this system derives from these meanings. This is the theory of 'analytic validity'. Unfortunately for the rationalist this theory has been completely devastated by the logician A.N. Prior.[37]

Prior develops his argument by taking the case of the very simple logical connective 'and'. Why is '*p* and *q*, therefore *q*' a valid inference? The theory of analytic validity says that it is valid because of the meaning of 'and'. What is the meaning of 'and'? This is given by stating the role that the term has in forming compound propositions, or conjunctions, and drawing inferences from them. 'And' is defined by the rules that (i) from any pair of statements '*p*' and *q*', we can infer the statement '*p* and *q*', and (ii) from any conjunctive statement '*p* and *q*', we can infer either of the conjuncts. As an antidote to the seductive power of this circular procedure Prior shows that a similar sequence of definitions would permit the introduction of connectives that would justify the inference of any statement from any other. Consider, he says, the new logical connective 'tonk':

> Its meaning is completely given by the rules that (i) from any
> 43 statement P we can infer any statement formed by joining P to any statement Q by 'tonk' (which compound statement we hereafter describe as 'the statement P-tonk-Q'), and that (ii) from any 'contonktive' statement P-tonk-Q we can infer the contained statement Q.[38]

Hence we can infer any *Q* from any *P*.

[35] Haack, in 'The justification of deduction' exhibits the similarity between the scandal of induction and the scandal of deduction. Needless to say, an *inductive* justification of deductive inference-forms and contradiction-avoiding rules is useless in the battle against relativism. If these rules and forms are favoured and institutionalized only where they prove profitable in discourse, then their incidence becomes intelligible in terms of contingent local determinants just as sociological relativism requires. And, conversely, all the deviations from the rules and forms are likely to become *equally* justified in the same way. This will show them to be just the same kinds of phenomena as the rules and forms themselves. Consider all the familiar locutions we find of pragmatic value in informal speech which appear to do violence to formal logical rules: 'Yes and no', 'It was, and yet it wasn't', 'The whole was greater than its parts', 'There is some truth in that statement', 'That statement is nearer to the truth than this one', '*A* is a better proof than *B*', and so on. All these locutions, indeed everything in discourse which Lukes identifies as needing elucidation by 'context-specific' rather than 'universal' rules, become identical in character to 'universally-rational' forms of discourse. The dualism essential to the anti-relativist position disappears.

[36] For instance, it may be said that justifications of deduction are 'superfluous' and that it is a mistake to concede that they are necessary. Critics of Lewis Carroll have taken this line, e.g. W. Rees, 'What Achilles said to the Tortoise', *Mind*, n.s., 60 (1951), pp. 241–6. But judgements about what is, or is not, superfluous are highly subjective. In the present type of case we may suspect that they will derive their credibility entirely from their convenience for the purposes in hand, namely evading the problem that justifications are circular.

[37] A.N. Prior, 'The runabout inference ticket', *Analysis*, 21 (1960), pp. 38–9.

[38] Ibid., p. 38.

What Prior's paper shows is that appeal to rules and meanings cannot by itself justify our intuitions about validity, because these rules and meanings are themselves judged according to those intuitions, e.g. intuitions to the effect that 'and' is defined by acceptable rules, whereas 'tonk' is not. The theory of analytic validity invites us to run to meanings to justify our intuitions of validity, but then we have to run back again to our intuitions of validity to justify our selection of meanings. Our preference for the 'right' rules which define 'acceptable' connectives reveals the circularity of the intended justification. The intuitions are basic and the problem of justification set by the tortoise is the end-point after all. Like the good relativist that he is, the tortoise awaits a reasoned justification of deduction, confident that none will be forthcoming.[39]

IX

What else is there to do then but to turn to causes for an answer to the question of the widespread acceptance of deductive inference forms and the avoidance of inconsistency? A plausible strategy is to adopt a form of nativism: the disposition arises from our biological constitution and the way the brain is organized. Such a move, needless to say, gives no comfort to rationalism: epistemologically, to invoke neuronal structure is no better than to invoke social structure; both moves seek explanations rather than justifications. And for this very reason nativism is perfectly compatible with relativism. At whatever point it is found necessary, the explanation of credibility may swing from social to biological causes. Our empirical curiosity swings from asking how our society is organized to asking how the brain is organized. Our general cognitive proclivities become subject to empirical enquiry just as are the cognitive proclivities of other species. The empirical scientific investigation of human cognition, its manifest structure and its physiological basis, is, of course, a lengthy task. At any given time our overall understanding of the matter, and in particular our verbal accounts of it, will be provisional and liable to change. They are subject to the same fluctuations and redescriptions as are found in the study of any other empirical phenomenon.

This consideration reinforces an important point: no account of our biologically-based reasoning propensities will justify a unique system of logical conventions. Just as our experience of a shared material world does not itself guarantee shared verbal descriptions of it, so our shared natural rationality does not guarantee a unique logical system.

[39] Prior's paper has been discussed by N. Belnap, 'Tonk, Plonk and Plink', *Analysis*, 22 (1962), pp. 130–4; J.T. Stevenson, 'Roundabout the runabout inference ticket', *Analysis*, 21 (1962), pp. 124–8; and Susan Haack, *Philosophy of Logics* (Cambridge University Press, Cambridge, 1978), p. 31.

None of these commentators address the main point in Prior's argument. They all treat his paper as if it posed the question of how we should define logical connectives, rather than the question of the source of the validity of the inferences containing them. Thus their response takes the form of saying that *properly* chosen meanings accomplish valid inferences. The point, then, is to locate the source of propriety for these choices, and this reintroduces our intuitions of validity again. Belnap explicitly invokes these intuitions but, despite his appreciation of Prior's paper, appears not to see that he is supporting rather than correcting him.

Hollis' paper 'A Retort to the Tortoise', *Mind*, 84 (1975), pp. 610–16, is simply another attempt to reify 'meaning' and impute to it the power to solve basic problems of validity.

Hollis and Lukes make the same mistake in dealing with logic as they do with descriptive predicates. They fail to keep what belongs to unverbalized reality separate from what belongs to language. Just as they conflated the two with their doctrine of a universal observation language; now they take the plausible belief that we possess deductive dispositions and render it, without a second thought, into the abstract and highly conventionalized notion of material implication. To combat this confusion we need to remember the gap between the varied systems of logic as they are developed by logicians and the primitive, biologically based, informal intuitions upon which they all depend for their operation. Hollis and Lukes' conflation soon takes its revenge on them: *modus ponens* for material implication, which they confidently take to be a rational universal, has been explicitly deemed to fail, and is rejected, in some interesting systems of logic.[40]

45 Logic, as it is systematized in textbooks, monographs or research papers, is a learned body of scholarly lore, growing and varying over time. It is a mass of conventional routines, decisions, expedient restrictions, dicta, maxims, and *ad hoc* rules. The sheer *lack* of necessity in granting its assumptions or adopting its strange and elaborate definitions is the point that should strike any candid observer. Why should anyone adopt a notion of 'implication' whereby a contradiction 'implies' any proposition? What is compelling about systems of logic which require massive and systematic deviation from our everyday use of crucial words like 'if', 'then', and 'and'?[41] As a body of conventions and esoteric traditions the compelling character of logic, such as it is, derives from certain narrowly defined purposes and from custom and institutionalized usage. Its authority is moral and social, and as such it is admirable material for sociological investigation and explanation. In particular the credibility of logical conventions, just like the everyday practices which deviate from them, will be of an entirely local character. The utility of granting or modifying a definition for the sake of formal symmetry; the expediency of ignoring the complexity of everyday discourse and everyday standards of reasoning so that a certain abstract generality can be achieved: these
will be the kinds of justification that will be offered and accepted or 46
disputed by specialists in the field.

[40] A.R. Anderson, and N. Belnap, 'Tautological entailments', *Philosophical Studies*, 13 (1961), pp. 9–24. This paper develops some of the consequences of demanding that the entailment relation satisfy conditions of 'relevance' between premise and conclusion. This is one of the plausible intuitive requirements that are violated by the 'Lewis Principle', that a contradiction entails any statement: 'P and not P entails Q', regardless of whether or not Q 'has anything to do with' P or its negation. The Lewis Principle is a theorem of the axiom system sometimes called the system of tautological implication. To challenge the theorem means rejecting, for instance, the disjunctive syllogism which is equivalent to *modus ponens*. This is what Anderson and Belnap do on grounds of relevance. The result is a perfectly consistent axiom system: the four-valued logic of De Morgan implication. See Haack, *Philosophy of Logics*, pp. 200–1; D. Makinson, *Topics in Modern Logic* (Methuen, London, 1973), ch. 2.

It is ironic that logicians, who expose with admirable ruthlessness how problematic, variable and difficult to ground patterns of inference are, and who freely confess how very little is agreed upon by the totality of practitioners in their field, are turned to again and again to provide constraints upon the possibilities of rational thought. Just as there is always a certain demand for iron laws of economics, so there seems always to be a demand for iron laws of logic.

[41] For a careful documentation of the relation between ordinary and technical usage, see P. Strawson, *Introduction to Logical Theory* (Methuen, London, 1952).

The point that emerges is that if any informal, intuitive reasoning dispositions are universally compelling, they are *ipso facto* without any reasoned justification. On the other hand, any parts of logic which can be justified will not be universal but purely local in their credibility. The rationalist goal of producing pieces of knowledge that are both universal in their credibility *and* justified in context-independent terms is unattainable.[42]

There is, of course, a final move that the rationalist can make. He can fall back into dogmatism, saying of some selected inference or conclusion or procedure: this just *is* what it is to be rational, or, this just *is* a valid inference.[43] It is at this point that the rationalist finally plucks victory out of defeat, for while the relativist can fight Reason, he is helpless against Faith. Just as Faith protects the Holy Trinity, or the Azande oracle, or the ancestral spirits of the Luba, so it can protect Reason. Faith has always been the traditional and most effective defence against relativism. But if at this point the relativist must retire defeated, to gaze from some far hilltop on the celebratory rites of the Cult of Rationalism, he can nevertheless quietly ask himself: what local, contingent causes might account for the remarkable intensity of the Faith in Reason peculiar to the Cult?[44]

[42] For an extension of this argument from logic to mathematics, see D. Bloor, *Knowledge and Social Imagery* (Routledge & Kegan Paul, 1966). chs 5–7. Here a modified empiricist theory of mathematics is defended against Frege. See also Bloor, 'Polyhedra and the abominations of Leviticus', *British Journal for the History of Science*, 11 (1978), pp. 245–72. This provides a sociological reading of I. Lakatos, *Proofs and Refutations* (Cambridge University Press, Cambridge, 1976).

[43] Although dogmatic assertions are perhaps the most common ways in which philosophers indicate the end-points at which they revert to faith, there are other ways. Quine for example explicitly takes for granted currently accepted scientific knowledge. Similarly, logicians often set out the points where they allow 'intuition' to decide for them which of different arguments they will accept.

Equally widespread, if less defensible, is the decision to reject an argument because of its consequences. If a series of inferences leads to solipsism, or scepticism, or relativism, it is assumed, simply by that very fact, that the series must contain an error. Thus, H. Putnam describes how one of his papers is designed to 'block' a perfectly good, but inconvenient, inductive inference: '[There is] a serious worry . . . that eventually the following meta-induction becomes overwhelmingly compelling: *just as no term used in the science of more than 50 . . . years ago referred, so it will turn out that no term used now . . . refers*.

It must obviously be a desideratum for the Theory of Reference that this meta-induction be blocked . . . ('What is realism?, *Philosophical Papers* (Cambridge University Press, Cambridge, 1975), vol.2).

Finally, of course, there is the occasional quite straightforward profession of faith, which scorns any disguise. I.C. Jarvie, for example, is disarmingly frank when he opposes relativism by suggesting: 'Perhaps, when we do science, and even more so mathematics, we participate in the divine . . . [It is] awe at the transcendental miracle of mathematics and science that has moved philosophers since Ancient Greece' ('Laudan's problematic progress and the social sciences', *Philosophy of the Social Sciences*, 9 (1979), p. 496).

[44] A plausible hypothesis is that relativism is disliked because so many academics see it as a dampener on their moralizing. A dualist idiom, with its demarcations, contrasts, rankings and evaluations is easily adapted to the tasks of political propaganda or self-congratulatory polemic. *This* is the enterprise that relativists threaten, not science. See notes 1–4 above. If relativism has any appeal at all, it will be to those who wish to engage in that eccentric activity called 'disinterested research'.

Martin Hollis and Steven Lukes, *Rationality and relativism*, Basil Blackwell, Oxford, 1982, pp. 21-47.

Michael Zenzen
and Sal Restivo

1.8

THE MYSTERIOUS MORPHOLOGY OF IMMISCIBLE LIQUIDS: A STUDY OF SCIENTIFIC PRACTICE

Introduction

Until recently, the nature of scientific work has been told at second and third hand by historians, philosophers, and sociologists of science, discussed in journalistic and anecdotal essays and biographies, and recalled in the memoires of scientists. The movement away from armchair and anecdotal reconstructions of scientific work began with critiques of idealistic and heroic histories of science, positivist philosophies of science, and normative-functionalist sociologies of science. More recently, the movement from critiques to new modes of research and theory in science studies has spawned an ethnographic approach to the study of scientific practice. Sociologists of science have begun to focus on scientific work as an "on-line", "real-time" process. Our research was undertaken in this ethnographic spirit.[1]

Objectives and methods

Our objectives are, first, to describe "what happened" in the laboratory we studied between the time a project was initiated and the culmination of the project in a published paper; second, to discuss the differences in style and rhetoric among the three major documents associated with the project — the report to the funding agency, the manuscript originally submitted for publication, and the manuscript accepted for publication; and third, to discuss the role of "contingencies" in problem formulation, dealing with difficulties and anomalies, constructing theories, and publishing results. We argue that contingencies (social and otherwise) do not simply affect the course of scientific work (i.e., they are not simply "externalities") but rather that they are an integral part of that work.[2]

We do not claim that our account of work in the laboratory is superior to or more "factual" than other accounts that might be given by other observers (including accounts by the laboratory scientists themselves). Our aim is to give *an* account of scientific practice based on a variety of standard ethnographic methods — in-depth interviews; observations; the analysis of notes and conversations, draft manuscripts, and published papers; and direct par-

This is a revised version of a paper presented at the McGill University Conference on "The Social Process of Scientific Investigation", Montreal, 9-21 October 1979. We are grateful for the criticisms and suggestions of Roger Krohn, Roger Hahn, Karin Knorr, Steve Woolgar, Doug McKegney, and Michael Lynch. We are especially indebted to Sydney Ross and Ralph Kronbrekke for allowing us to come into their laboratory, and for their and Henry Hollinger's unfailing cooperation at all stages of our research.

ticipation in laboratory work. Our account is thus one of many alternative accounts that are possible. This is a consequence of what we call the "Rashomon theorem": there are numerous, if not an infinity of, ways of describing and interpreting any given phenomenon; the status of any given account is not determined *a priori* by whether it is "true" or "real" in some absolute sense, but by how useful it is in the competitive realm of knowledge production and utilization.[3]

Our study spans a period of approximately two years (1979-1980). During the first year, we — singly or together — were in the laboratory two to four days a week, sometimes for several hours at a time. We were, for the most part, observers; sometimes we asked questions, sometimes we watched people at work, sometimes we pored over laboratory notebooks. But we also participated in efforts to explain the results of the morphology experiments. We spent less time in the laboratory during the second year, but interviews, observations, the analysis of notebooks and documents, and participation in theoretical work continued. The following description of the morphology research is based in part
449 on our reconstruction of events that occurred prior to the time we entered the laboratory, and on our own observations during the two years we were associated with the laboratory.

Colloids and chemists

The discipline: colloid chemistry

Colloid chemistry is a remarkably complex discipline which is concerned with phenomena ranging in size from one to one-tenth of a micron. If we use the ordinary categories of liquid, solid, and gas to describe the phases of a physical system, then colloid chemistry can be described as the study of mixed or intermediate states such as mixtures of dissimilar liquids or liquid-gas systems of given substances. Bubbles, foams, emulsions, and slurries are the relevant phenomena in colloid chemistry. Such phenomena are part of everyday life in households and industries; thus, colloid chemistry (along with surface chemistry) is intimately tied to applications. It is important in creating exotic alloys as well as salad dressings that will not separate.

The chemists

An alarm sounds. Pilots race to their planes, take off, and climb rapidly to meet approaching bombers. But oil spews from their engines and smears the windshields. What causes this problem, and how can it be remedied? This problem provided Sydney Ross with a major post-doctoral research project at Stanford University during the 1940s. Ross went on to become a specialist in colloid chemistry, and now heads the laboratory we studied.

Ralph Kornbrekke majored in physics as an undergraduate. At the end of his first year in graduate school, he transferred from the laboratory he had been working in to the Ross' laboratory. His decision to transfer to Ross' laboratory was influenced by the recommendations of his peers, tacit institutional pressure to find a

funded project, and the fact that his original interest in solid state chemistry had led him to surface and colloid chemistry. Ross does not have a reputation as a grantsperson; but Kornbrekke was attracted to his laboratory by what he perceived as the "wide-open" character of colloid chemistry, and its "practicality". He viewed 450
the discipline as an "employable field"; indeed, he expressed a keen awareness of the differences between areas which are heavily funded as academic research but do not readily lead to employment upon graduation, and those areas which get modest levels of support within a university context but lead to numerous employment possibilities.

Henry Hollinger is a physical and theoretical chemist who specializes in thermodynamics and statistical mechanics. He is what we refer to as a *resident theoretician*. Colleagues (including Ross) come to him with theoretical and conceptual dilemmas, or simply to get a "theoretical framing" for an experimental piece of work which is ready to be reported. His role is such that he appears to get credit in an acknowledgement sometimes for a contribution that might give him a claim to co-authorship.

The laboratory atmosphere

Ross' laboratory consists of four rooms on the third floor of the Cogswell Laboratory Building on the Rensselaer Polytechnic Institute campus. There is also a basement in another building which is sometimes used by members of Ross' laboratory. Ross generally works very closely with one or two graduate students; there are usually three or four additional people working in the lab — other graduate students, undergraduates doing senior theses, and occasionally high school students soon to matriculate at RPI.

Ross has a strong bond of *professional* intimacy with his older and premier graduate students. He is uncomfortable with students who have "personal problems", and he will not tolerate what he refers to as "neurotic personalities" in his laboratory. Ross and his students are linked to the world outside the laboratory and the profession not by a shared social life but by anecdotes and tales about life "out there".

Ross does not see himself as a "director". His image of the educational transformation that ideally takes place in his laboratory is one in which "students" become "collaborators". He does not think that "too many ideas arise from cross-fertilization between the students"; he notes, however, that he does not "monitor their conversations" and there may in fact be more intellectually stimulating interactions among the graduate students than he is aware of. 451

As the morphology research developed from 1977 onwards, Kornbrekke and Ross developed the professional bond that would eventually make Kornbrekke the "top man" or "senior man" in the laboratory (Ross' designations for his premier advanced doctoral candidate). By the time we entered the laboratory toward the latter part of the Fall 1978 academic term, Kornbrekke was "top man" and nearly ready to begin writing (with Ross) the first paper on the morphology project, a report to the National Aeronautics and Space Administration (NASA), the agency which had funded the project. At this time, Kornbrekke and two other full-time PhD candidates were working in the laboratory, along with two first year MS students. There were also three undergraduate seniors working in the laboratory on theses related to the morphology project. Kornbrekke noted that he was working on "conceptual things" at this point, whereas earlier in his graduate career Ross had had him "doing *things*", that is, working on more or less mechanical problems. He was now in what Ross referred to as a "mature state"; another sign of this was the fact that he was now being given responsibility for writing reports for and meeting with officials of NASA. Still, the hands-on aspect of laboratory work remained a central part of Kornbrekke's research. We will have more to say about the laboratory atmosphere later on.

The morphology research

The following account of the morphology research focusses on the work of the graduate student Kornbrekke. It is based on in-depth interviews with Kornbrekke as well as with Ross and Hollinger; analyses of Kornbrekke's detailed log of his laboratory work; and our observations in the laboratory. Our account focusses in part on Kornbrekke's socialization as a scientist. We are concerned here with how Ross and Kornbrekke got involved in studying the morphology of a liquid-liquid dispersion; why they chose to study the two-component system benzene-water (with ethanol as a nonreactive solvent); what problems arose in the course of the research and how they were solved; and what contingencies were associated with the research.

During his first year in graduate school, Kornbrekke did some work on solar cells and surface effects. He also took a course on the chemistry of interfaces taught by Ross — and, for reasons noted earlier, Kornbrekke sought out Ross as an advisor. When Kornbrekke arrived in Ross' laboratory, two projects were in progress. One was an Environmental Protection Agency project on pesticides; the other was a NASA project on emulsions.

The morphology story begins at NASA. NASA scientists are interested in making alloys of two metals for certain space applications. But their efforts at "alchemy" prove unsuccessful. A researcher at NASA about to be promoted to project director contacts Ross. Ross is a NASA consultant and has a good reputation there. The NASA project director and Ross discuss a research proposal, the proposal is written, and the morphology project receives funding. This establishes the "NASA connection", a significant and continuing influence on the nature and directions of the morphology project.

Ross (1973, p. 54) had written a paper on "Adhesion vs. cohesion in liquid-liquid and solid-liquid dispersions" some five years prior to beginning work on the NASA project. This paper was a conceptual analysis prompted by Ross' readings in his collection of rare books in the history of science; the analysis contributed to Ross' view of alloys as emulsions. Metallurgists at NASA had been using phase diagrams as guides in their work on creating alloys. Experiments were therefore developed to study emulsions and elaborate the phase diagrams. The morphology of systems containing two immiscible liquids is not given in their phase diagrams; one of the early aims of the morphology research was to gather data that would make it possible to include morphology in the phase diagrams of two-component immiscible liquid systems. Ross put Kornbrekke to work on the morphology experiments.

Kornbrekke, more or less on his own at this point, had to decide what system — that is, what mix of liquids — he should study. One factor that had to be taken into account was NASA's prohibition on experiments in space with aniline, a substance NASA scientists considered too dangerous to work with. One of the senior graduate students in the laboratory had been studying foaming for a certain system and had a great deal of data on surface tensions for this system; Kornbrekke decided it was reasonable for him to study the same system. His efforts were frustrated. He constructed isothermal bath experiments but was unable to make up a mixture which behaved predictably near the critical temperature (inversion point). He turned to the literature and found eight different critical temperatures reported for the system he was studying, with a range of 15 degrees. This appeared to be a recalcitrant system. Ross, sensing Kornbrekke's frustration (Kornbrekke had worked an entire summer so far), told him to drop the system he was working on. He told Kornbrekke that another student was measuring interfacial and surface tensions for a benzene and water system and that perhaps this system would "behave better". Kornbrekke was particularly attracted to this idea since it would involve a new experimental procedure. He would not need to use an isothermal bath. Isothermal baths, notoriously temperamental according to Kornbrekke, are used for cooling experiments; the new experiments would involve *manually shaking* test tube mixtures. This was an innovative approach to the morphology problem and appeared to offer some hope for success.

Ross, apparently still very much aware of Kornbrekke's frustration over the first set of experiments, suggested a procedure for the new set of experiments. He recommended setting up a variety of test tube mixtures, each having different volume fractions of benzene and water (this mixture soon came to be referred to around the laboratory as "oil and water"). The test tubes were to be *manually shaken* and visually examined to determine whether the emulsion type was benzene in water or water in benzene. The morphology mystery emerged from these shaking experiments.

Kornbrekke's first problem was to determine what emulsion type he had after shaking. This sounds innocent enough, but it is fraught with difficulty. After shaking, most of the mixtures separate quite rapidly and one sees a colorless solution with a highly active boundary or interface between the two volumes of liquids. All the standard techniques for determining emulsion type or morphology apply only to *stable* emulsions. Kornbrekke had *to teach himself to see* what a short-lived emulsion "looks like". By "thinking about what you have" (in Kornbrekke's words) and hypothesizing relevant visual parameters, Kornbrekke had to make the data manifest themselves. One could hardly ask for a closer parallel between "scientific observation" and "aesthetic seeing" (Hanson, 1958; 1969) than what is involved in determining emulsion types in these shaking experiments. The foreground-background character of perception is crucial here; Kornbrekke found it extremely difficult to judge the relative motions of the liquids as they separated and moved up or down in the test tube. He could not judge initially whether droplets were moving up (or

454 down) against a stationary liquid background or whether the liquid background was moving down (or up) while the droplets remained stationary. After numerous observations, Kornbrekke was able to distinguish "a roving interface"; droplets seemed to be moving in two directions, but "one direction was smooth, the other showed oscillation". He "saw" that the "emulsified droplets *break* into their phase". In the end, Kornbrekke learned to focus on the interfacial boundary because it forms right after the shaking stops. By attending to the way the droplets coalesced and the direction in which they ruptured, Kornbrekke was able to satisfy himself and others that he was able thus to determine the morphology of different mixtures. In spite of the fact that the shaking technique was not mechanized (something we heard several outside scientists object to), the results seemed to be *reproducible*. The question was: "Is the result reasonable?"

Kornbrekke had anticipated certain results based on conversations with Ross; his expectations were violated in a rather odd way. When he shook a test tube containing approximately equal volumes of liquids A and B, he sometimes got A dispersed in B and sometimes B dispersed in A. Thus, it was this *inconsistency* — in effect, the impossibility of determining the inversion point — that was reproducible. Kornbrekke was unwilling to go to Ross with such results. He consulted a graduate student working in the laboratory who had a background in statistics. The student was a chemical engineering major who had recently moved to a workplace right across from Kornbrekke's territory in the laboratory. This student suggested that Kornbrekke should try an "averaging procedure". Kornbrekke obtained data on the probability of getting A dispersed in B for a given relative initial volume of A and B. These data were plotted and Kornbrekke discovered that a straight line relationship could be obtained by plotting logarithms. Ross then noticed that the analytic expression for the plotted data was formally analogous to the Boltzmann expression for entropy. This was interpreted to mean that here is a case where the Boltzmann W's of statistical mechanics would be proportional to *probabilities* that are *directly measurable*. This was a remarkable possibility, according to the chemists; they spoke, apparently somewhat loosely, about the possibility of "seeing entropy".

After discussions with Hollinger about interpreting the experimental results appeared to shed some light on some of the thermodynamics arguments, Ross began preparing a manuscript on the
455 research. In doing so, he discovered that the thermodynamics arguments which were to provide a basis for a mathematical description of the observations were invalid. Not only was the derived curve quantitatively incorrect, but it even had the wrong slope. There were thus two anomalies that emerged in the morphology research. One was the experimental result that the inversion point is stochastically related to the volume fraction; the second was theoretical: straightforward thermodynamics arguments which should have worked (according to Hollinger) failed.

At this point, the theoretical work was in limbo. Ross felt that the experimental results were intrinsically interesting and deserved publication on that basis. It seemed to him that an extremely complicated model based on high level mathematical work would be needed to provide a theoretical basis for the observations. In his judgement, further experimentation was unwarranted. He quite willingly admitted that at this point he had lost interest in the problem.

Kornbrekke continued to think about the problem of modeling the experimental results; he suggested a "thin films" model in which the driving forces toward nucleation would come primarily from the cohesion of the liquid. Ross and Kornbrekke discussed the model and the possibility of additional experiments at some length. When Hollinger was involved in these discussions, he would offer an occasional suggestion or question. He felt, as we noted earlier, that "if statistical mechanics ever works, it should work now", that is, in the case of the liquid-liquid dispersion results. He continued to reserve judgement until he could examine the thermodynamics arguments upon which Ross relied for his conclusion that the slope of the derived curve was wrong.

Ross countered every suggestion made by Kornbrekke regarding what could be done experimentally to determine the driving force behind the morphology process. According to Ross, the effects due to cohesion cannot be readily separated from those due to viscosity and surface tension. It is possible to imagine various experimental configurations but the physical situation is such — apparently — that parameters cannot be separated. Therefore, it is not possible to treat one parameter as an independent variable. Ross believed that he and Kornbrekke had been quite lucky to start with a configuration that led to a relation in which the probability of a certain morphology is a smooth continuous function of the concentration.

Kornbrekke and Hollinger remained interested in the theoretical 456
problems raised by the morphology experiments, but were uncertain about how to proceed. Zenzen then came across a general article on critical point phenomena in *Scientific American* which he (acting as a participant) thought might stimulate some new thoughts on the morphology problem (Wilson, 1979). Ross left the country at this point, leaving Kornbrekke with the task of sending the morphology research paper to the *Journal of Colloid and Interface Science*. Kornbrekke had serious reservations about some of the claims made in the paper and about the interpretations; it should be stressed that Ross takes full responsibility for writing papers co-authored with graduate students. Kornbrekke began re-examining and re-plotting his original data (gathered about a year and a half previously), and discovered some arithmetical errors and questionable experimental practices. In any event, the paper was submitted.

The morphology project was beginning to run down. The submitted paper was a revised version of a report to NASA. It was not accepted. One of the major criticisms raised by the referees was that the paper lacked a theoretical foundation. Hollinger was able to help with a theoretical frame (in his role as theorist), and after some additional modifications the paper was resubmitted and accepted. We will examine these documents in some detail below. But first we offer some reflections on what we call "a phenomenological laboratory" prompted by Kornbrekke's experience of "teaching himself to see".

A phenomenological laboratory

Kornbrekke's experience in learning how to identify emulsion types

in the shaking experiments was not, in the context of Ross' laboratory, idiosyncratic. Indeed it appears to reflect precisely the outcome Ross consistently seeks in what can be characterized as "a phenomenological laboratory". The emphasis is on "seeing", and especially on "learning to look for something different". Ross admires Faraday. He has written scholarly papers on Faraday, and a portrait of Faraday hangs in the office adjoining the room where the shaking experiments were carried out. Faraday's style of research is the model for Ross' scientific work, and the model he
457 tries to pass on to his students. One hears Faraday's motto: "Work, Finish, Publish"; and one senses Faraday's spirit as Ross tries to educe in his students that power of observation that will allow them to look at a situation and see what no one else has seen before.

There are, Kornbrekke reports, "anomalies everywhere"; and this is generally accepted not as a temporary situation awaiting its Kuhnian resolution, but as characteristic of a creative research environment. The anomalies sometimes cause consternation; sometimes they are viewed as curious puzzles; sometimes they are ignored. But they are always there. Bits of rubber tubing, styrofoam, scotch tape, and aluminium foil are converted, adapted, modified, and transmuted into tools of modern research. Idiosyncratic test-tubes, floor to ceiling tubing, outrageous twists and turns in home-made glassware are additional signs of the alchemic instincts drawn on in this laboratory.

But there is more to this laboratory than the romantic idea of a deceased scientist's spirit, or the equally romantic notion of alchemic instincts. The phenomenological quality of this laboratory is a sign of adaptation to an environment of scarcity. The interface between the laboratory scientist and his/her technology becomes more tightly bonded under conditions of scarcity than it is likely to become in a well-funded laboratory where state-of-the-art equipment can be taken for granted. The very shaking experiments at the center of the morphology mystery were designed to provide Kornbrekke with a relatively simple, inexpensive project. As we travel through the morphology project with him, we find Kornbrekke cleaning glassware, scrambling for parts from shelves of outdated electronic equipment to put together a Wheatstone bridge for the thermo-couple that he will use instead of an expensive electronic thermometer for temperature control, blowing his own glassware. Survival — and success — in this laboratory depends on getting the "hang of putting things together out of what's available". So Kornbrekke, like the graduate students before him, has developed a sense of the importance of *generational linkages* — the tranmission of survival skills takes place through the interactions between the older and the newer students. This is not unusual except that in this case scarcity makes it a more salient and crucial feature of social interaction. People must be good at electronics, and handy with tools and various materials so that the inevitable problems of building, maintaining,
458 and modifying apparatus can be handled. It seems obvious to us that selection and socialization in such an environment of scarcity will produce a different scientific "type" than selection and socialization in a laboratory environment of abundance. The environment of scarcity seems more likely than the environment of abundance to put a premium on a keen awareness of, and intimacy

with, a wide range of "things" which — in one form or another — can be drawn on to nourish the research process — and among these "things" one must count the five unaided senses.

The rhetoric of persuasion in science

Three principal documents are associated with the morphology research we have described. The first, published as a quarterly report to NASA, is titled "Emulsion-type inversion for the system benzene, ethanol, and water"; it is co-authored by Kornbrekke and Ross. The name order is reversed in the paper submitted for publication; it is titled "Change of morphology of a liquid-liquid dispersion as a stochastic process". This paper was submitted to the *Journal of Colloid and Interface Science* in the Winter of 1980. In March, the journal editor informed Ross that he and Kornbrekke would have to comment on the criticisms raised by the two referees and/or make appropriate revisions before a "final decision on acceptance" could be made. The paper was revised and resubmitted in September 1980. Within less than a week, Ross and Kornbrekke were notified that the paper had been accepted for publication.

The relationship between documents and scientific practice has been discussed in earlier studies of laboratory life. Latour and Woolgar for example (1979, pp. 51-52, 245), argue that there is "an essential similarity between the inscription capabilities of apparatus, the manic passion for marking, coding, and filing, and the literary skills of writing, persuasion, and discussion"; the laboratory is "a system of literary inscription". Writing is "not so much a method of transferring information as a material operation of creating order". Knorr and Knorr (1978, p. 39) have studied the relationship between scientific practice and published paper in a laboratory in some detail. They analyze texts as media for *constructing* reality rather than as "data which represent reality". We do not have the space to discuss all the documents associated with the morphology project in detail. The works of Latour and Woolgar, and Knorr and Knorr, offer a rationale for focussing on the *persuasive* aspects of article production. We will examine the three principal documents produced by Ross and Kornbrekke, then, as persuasive efforts. We do not claim they are designed to persuade people that something that doesn't exist does exist; nor that persuasion is the only aim scientists pursue in writing papers. We do claim that scientists need to use a *rhetoric of persuasion* in order to draw attention to and legitimate their findings. Getting a paper which sets forth claims accepted for publication requires skillful use of a persuasive rhetoric. This is not simply a matter of choosing the "right" words, but of deciding when and how to use analogies, mathematics, systematic theory, and so on.

We begin by describing some of the differences among the three documents and in particular between the paper originally submitted for publication (M1) and the revised version (M2) accepted for publication. We will then examine these differences in terms of their persuasive relevance.

The manuscript submitted for publication (M1) was a revised version of the first formal document on the morphology research, the NASA report. The text of the eighteen-page NASA report is all under one subheading: "Phase diagrams and morphology". There are five references, one table, and six diagrams in the NASA report. M1 is organized as follows: (1) abstract, (2) subheadings: Factors affecting morphology; Experimental section; Results. Only two of the five references in the NASA report are included among the seven references in M1.

Most of the text changes in M1 make the argument more general, more conclusive and less controversial (this is accomplished by selecting words that indicate greater confidence in what is being reported, or by adding qualifications), and more technical. Here are some illustrations: (1) a "certain range" (NASA report) becomes an "intermediate range" in M1 (specification); (2) "is shaken a large number of times" (NASA report) becomes "is given a large number of trials" in M1 (technicization); (3) "Phase diagrams and morphology", the single subheading in the NASA report, becomes "Factors affecting morphology" in M1 (generalization); (4) "defined" (NASA report) becomes "defined operationally" in M1 (specification); (5) "cohesive forces overcome to some degree by the physical action of shaking" (NASA report) becomes in M1 "cohesive forces must be overcome by an input of energy from mechanical agitation" (technicization); and (6) "This mechanism holds..." (NASA report) becomes "The rule that..." in M1 (generalization). In addition, there is a pragmatic aspect to article production illustrated by the changes in the explanatory framework adopted in each document. The NASA report contains a speculative section on entropy; this is replaced by a magnetic coin analogy in M1; in response to referees' criticisms, M2 eliminates the magnetic coin analogy and introduces a statistical mechanics argument. Let us examine a few of the details of these transformations.

In the NASA report, the following mathematical discussion occurs:

> The cohesive energies of the two liquids leading to one type of emulsion may be opposed or assisted by the tendency of the system to produce the least interfacial area. When the more cohesive liquid is present at a minority volume fraction, the tendency for it to appear as the dispersed phase is assisted; but when it is present at a majority volume fraction, the tendency is opposed. The inversion point of the emulsion occurs at a volume fraction where the two tendencies are equal. These two factors, energy or force of coalescence, and degree of subdivision of liquids (and area of films) depends upon the amount of shearing of the two liquids as well as their volume. Hence the viscosity (η), density (ϱ), surface tension (γ), interfacial tension (γ_i) and volume fraction ϕ (where ϕ represents the volume fraction of (A) the less dense phase) of each liquid must be considered to determine the Helmholtz Free Energy (ΔA) of coalescence:
>
> $$A = \gamma\Delta\Sigma,\ \Sigma = \frac{k\phi\gamma_i}{\varrho\eta}$$
>
> where k is the constant which relates ϕ, ϱ, η and γ_i to the area.

In another section of the NASA report, there is a discussion of probability and entropy:

> Probability of the existence of one state versus another is a function of the entropy change between states. The inversion region describes conditions where two different states can exist. The probability is related to the entropy difference as follows:

> $$\Delta S = S_{final} - S_{initial} = k\ \ln(1-P) - \ln P$$
>
> $$\Delta S = -k\ \ln\left(\frac{P}{1-P}\right)$$
>
> The Helmholtz Energy, if differentiated with respect to temperature (T) is also ΔS:
>
> $$\left(\frac{\delta \Delta A}{\delta T}\right)_{p,V} = \Delta S = \frac{\delta}{\delta T}\left(\frac{k_B \gamma_B (1-\phi)\gamma_i}{\varrho_B \eta_B} - \frac{k_A \gamma_A \phi \gamma_i}{\varrho_A \eta_A}\right)_{p,V}$$ 461
>
> Equating the two expressions gives:
>
> $$-k\ \ln\left(\frac{P}{1-P}\right) = k_B(1-\phi)\frac{\delta}{\delta T}\left(\frac{\gamma_B \gamma_i}{\varrho_B \eta_B}\right)_{p,V} - k_A \phi \cdot \frac{\delta}{\delta T}\left(\frac{\gamma_A \gamma_i}{\varrho_A \eta_A}\right)_{p,V}$$
>
> This equation is similar in form to the one obtained empirically, but with additional modifying terms.

There is also a reference in the NASA report to Maxwell's equations, although the equations are not actually written out:

> The dispersement of tiny droplets of one phase will alter the conductivity in the direction of the conductivity of that phase and proportionally to the quantity present, as in Maxwell's equations.

All mathematical equations, with the exception of one which has no theoretical import (the standard deviation formula) are deleted in M1.

M1 is organized into three sections: factors affecting morphology, experimental section, and results. There are no references in M1 to the Helmholtz Free Energy equations, nor to entropy. The emphasis is on the stochastic nature of the "ability of the dispersion to invert":

> The morphology of systems containing two immiscible liquids is not a phase-diagram variable, nor can it be predicted from any current theory but must be observed in every case. The ability of the dispersion to invert is spread over a range of compositions, and within that range only a probability of one type rather than another can be determined.

The entropy discussion and the accompanying equations are replaced in M1 with a magnetic coin analogy. This change reflects the uncertainties associated with the otherwise interesting exercise in linking probability and entropy as an explanatory strategy. In M1, Ross and Kornbrekke eschew esoteric argument in favour of a modest explanatory effort based on an analogy to an everyday phenomenon — coin tossing. The idea of a *magnetic* coin subtly transforms the everyday phenomenon into a complex physical analogy but one that remains intuitively accessible (at least in the view of Ross and Kornbrekke). It should be noted that the mag-
netic coin analogy was discussed at great length in trying to account 462
for the results of the shaking experiments.

Ross and Kornbrekke were not persuasive enough in M1 to satisfy the two chemists who refereed their paper. They were, however, persuasive enough to earn a recommendation for publication with revisions from one referee. The major criticism in the other review turned on theory. In order to satisfy the second referee, Ross and Kornbrekke had to delete the magnetic coin analogy (which the referee saw as an indication that their treatment was not "fundamental" and therefore more suited to a chemical engineering journal), and construct a theoretical frame for their experimental results. They were able to supply the theory with some assistance from Hollinger. The major change in M2 is the inclusion of a statistical mechanical interpretation in place of the magnetic coin analogy.

The abstract for M2 begins with the statement: "We report a new phenomenon"; this is followed by a more general statement than occurs in the earlier versions — the new phenomenon is "that the morphology of an unstabilized liquid-liquid dispersion is predicted by a statistical law rather than a causal law". It should be noted that this rhetoric is designed to establish the "fundamental" nature of the way in which Ross and Kornbrekke treat the morphology results. This is the significance of the reference in M2 to "lawfulness" — even though the treatment remains statistical in keeping with the experimental data.[4]

The "alchemic" rhetoric of "created" and "disappeared" in M1 is transformed into a more mechanistic mode in M2: "retraction" replaces "disappears", and "extended as films" is substituted for "created"; and "*tend* toward coalescence of like liquids" becomes "*cause* the extended liquids to retract" (our emphases). In general, the rhetoric in M2 is more mechanistic and causal or deterministic. The same rhetorical tendency characterizes the transition from scientific practice to scientific exposition. There is no explicit reference, for example, to the fact that the morphology research involved shaking test tubes *by hand* in any of the three documents.

In M2, the words "shaking" and "agitation" are used synonymously. We find such phrases as "vigorously mixed", "thorough mixing", "vigorously shaken up and down", "mechanically conferred motion", and "externally applied agitation", but nothing about "manual-" or "hand-shaking". There is a clear effort in M2 to portray the shaking or agitation procedure as
463 a mechanistic, controlled procedure. For example: (1) "In the foregoing argument the mode and degree of mechanical agitation in creating the dispersion is taken to be invariable", elsewhere in the sentence this phrase is taken from, the word "shaking" is modified by "manual" and the word "agitation" is modified by "mechanical", and this *might* lead readers to assume that the test tubes were shaken in appliances when they see the words "mechanical agitation" used to describe the shaking procedure; (2) "To ensure that shaking produced each time a consistent and thorough degree of mixing, several precautions were maintained. The length of time and manner of shaking were kept uniform for all trials"; (3) the authors cite a 1939 paper in which Sasaki points out that dispersion type is affected by the mode of agitation;[5] they go on to list other factors that are known to affect dispersion type, and note that in the present case all the relevant variables are "frozen" and only volume fraction is allowed to vary; (4) "Some variation in shaking technique and method of mixing was tried and gave the same result for the time of separation".

The failure to specify that the test tubes were shaken by hand and the stress on the controls established to ensure uniform agitation (and replicability) are especially noteworthy because in the two instances in which we were present when the morphology research was presented to a scientific audience, physicists, chemists, and engineers raised strenuous objections to, and queries about, the shaking procedure. Thus, it appears that Ross and Kornbrekke enhanced — or believed they enhanced — the persuasiveness of their argument by not specifically referring to "manual-" or "hand-shaking". On the other hand, they might claim that "insiders" who read the paper will know that the test tubes were shaken by hand. In any case, the discussion of the shaking experiments is consistent with the general tendency to mechanize rhetoric in the movement from the initial to the later stages of scientific practice, and from scientific practice to formal expositions observed in this and other laboratory studies.

Understanding laboratory life 464

1. Reflections on life among the colloids and the chemists

We begin our analysis of our observations in the laboratory by reflecting on the problems of studying scientific practice, and on the way in which we were led to the conclusion (supported by the results of other studies of scientific practice) that contingencies are constitutive of scientific practice and products.

Orienting ourselves to Ross' laboratory and the morphology research was a difficult task. Part of the difficulty involved finding a point of entry, something we could identify roughly as the "beginning" of a particular phase of the laboratory work. As we interviewed Ross and Kornbrekke, we found that, in their view, research is a rather continuous process punctuated by the arrival of a new "man" in the lab, or of a new grant, or of other factors moving from outside to inside the laboratory. Thus there seemed to be two overlapping histories: one, a continuous story at the level of ideas and experiments; the other, a story characterized by the rhythms of people starting and finishing projects in the lab, and by the pushes and pulls of the funding cycle.

Initially, we attempted to establish a point of reference by focussing on experimentally and theoretically anomalous aspects of the research in progress. This gave us something around which we could try to organize our own research, something on which to hang a narrative. However, things were not to work out so simply.

As we grew more accustomed to laboratory life, we found that the importance attached to anomalous features of the research process varied greatly. Sometimes a theoretically perplexing situation was stressed because publication would be impossible unless the difficulty could be resolved. But if the experimental work was going well, there was a tendency to argue that the data were intriniscally interesting and should be published without theoretical embroidery. The ebb and flow of judgements about the significance of anomalies made it extremely difficult for us to construct a sort of one-dimensional narrative. The day-to-day, ongoing work in the laboratory seemed to continually overflow simple categorizations. This work appeared as a large series of responses to imposed demands, perceived needs, given conditions, etc. — including:

(1) The background knowledge and experience of the researcher; what courses he/she has taken; what journal articles he/she reads and remembers; what equipment he/she is familiar with; 465
what experimental techniques he/she knows; how cautious he/she is about materials; how methodical he/she is; whether he/she is theoretically or experimentally oriented; whether he/she does exhaustive literature searches before beginning an

experiment; how creative he/she is at experimental design; how patient he/she is with delicate apparatus; how open he/she is to new phenomena; how secure he/she is in terms of ability and position; how willing he/she is to make his/her mistakes known to others; how open he/she is to advice from others...

(2) what equipment is available, and how much it costs; the possibilities for modifying given instruments, and the costs involved...

(3) the style of the laboratory and the way it is organized for research; available funds and attitudes about how they should be spent; contractual obligations with funding agencies ("ropes attached to research funding", in Kornbrekke's words); the pressure on PhD candidates to finish quickly; the pressure to choose research which is fundable and likely to lead to many job prospects; the pressure to be productive...

(4) the respect people have for each other's judgements; the roles various people play in the research process and in the university community; the form that communication takes depending on who is involved; the biases researchers have about ideals in science and education...

A multitude of contingencies have to be continually managed. As our work proceeded and our involvement intensified, we saw these contingencies increase and form an interlocking structure, a structure which was inseparable from "the research itself".

By focussing initially on anomalies, our research became an anomaly! We had been proceeding as if the "real research process" (which we at first identified with the discovery and analysis of certain anomalies) could be separated from the contingencies. As our work progressed, we experienced a shift in conceptual gestalt — the contingencies were *constitutive* of the research process. They continually *problematized* research, in such an intimate way that we had to abandon any vestigial ideas we had about "externalities".

2. *Contingencies and science*

The concept of "contingencies", the more complex concept of "contingencies as constitutive of scientific practice and products", and the general notion of "the social construction of scientific facts" are all being used in laboratory life research and in science studies generally. We believe these are useful ideas; however, a great deal of work remains to be done in order to clarify these ideas and determine whether, to what degree, and in what circles in science studies consensus about the meaning and usage of these ideas can be achieved. It would be premature for us to try to resolve these difficulties, and presumptuous of us to try to construct a theory or model of scientific practice based on the ideas of contingency, constitutiveness, and constructivism. More modestly, our objective in this section is to offer some remarks as a contribution to the discussion and debate about contingencies and science. Our remarks are general, but we offer them as conjectures not as generalizations applicable to all of science or as prescriptions for "good" science.

One of the difficulties we face in trying to convey the way in which laboratory workers create, elicit, and incorporate contingencies, and the way scientific products or results embody contingencies, is that when they are simply enumerated contingencies seem quite unremarkable. It is not surprising that certain instruments are unavailable, break down, or need to be drastically modified. Nor is it surprising that literature searches, professional meetings, and luncheon discussions sometimes lead to ideas which structure ongoing research in important ways. Taken singly, or in a list, such things seem to be discrete events, the inevitable "asides" of any directed action in the "real" world. The point is, however, that the research process is *not* primarily determined by "nature" or "physical reality". Scientific knowledge is created out of available resources — including formal and informal modes of communication, and instrumentation (Mulkay, 1979, pp. 60-61). In the deepest sense, the available resources in a given laboratory refer to the researchers' capacities for creative and critical thought, persuasion, communication, conflict, and cooperation. The indeterminacy of scientific criteria, the "looseness" of laboratory research, provide room for the exercise of those capacities. It is not as if a determinate path to some piece of information pre-existed and the researchers are deflected toward or away from that path by various sorts of perturbations. Rather, there is always a context, an inherited, assigned, or constructed problem situation which must be continually problematized and kept in motion. This flux (change without directionality) can be maintained and eventually given direction by the creative use of relatively forced choices and judicious selections from among relatively free choices (i.e., where alternatives are available). We have then a kind of evolution from randomness, the building of an organism — the research process — from an environment which the organism itself helps to create while it itself is growing and transforming. In this evolution, the discrete events called contingencies lose their contingent aspect because of a complex implication, literally an enfolding, which makes them *constitutive of*, and not *external influences on*, research.

The research environment the research process helps to create is the environment of the immediate problem-context and it is structured primarily by the instrumentation and first order mediation of informal dialogue among the researchers. There is also a more remote and subtle background environment which is formed by such things as the social structure of the laboratory, the style of the researchers, the respect people in the laboratory have for each other's judgements, the reputation of the laboratory outside the university, and the relationship of the laboratory to the larger university community.

For the most part, this more general environment functions as a background. The research process does not *usually* make use of this domain to problematize things, nor is it *likely* to lead to relatively forced choices. We have then a sort of figure/ground structure but a complex one in that the figuration (the research process) operating with a general ground creates and maintains itself from a more immediate ground which it helps to create.

Having said this we must immediately stress the dynamical features of this characterization. Figure/ground have, unfortunately, connotations of fixity and definite boundaries. We do not intend those connotations here. The figure/ground structure itself may be radically altered during the flux of research.

There is, then, a constantly shifting set of relevancy systems which alter the figure/ground structure of the research process (cf.

Gurwitsch, 1974, p. 121). The remote comes near and the near recedes. Thus while one might be tempted to identify levels of, or a hierarchy of, contingencies (instrumental, conceptual, psychological, and sociological), the dynamics of the situation we experienced in the laboratory suggest that this would be a fruitless approach. Which choices, relatively free or forced, will be most
468 significant for the research process can not be determined *a priori*. In studies such as this, the social investigators *are themselves* contingencies; with or without the help of the laboratory scientists, they *introduce* contingencies. These contingencies affect how the social investigators view the role of contingencies in science. The social construction of stories about laboratory life involves negotiations between social investigators and laboratory scientists; stories are not constructed in an "objectified", "detached" setting and manner. Recognizing the complexities involved in the interactions among contingencies and in the negotiations out of which sociological or ethnographic accounts of laboratory life emerge can help to problematize the *social* research process. Ruminating on these issues led us to consider the probabilistic figure/ground model we sketched in this section. It seems clear, furthermore, that many of the things we have said about laboratory work apply reflexively to our study.

Denouement: Indra's net

The Hindu god Indra inhabits a celestial palace that is covered by a network of jewels arranged so that by looking at any one you can see all the others reflected in it. We were reminded of Indra's net when we reflected on various ways in which our study reflected the morphology study — and even the morphology phenomenon. The *Scientific American* article that Zenzen brought to the chemists' attention, for example, appears to have implications for our research too. An often neglected aspect of natural systems is that in order to study them the theorist must be able to isolate some limited range of length scales (Wilson, 1979, p. 158):

> ...events distinguished by a great disparity in size have little influence on one another; they do not communicate, and so the phenomena associated with each scale can be treated independently. The interaction of two adjacent water molecules is much the same whether the molecules are in the Pacific Ocean or in a teapot. What is equally important, an ocean wave can be described quite accurately as a disturbance of a continuous fluid, ignoring entirely the molecular structure.

Thus, the causal analysis of multi-scaled complex phenomena presupposes a decomposition into levels which can be treated as effectively non-interactive. When we use categories such as "new
469 graduate student", "experienced" or "older graduate student", "researcher", "NASA", "resident theoretician", and so on, we already are performing a decomposition of the multi-scaled process called scientific research. Now it may be that such a decomposition is significant, but what should be noted is that the degree of success one has in developing a causal analysis of the phenomena associated with each scale will depend on the degree to which these scales can be treated as if they were non-interactive.

We see no *a priori* justification for interpreting the social production and construction of knowledge in the restricted manner necessitated by a commitment to causal analysis. This is not the way to make the sociology or ethnography of science "scientific"; the success of causal analysis in *natural* science depends on assumptions about non-interaction among levels of phenomena.[6] The appropriate "scientific" approach to science studies may be to *research* the question of interactions and leave open the question of scales. Various decompositions are possible in studying science, and it is probably wise to avoid the naive assumption that the obvious and the easiest decompositions are necessarily the most important ones or the crucial operative factors in the scientific research process. We ourselves may have fallen into some methodological and interpretative traps in this study for failing to take account of problems of decomposing phenomena.

We seem to have been in a position relative to the morphology project somewhat like that of the chemists relative to the morphology phenomena. Like Kornbrekke, we had to learn to "*see* changes of morphology". The clearest instance of this isomorphism occurred when we analyzed the three documents. Learning to see the changes in the papers was like learning to see the changes of morphology in the test tubes. There appears to be a reciprocity between some of the major conceptual features of the physical system at the core of the morphology project, the structure of the laboratory we studied, and the way in which we have come to think about scientific research in the course of this study. Perhaps we can say that the social production and construction of natural scientific knowledge is mirrored by the scientific production and construction of social scientific knowledge. Such a reciprocal animation of "natural" and "social" is quite harmonious with the view that foreground and background can reverse their roles.

Conclusion

470

In general, the results of our research are consistent with the "constructivist interpretation" of science associated with the recent research on laboratory life, and some other areas of science studies. The laboratory studies suggest that "scientific objects are *produced* and *reproduced* at the sites of scientific action". They converge on a view of scientific objects as socially situated, contingent, discursive accomplishments (Knorr, 1981, pp. 4-5). This conclusion does not necessarily support radically relativistic sociologies of knowledge. The social construction of scientific facts can be conceived, following Fleck (1979, p. 100), as events in the history of ideas, stylized by contemporary, local, social, cultural, and environmental factors (cf. Spengler, 1926, p. 59). Neither Latour and Woolgar nor Knorr, for example, adopt relativism. They do not deny that in some sense facts exist; there *is* a recalcitrant reality. Latour and Woolgar (1979, p. 180) argue, however, that the notion of a reality-out-there "is a *consequence* of scientific work rather than its cause". And Knorr (1979, p. 369) writes:

> A constructivist interpretation of knowledge is not to be confused with an idealist ontology: I do not maintain that reality is produced (constructed) in the sense that its appearance has no independent existence. Rather, this approach claims that once we see scientific products as selectively carved out, transformed and constructed from whatever is, we will also see that there cannot be any war-

rant in the claim that we have somehow captured (subject to progressive improvement) what is.

Perhaps the most important and controversial idea which has emerged in contemporary science studies, especially among students of laboratory life, is that social constructions are *constitutive* of truths, facts, and scientific knowledge — and of knowledge in general. The idea remains primitive and elusive. Clearly, there is an intention here of moving beyond traditional notions of social construction in the sociology of knowledge, and beyond the idea that science is *mediated* by society, or "externalities". It remains for future studies of scientific practice to establish whether sociologists of science are indeed, as we believe, engaged in a process which will radically alter our views on the nature of science by transcending traditional distinctions between "external" and "internal", "objective" and "subjective", and "reality-out-there" and "social constructions of reality".[7]

Sal Restivo (born 1940) is Associate Professor of Sociology at Rensselaer Polytechnic Institute. His paper on "The myth of the Kuhnian revolution in the sociology of science" will appear in Volume 1 of *Sociological Theory*, edited by Randall Collins (Jossey-Bass, 1982).

Michael Zenzen (born 1945) is Associate Professor of Philosophy at Rensselaer Polytechnic Institute. He has published in aesthetics, value theory and the philosophy of science and technology. His paper, "Thinking about technology: A meta-inquiry," appeared in *Man and World*, 11 (3/4), 1978. A paper on the origins of irreversibility is forthcoming in *Philosophy of Science*.

Authors' address: Rensselaer Polytechnic Institute, Troy, New York 12181, USA.

Notes

1. See, for example, Latour and Woolgar (1979); Knorr (1980); and Lynch (1981). For a critical introduction to this literature see Knorr (1981).
2. On contingencies and scientific research see Dean (1979, p. 212); Barnes (1977, p. 54); and R. Collins (1975, p. 496).
3. The Japanese film 'Rashomon' deals with various versions of "the truth" regarding an attack, a rape, and a robbery: see Richie (1972); cf. Latour and Woolgar (1979, p. 257); and H. Collins (1975, p. 205).
4. We do not mean to imply that statistical results are not "lawful"; see, for example, Bohm (1971, pp. 28-32). In this case, Ross seemed satisfied with *reporting* a statistical result. In the end, he and Kornbrekke were led to present this result in a theoretically-grounded manner that transformed "result" into "law".
5. Sasaki (1939), incidentally, discusses experiments in which test tubes are shaken by hand and describes two modes of manual shaking. This paper was not discovered until after the morphology experiments had been carried out.
6. Bloor (1976) advocates a causal sociology of science that ignores this point.
7. The citation for M2 is as follows: Ross, S. and Kornbrekke, R., "Change of morphology of a liquid-liquid dispersion as a stochastic process", *Journal of Colloid and Interface Science* 81 (1), pp. 58-68.

References

Barnes, B. 472
1977 *Interests and the growth of knowledge.* London, Routledge and Kegan Paul.

Bloor, D.
1976 *Knowledge and social imagery.* London, Routledge and Kegan Paul.

Bohm, D.
1971 *Causality and chance in modern physics.* Philadelphia, Pa., University of Pennsylvania Press.

Collins, H.
1975 "The Seven Sexes: A study in the sociology of a phenomenon, or The replication of experiment in physics", *Sociology* 9: 205-224.

Collins, R.
1975 *Conflict sociology.* New York, Academic Press.

Dean, J.
1979 "Controversy over classification: A case study from the history of botany", pp. 211-230 in: B. Barnes and S. Shapin (eds.), *Natural order.* London, Sage.

Fleck, L.
1979 *Genesis of a scientific fact.* Chicago, Ill., University of Chicago Press (originally published in German in 1935).

Gurwitsch, A.
1974 *Phenomenology and the theory of science.* Evanston, Northwestern University Press.

Hanson, N.
1958 *Patterns of discovery.* Cambridge, Cambridge University Press.
1969 *Perception and discovery.* San Francisco, Calif., Freeman, Cooper and Co.

Knorr, K.
1979 "Tinkering toward success: Prelude to a theory of scientific practice", *Theory and Society* 8: 347-376.
1981 "The ethnography of laboratory life: Empirical results and theoretical challenges", pp. 4-9 in: S. Restivo (ed.), *New directions in the sociology of science*, special issue of the *Newsletter* of the International Society for the Sociology of Knowledge 7 (1&2).

Knorr, K. and D. Knorr 473
1978 "From scenes to scripts: On the relationship between laboratory research and published paper in science", *Research Memorandum No. 132.* Vienna, Institute for Advanced Studies.

Latour, B. and S. Woolgar
1979 *Laboratory life.* London, Sage.

Lynch, M.
1981 *Art and artifact in laboratory science.* London, Routledge and Kegan Paul.

Mulkay, M.
1979 *Science and the sociology of knowledge.* London, G. Allen and Unwin.

Richie, D. (ed.)
1972 *Focus on Rashomon.* Englewood Cliffs, NJ, Prentice-Hall.

Ross, S.
1973 "Adhesion vs. cohesion in liquid-liquid and solid-liquid dispersion", *Journal of Colloid and Interface Science*, 42.

Sasaki, T.
1939 "On the nature of foam. IV. Phase inversion and foaming of emulsion consisting of acetic acid, ethyl ether, and water", *Chemical Society of Japan Bulletin*, 14: 63-72.

Spengler, O.
1926 *The decline of the West*. New York: International Publishers.

Wilson, E.
1979 "Problems in physics with many scales of length", *Scientific American* 241 (2): 158-197.

Social Science information, vol. 23, no. 3, 1982, pp. 447–473.

Bruno Latour

1.9

GIVE ME A LABORATORY AND I WILL RAISE THE WORLD

Now that field studies of laboratory practices are starting to pour in, we are beginning to have a better picture of what scientists do inside the walls of these strange places called 'laboratories' (Knorr-Cetina, this volume). But a new problem has emerged. If we are not able to follow up our participant-observation studies far enough to take in questions outside the laboratory, we are at great risk of falling back into the so-called 'internalist' vision of science. From the very beginnings of these microstudies, this criticism was levelled at us by scholars preoccupied by larger problems such as science policy, history of science, or more broadly, what is known as Science, Technology and Society (STS). For such topics, laboratory studies seemed utterly irrelevant. At the time, our critics were largely wrong because we first of all had to penetrate these black boxes, and to get firsthand observations of the daily activity of scientists. This was the foremost priority. The result, to summarize it in one sentence, was that nothing extraordinary and nothing 'scientific' was happening inside the sacred walls of these temples (Knorr, 1981). After a few years of studies, however, our critics would be right in raising again the naïve but nagging question: if nothing scientific is happening in laboratories, why are there laboratories to begin with and why, strangely enough, is the society surrounding them paying for these places where nothing special is produced?

The question appears innocent enough, but is actually a rather tricky one because there is a division of labour between scholars studying organizations, institutions, public policy on the one hand, and people studying micronegotiations inside scientific disciplines on the other. It is truly difficult to see common elements between the analysis of the laetrile controversy (Nelkin, 1979) and the semiotic study of a single scientific text (Bastide, 1981); between the study of indicators for following the growth of R&D and the history of the gravitational wave detector (Collins, 1975); or between the Windscale Inquiry and the deciphering of the mutterings of a few scientists during a chat at a bench (Lynch, 1982); it is so hard to grasp common features among these interests, that people tend to think that there are indeed 'macroscopic' problems, and that the two sets of issues ought to be treated differently, with different methods, by different breeds of scholars. This belief in a *real* difference of scale between macro- and micro-objects in society is common among sociologists (Knorr and Cicourel, 1981), but is especially strong in sociology of science. Many analysts of STS are proud of not entering at all into the content of science and into the microlevel of scientific negotiations, while, at the other end of the spectrum, some analysts claim that they are interested only in con-

Author's note: Many arguments developed here are commentaries on ideas discussed with my colleague Michel Callon. I wish to thank Mark Smith for his assistance in preparing the manuscript.

troversies between scientists (Collins, 1982), or even claim that there is no society at all or at least no macrosociety about which something serious could be uttered (Woolgar, 1981). The funny thing about this misunderstanding is that it reproduces on slightly different grounds the age-old polemic between 'internalist' and 'externalist' in the study of science and technology. While the debates of earlier times opposed 'social influences' to 'purely internal development' in accounting for the movement of scientific disciplines, people are now opposing 'public policy', 'large-scale economic push and pull' to 'micronegotiations', 'opportunism' and 'laboratory folklore'. The terms have changed, the belief in the 'scientificity' of science has disappeared, but the same respect for the boundaries of scientific activity is manifested by both schools of thought.

The time has now come for the analysts of scientists at work to deal with the naïve but fair criticism put to them by scholars interested in 'macro' issues. But there is of course no way that we can easily conciliate such profoundly different perspectives and methods. In particular, it is impossible for observers used to laboratory studies to leave this firm ground where so much has been achieved and simply dive into 'macro'

problems, computing gross national product percentages, citations and rewards and so on. If we do deal with these questions it will be on our own terms.

In this chapter, I would like to propose a simple line of enquiry: that is, to stick with the methodology developed during laboratory field studies, focusing it not on the laboratory itself but on the construction of the laboratory and its position in the societal milieu (Callon, 1982). Indeed, I hope to convince the reader that the very difference between the 'inside' and the 'outside', and the difference of scale between 'micro' and 'macro' levels, is precisely what laboratories are built to destabilize or undo. So much so, that without keeping back the discoveries we made while studying laboratory practices we can reassess the so-called 'macro' problems much more clearly than before and even throw some light on the very construction of macroactors themselves. I simply beg the readers to put aside for a time their belief in any *real* difference between micro- and macroactors at least for the reading of this paper (Callon and Latour, 1981).

I. 'Give me a place to stand and I will move the earth'

To illustrate my argument I will extract an example from a recent study done in the history of science (Latour, 1981a). We are in the year 1881, the French semi-popular and scientific press is full of articles about the work being done in a certain laboratory, that of Monsieur Pasteur at the École Normale Supérieure. Day after day, week after week, journalists, fellow scientists, physicians and hygienists focus their attention on what is happening to a few colonies of microbes in different mediums, under the microscope, inside inoculated animals, in the hands of a few scientists. The mere existence of this enormous interest shows the irrelevance of too sharp a distinction between the 'inside' and the 'outside' of Pasteur's lab. What is relevant is the short circuit established between many groups usually uninterested by what happens inside laboratory walls, and laboratories usually isolated and insulated from such attention and passion. Somehow, something is happening in these dishes that seems directly essential to the projects of these many groups expressing their concern in the journals.

This interest of outsiders for lab experiments is not a given: it is the result of Pasteur's work in enrolling and enlisting them. This is worth emphasizing since there is a quarrel among sociologists of science about the possibility of imputing interests to people. Some, especially the Edinburgh school, claim that we can impute interests to social groups 144
given a general idea of what the groups are, what society is made of, and even what the nature of man is like. But others (Woolgar, 1981) deny the possibility of such imputation on the grounds that we do not have any independant way of knowing what the groups are, what society is after and what the nature of man is like. This dispute, like most, misses the fundamental point. Of course there is no way of knowing which are the groups, what they want and what man is, but this does not stop anyone from convincing others of what their interests are and what they ought to want and to be. He who is able to translate others' interests into his own language carries the day. It is especially important *not* to rely on any science of society or science of man to impute interests because as I will show, sciences are one of the most convincing tools to persuade others of who they are and what they should want. A sociology of science is crippled from the start if it believes in the results of one science, namely sociology, to explain the others. But it is still possible to follow how sciences are used to transform society and redefine what it is made of and what are its aims. So it is useless to look for the profit that people can reap from being interested in Pasteur's laboratory. Their interests are a consequence and not a cause of Pasteur's efforts to translate what they want or what he makes them want. They have no a priori reason to be interested at all, but Pasteur has found them more than one reason.

1. Move one: capturing others' interests

How has Pasteur succeeded in capturing the interests of other indifferent groups? By the same method he has always used (Geison, 1974; Salomon-Bayet, 1982). He transfers himself and his laboratory into the mist of a world untouched by laboratory science. Beer, wine, vinegar, diseases of silk worms, antisepsy and later asepsy, had already been treated through these moves. Once more he does the same with a new problem: anthrax. The anthrax disease was said to be terrible for French cattle. This 'terrible' character was 'proven' by statistics to officials, veterinarians and farmers and their concerns were voiced by the many agricultural societies of the time. This disease was studied by statisticians and veterinarians, but laboratory practice had no bearing on it before Pasteur, Koch and their disciples. At the time, diseases were local events that were to be studied with all possible attention by taking into account all the possible variables – the soil, the winds, the weather, the farming system, and even the individual fields, animals and 145
farmers. Veterinary doctors knew these idiosyncrasies, but it was a careful, variable, prudent and uncertain knowledge. The disease was unpredictable, and recurred according to no clear pattern, reinforcing the idea that local idiosyncrasies had to be taken into account. This multifactorial approach made everyone extremely suspicious of any attempt to cut through all these idiosyncrasies and to link one disease with any single cause, such as a micro-organism. Diseases like anthrax, with all their variations, were typically what was thought not to be related to laboratory science. A lab in Paris and a farm in Beauce have nothing in common. They are mutually uninteresting.

But interests, like everything else, can be constructed. Using the work of many predecessors who had already started to link laboratories and anthrax disease, Pasteur goes one step further and works in a makeshift laboratory right on the farm site. No two places could be more foreign to one another than a dirty, smelling, noisy, disorganized nineteenth-century animal farm and the obsessively clean Pasteurian laboratory. In the first, big animals are parasited in seemingly random fashion by invisible diseases; in the second, micro-organisms are made visible to the observer's eye. One is made to grow big animals, the other to grow small animals. Pasteur (the 'shepherd' in French) is often seen in the enthusiasm of the moment as the inventor of a new animal husbandry and a new agriculture, but at the time these two forms of livestock have little relation to one another. Once out in the field, however, Pasteur and his assistants learn from the field conditions and the veterinarians and start creating these relations. They are interested in pinpointing all the variations in the onset and timing of the outbreaks of anthrax and in seeing how far these could fit with their one living cause, the anthrax bacillus. They learn from the field, translating each item of veterinary science into their own terms so that working on their terms is also working on the field. For instance, the spore of the bacillus (shown by Koch) is the translation through which dormant fields can suddenly become infectious even after many years. The 'spore phase' is the laboratory translation of the 'infected field' in the farmer's language. The Pasteurians start by learning this language and giving one of their own names for each of the relevant elements of the farmer's life. They are interested in the field but still useless and uninteresting for the farmers and their various spokesmen.

2. *Move two: moving the leverage point from a weak to a strong position*

At this point Pasteur, having situated his laboratory on the farm, is going to transfer it back to his main workplace at the École Normale Supérieure, taking with him one element of the field, the cultivated bacillus. He is the master of one technique of farming that no farmer knows, microbe farming. This is enough to do what no farmer could ever have done: grow the bacillus in isolation and in such a large quantity that, although invisible, it becomes visible. Here again we have, because of laboratory practice, a variation of scale: outside, in the 'real' world, inside the bodies, anthrax bacilli are mixed with millions of other organisms with which they are in a constant state of competition. This makes them doubly invisible. However, in Pasteur's laboratory something happens to the anthrax bacillus that never happened before (I insist on these two points: something happens *to the bacillus* that *never* happened before). Thanks to Pasteur's methods of culture it is freed from all competitors and so grows exponentially, but, by growing so much, ends up, thanks to Koch's later method, in such large colonies that a clear-cut pattern is made visible to the watchful eye of the scientist. The latter's skills are not miraculous. To achieve such a result you only need to extract one micro-organism and to find a suitable milieu. Thanks to these skills, the asymmetry in the scale of several phenomena is modified: a micro-organism can kill vastly larger cattle, one small laboratory can learn more about pure anthrax cultures than anyone before; the invisible micro-organism is made visible; the until now uninteresting scientist in his lab can talk with more authority about the anthrax bacillus than veterinarians ever have before.

The translation that allows Pasteur to transfer the anthrax disease to his laboratory in Paris is not a literal, word-for-word translation. He takes only one element with him, the micro-organism, and not the whole farm, the smell, the cows, the willows along the pond or the farmer's pretty daughter. With the microbe, however, he also draws along with him the now interested agricultural societies. Why? Because having designated the micro-organism as the living and pertinent cause, he can now reformulate farmers' interests in a new way: if you wish to solve *your* anthrax problem you have to pass through *my* laboratory first. Like all translations there is a real displacement through the various *versions*. To go straight at anthrax, you should make a detour through Pasteur's lab. The anthrax disease *is* now at the École Normale Supérieure.

But this version of the translation is still a weak one. In Pasteur's lab, 147
there is a microbe, but anthrax infection is too disorderly a thing to be explained with a single cause only. So the outside interests could as well say that the laboratory has no real bearing on the spread of anthrax disease, and that it is just plain arrogance for a scientist to claim that he holds the key to a real disease 'out there'. But Pasteur is able to make a more faithful translation than that. Inside the walls of his laboratory, he can indeed inoculate animals he has chosen with pure, much-diluted culture of anthrax. This time, the outbreak of an epizootic is mimicked on a smaller scale entirely dominated by the charting and recording devices of the Pasteurians. The few points deemed essential are imitated and reformulated so as to be scaled down. The animals die of the microbes, and only of that, and epizootics are started at will. It can now be said that Pasteur has inside his laboratory, on a smaller scale, the 'anthrax disease'. The big difference is that 'outside' it is hard to study because the micro-organism is invisible and strikes in the dark, hidden among many other elements, while 'inside' the lab clear figures can be drawn about a cause that is there for all to see, due to the translation. The change of scale makes possible a reversal of the actors' strengths; 'outside' animals, farmers and veterinarians were *weaker* than the invisible anthrax bacillus; inside Pasteur's lab, man becomes stronger than the bacillus, and as a corollary, the scientist in his lab gets the edge over the local, devoted, experienced veterinarian. The translation has become more credible and now reads: 'If you wish to solve your anthrax problem, come to my laboratory, because that's where the forces are reversed. If you don't (veterinarians or farmers) you will be eliminated.'

But even at this point, the strength is so disproportionate between Pasteur's single lab and the multiplicity, complexity and economic size of the anthrax outbreaks, that no translation could last long enough to keep the aggregation of interest from falling apart. People readily give their attention to someone who claims that he has the solution to their problems but are quick to take it back. Especially puzzling for all practitioners and farmers, is the *variation* of the disease. Sometimes it kills, sometimes not, sometimes it is strong, sometimes weak. No contagionist theory can account for this variety. So Pasteur's work, although interesting, could soon become a curiosity or more precisely, a laboratory curiosity. It would not be the first time that scientists attract attention, only to have nothing come out of it in the end. Microstudies remain 'micro', the interests captured for a time soon go to other translations from groups that succeed in enrolling them.
This was especially true of medicine which at the time was tired of 148
continuous fashions and fads (Leonard, 1977).

But here Pasteur does something on chicken cholera and on anthrax bacillus inside his laboratory that definitively modifies the hierarchy between veterinary science and microbiology. Once a great many microbes are cultivated in pure forms in laboratories and submitted to numerous trials to make them accelerate their growth or die, a new practical know-how is developed. In a few years, experimenters acquire skills in manipulating sets of materials that never existed before. This is new but not miraculous. Training microbes and domesticating them is a craft like printing, electronics, blue-ribbon cooking or video art. Once these skills have accumulated inside laboratories, many cross-overs occur that had no reason to occur anywhere else before. This is not because of any new cognitive attitude, or because suddenly people become conscious of micro-organisms they were unaware of before. It is simply that they are manipulating new objects and so acquiring new skills in a new idiosyncratic setting (Knorr, 1981).

The chance encounter that made possible the first attenuated culture of chicken cholera is well-known (Geison, 1974), but chance favours only well-prepared laboratories. Living causes of man-made diseases undergo so many various trials that it is not that surprising if some of these trials leave some microbes alive but weak. This modification would have been invisible if the laboratory had not tried to imitate the salient features of epizootics by inoculating many animals. The invisible modification of the invisible microbes is then made visible; chickens previously inoculated with the modified strain don't get cholera but they resist inoculation of intact microbes. Submitting cultures of chicken cholera to oxygen is enough to make them less virulent when they are inoculated into the animals. What is made visible through the lab statistics is the chain of weakened microbes, then strengthened microbes and eventually, strengthened animals. The result is that laboratories are now able to imitate the *variation of virulence*.

It is important to understand that Pasteur now does more and more things inside his laboratory which are deemed relevant by more and more groups to their own interests. Cultivating the microbes was a curiosity; reproducing epizootics in labs was interesting; but varying at will the virulence of the microbes is fascinating. Even if they believed in contagion, no one could with this one cause explain the randomness of the effects. But Pasteur is not only the man who has proved the relation of one microbe/one disease, he is also the one who has proved that the infectiousness of microbes could vary under conditions that could be controlled, one of them being, for instance, a first encounter of the body with a weakened form of the disease. This variation in the laboratory is what makes the translation hard for others to dispute: the variation was the most puzzling element that previously justified the scepticism towards laboratory science, and made necessary a clear differentiation between an outside and inside, between a practical level and a theoretical level. But it is precisely this variation that Pasteur can imitate most easily. He can attenuate a microbe; he can, by passing it through different species of animals, on the contrary, exalt its strength; he can oppose one weak form to a strong one, or even one microbial species to another. To sum up, he can do inside his laboratory what everyone tries to do outside but, where everyone fails because the scale is too large, Pasteur succeeds because he works on a small scale. Hygienists who comprise the largest relevant social movement of that time are especially fascinated by this imitated variation. They deal with whole cities and countries, trying to pinpoint why winds, soil, climates, diets, crowding, or different degrees of wealth accelerate or stop the evolution of epidemics. They all see – they are all led to see – in the Pasteurian microcosmos what they are vainly trying to do at the macroscopic level. The translation is now the following: 'If you wish to understand epizootics and soon thereafter epidemics, you have one place to go, Pasteur's laboratory, and one science to learn that will soon replace yours: microbiology.'

As the reader is aware, I am multiplying the words 'inside' and 'outside', 'micro' and 'macro', 'small scale' and 'large scale', so as to make clear the destabilizing role of the laboratory. It is through laboratory practices that the complex relations between microbes and cattle, the farmers and their cattle, the veterinarians and the farmers, the veterinarians and the biological sciences, are going to be transformed. Large interest groups consider that a set of lab studies talk to them, help them and concern them. The broad concerns of French hygiene and veterinary sciences will be settled, they all say, inside Pasteur's laboratory. This is the dramatic short circuit I started with: everyone is interested in lab experiments which a few years before had not the slightest relation to their fields. This attraction and capture were made by a double movement of Pasteur's laboratory to the field and then from the field to the laboratory where a fresh source of know-how has been gained by manipulating a new material: pure cultures of microbes.

3. Move three: moving the world with the lever 150

But even at this stage, what was in the laboratory could have stayed there. The macrocosmos is linked to the microcosmos of the laboratory, but a laboratory is never bigger than its walls and 'Pasteur' is still only one man with a few collaborators. No matter how great the interests of many social groups for what is being done in one laboratory, there is nothing to stop interests from fading and dispersing if nothing more than laboratory studies happens. If Pasteur stays too long inside his laboratory and, for instance, shifts his research programme using the anthrax microbe to learn things in biochemistry, like his disciple Duclaux, people could say: 'Well after all, it was just an interesting curiosity!' It is only by hindsight that we say that in this year 1881, Pasteur invented the first artificial vaccination. By doing so we forget that to do so it was necessary to move still further, this time from the laboratory to the field, from the microscale to the macroscale. As for all translations it is possible and necessary to distort the meanings but not to betray them entirely. Groups that accepted to pass through Pasteur's hands in order to solve their problems, nevertheless only go through him to their own ends. They cannot stop in his laboratory.

Pasteur, from the start of his career, was an expert at fostering interest groups and persuading their members that their interests were inseparable from his own. He usually achieved this fusion of interests (Callon, 1981) through the common use of some laboratory practices. With anthrax he does just that but on a more grandiose scale, since he is now attracting the attention of groups that are the mouthpiece of larger social movements (veterinary science, hygiene, soon medicine), and about issues that are the order of the day. As soon as he has performed vaccinations in his laboratory he organizes a field trial on a larger scale.

This field experiment was organized under the auspices of the agricultural societies. Their attention had been captured by Pasteur's former moves, but the translation ('solve your problems through Pasteur's lab') implied that *their* problems could be solved and not only Pasteur's. So the translation is also understood in part as a contract, the

counterpart of which is now expected from Pasteur. 'We are ready to displace all our interests through your methods and practices so that we can use them to reach our own goals.' This new translation (or displacement) is as hard to negotiate as the first one. Pasteur has vaccine for anthrax in his laboratory at Paris. But how can laboratory practice be extended? In spite of all the niceties written by epistemologists on that point, the answer is simple: only by extending the laboratory
151 itself. Pasteur cannot just hand out a few flasks of vaccine to farmers and say: 'OK, it works in my lab, get by with that.' If he were to do that, it would *not* work. The vaccination can work only on the condition that the farm chosen in the village of Pouilly le Fort for the field trial be in some crucial respects transformed according to the prescriptions of Pasteur's laboratory. A hard negotiation ensues between Pasteurians and agricultural interests on the conditions of the experiment. How many inoculations? Who will be the umpire? And so on. This negotiation is symmetrical to the initial one when Pasteur came to the farm site, trying to extract the few pertinent elements of the disease that he could imitate inside his laboratory. Here, the problem is to find a compromise that extends Pasteur's laboratory far enough – so that the vaccination can be repeated and work – but which is still acceptable to the farming representatives so that it is seen as an extension of lab science outside. If the extension is overreached, the vaccination will fail and Pasteur will be thrown back inside his laboratory by the disappointed farmers. If the extension is too modest, the same thing will happen: Pasteur will be considered to be a lab scientist uninteresting for others' outside use.

The Pouilly le Fort field trial is the most famous of all the dramatic proofs that Pasteur staged in his long career. The major mass media of the time were assembled on three successive occasions to watch the unfolding of what was seen as Pasteur's prediction. 'Staging' is the right word because, in practice, it is the public showing of what has been rehearsed many times before in his laboratory. It is strictly speaking a repetition, but this time in front of an assembled public which has previously invested so much interest and is now expecting its rewards. Even the best performer has stage fright, even if everything has been well rehearsed. Indeed this is what happened (Geison, 1974). But for the media it was not seen as a performance, it was seen as a prophecy. The reason behind this belief shows us exactly why the distinction between inside and outside of the laboratory is so misleading. If you isolate Pasteur's laboratory from the Pouilly le Fort farm, so that one is the inside and the other is the outside world, then of course there is a miracle for all to see. In his lab Pasteur says, 'all vaccinated animals will be alive by the end of May; all the untreated animals will have died by the end of May; and outside the lab the animals die or survive'. Miracle. Prophecy, as good as that of Apollo. But if you watch carefully the prior displacement of the laboratory to capture farmers' interest, then to learn from veterinary sciences, then to transform the farm back into the guise of a laboratory, it is still interesting, extraordinarily clever and ingenious, but it is *not* a miracle. I will show later that most
152 of the mystified versions of scientific activity come from overlooking such displacements of laboratories.

But there is still one step to make so that we reach our point of departure: the anthrax outbreaks and their impact on French agriculture. Remember that I said it was a 'terrible' disease. While saying this I heard my ethnomethodologist friends jumping on their chairs and screaming that no analyst should say that 'a disease is terrible' or that 'French agriculture' exists, but rather that these are social constructions. Indeed they are. Watch now how the Pasteur group is going to use these constructions to their advantage and to France's. Pouilly le Fort was a staged experiment to convince the investors – in confidence and later in money – that the translation made by Pasteur was a fair contract. 'If you want to solve your anthrax problem go through my microbiology.' But after Pouilly le Fort, everyone is convinced that the translation is now: 'If you want to save your animals from anthrax, order a vaccine flask from Pasteur's laboratory, École Normale Supérieure, rue d'Ulm, Paris.' In other words, on the condition that you respect a limited set of laboratory practices – disinfection, cleanliness, conservation, inoculation gesture, timing and recording – you can extend to every French farm a laboratory product made at Pasteur's lab. What was at first a capture of interest by a lab scientist is now extending through a network much like a commercial circuit – not quite since Pasteur sends his doses free of charge – that spreads laboratory products all over France.

But is 'all over France' a social construction? Yes indeed; it is a construction made by statistics-gathering institutions. Statistics is a major science in the nineteenth century, and is what 'Pasteur', now the label for a larger crowd of Pasteurians, is going to use to watch the spread of the vaccine, and to bring to the still uncertain public a fresh and more grandiosely staged proof of the efficacy of the vaccine. Throughout France as it is geographically marked out by its centralized bureaucracy, one can register on beautifully done maps and diagrams the decrease of anthrax wherever the vaccine is distributed. Like an experiment in the Pasteur lab, statisticians inside the office of the agricultural institutions are able to read on the charts the decreasing slopes that mean, so they say, the decrease of anthrax. In a few years, the transfer of the vaccine produced in Pasteur's lab to all farms was recorded in the statistics as the cause of the decline of anthrax. Without these statistical institutions it would of course have been utterly impossible to say whether the vaccine was of any use, as it would have been utterly impossible to detect the existence of the disease to begin with. We have now reached the point we started from. French society, in some of its important aspects, has been transformed through the displacements of a few laboratories.

II. Topology of laboratory positioning

I have chosen one example but many could be found in Pasteur's career and I am confident that every reader has many more of these in mind. The reason why we do not acknowledge these many examples is to be found in the way we treat science. We use a model of analysis that respects the very boundary between micro- and macroscale, between inside and outside, that sciences are designed to not respect. We all see laboratories but we ignore their construction, much like the Victorians who watched kids crawling all over the place, but repressed the vision of sex as the *cause* of this proliferation. We are all prudish in matters of science, social scientists included. Before drawing some general conclusions about laboratories in the third part, let me propose a few concepts that would make us become less prudish and would help to liberate all the information that we cannot help having.

1. Dissolution of the inside/outside dichotomy

Even in the brief outline given above, the example I have chosen is enough to show that, at worst, the categories of inside and outside are totally shaken up and fragmented by lab positioning. But what word can be used that could help us to describe what happened, including this reversion leading to the breaking down of inside/outside dichotomies? I have used several times the words 'translation' or 'transfer', 'displacement' or 'metaphor', words that all say the same thing in Latin, Greek or English (Serres, 1974; Callon, 1975). One thing is sure throughout the story told above: every actor you can think of has been to some extent *displaced* (Armatte, 1981). Pasteur's lab is now in the middle of agricultural interests with which it had no relation before; in the farms an element coming from Paris, vaccine flasks, has been added; veterinary doctors have modified their status by promoting 'Pasteur's' science and the vaccine flasks: they now possess one more weapon in their black bags; and sheep and cows are now freed from a terrible death: they can give more milk and more wool to the farmer and be slaughtered with greater profit. In McNeil's terms (McNeil, 1976), the displacement of microparasites allows the macroparasites – here the farmers – to grow fatter by feeding off healthier cattle. By the same token all the macroparasitic chain of tax collectors, veterinarians, administrators and landlords prosper by feeding off the richer farmers (Serres, 1980). One last element is pushed out – the anthrax bacillus. Wherever the veterinarian comes the small parasite has to go. In this succession of displacements, no one can say *where the laboratory is* and *where the society is*. Indeed the question 'where?' is an irrelevant one when you deal with *displacements* from a lab in Paris to some farms then back to Paris, drawing along with it the microbes and the farmers' interests; then to Pouilly le Fort where an extended repetition is staged, then to the whole agricultural system through statistics and bureaucracy. But it is clear that the situation of the farms after the moves is not the same as before. Through the leverage point of the lab, which is a moment in a dynamic process, the farm system has been displaced. It now includes a routine annual gesture, part of which used to be a laboratory practice and still is a lab product. Everyone has changed, including the 'whole society', to use common terms. This is why I used in the title a parody of Archimedes's famous motto: 'give me a laboratory and I'll move the earth'. This metaphor of the lever to move something else is much more in keeping with observation than any dichotomy between a science and a society. In other words, it is the same set of forces that drives people inside Pasteurian labs to strengthen microbiology and outside to stage the Pouilly le Fort experiment or to modify French agriculture. What we will have to understand later is why in this *moment* the laboratory gains strength to modify the state of affairs of all the other actors.

Another reason why the inside/outside notion is irrelevant, is that in this example the laboratory positions itself precisely so as to reproduce inside its walls an event that seems to be happening only outside – the first move – and then to extend outside to all farms what seems to be happening only inside laboratories. As in some topological theorem, the inside and the outside world can reverse into one another very easily. Naturally, the three relations outside, inside, outside again, are in no way identical. Only a few elements of the macroscopic epizootics are captured in the lab, only controlled epizootics on experimental animals are done in the lab, only specific inoculation gestures and vaccine

inoculant are extracted out of the lab to be spread to farms. That this metaphorical drift, which is made of a succession of displacements and changes of scale (see below p.164), is the source of all innovations is well known (Black, 1961). For our purpose here, it is enough to say that each translation from one position to the next is seen by the 155
captured actors to be a faithful translation and not a betrayal, a deformation or something absurd. For instance, the disease in a Petri dish, no matter how far away from the farm situation, is seen as a faithful translation, indeed *the* interpretation of anthrax disease. The same thing is true when hygienists see as equivalent the trials microbes undergo in Pasteur's lab, and the variations of epidemics that masses of people undergo in a large city like Paris. It is useless trying to decide if these two settings are really equivalent – they are not since Paris is not a Petri dish – but they are deemed equivalent by those who insist that if Pasteur solves his microscale problems the secondary macroscale problem will be solved. The negotiation on the equivalence of non-equivalent situations is always what characterizes the spread of a science, and what explains, most of the time, why there are so many laboratories involved every time a difficult negotiation has to be settled.

For the vaccine to be effective, it has to spread outside in the 'real world out there', as people say. This is what best shows the absurdity of the dichotomy between inside/outside and the usefulness of micro-studies of science in understanding macroissues. Most of the difficulties associated with science and technology come from the idea that there is a time when innovations are in laboratories, and another time when they are tried out in a new set of conditions which invalidate or verify the efficacy of these innovations. This is the 'adequatio rei et intellectus' that fascinates epistemologists so much. As this example shows, the reality of it is more mundane and less mystical.

First, the vaccine works at Pouilly le Fort and then in other places only if in all these places the same laboratory conditions are extended there beforehand. Scientific facts are like trains, they do not work off their rails. You can extend the rails and connect them but you cannot drive a locomotive through a field. The best proof of this is that every time the method of extension of the anthrax vaccine was modified, the vaccine did *not* work and Pasteur got bogged down in bitter controversy, for instance with the Italians (Geison, 1974). His answer was always to check and see if everything was done according to the prescriptions of his lab. That the same thing can be repeated does not strike me as miraculous, but it does seem to be for all the people who imagine that facts get out of laboratories without the extension of lab practices.

But there is a second reason why the laboratories have no outside. The very existence of the anthrax disease in the first place, and the very efficacy of the vaccine at the end of the story, are not 'outside' facts given for all to see. They are, in both cases, the result of the prior 156
existence of statistical institutions having built an instrument (statistics in this case), having extended their network through the whole French administration so as to gather data, and having convinced all the officials that there was a 'disease', a 'terrible' one, and that there was a 'vaccine', an 'efficient' one. Most of the time when we talk about the outside world *we are simply taking for granted the prior extension of a former science* built on the same principle as the one we are studying. This is why lab studies in the end hold the key to the understanding of macro-problems, as I will show at the end of this chapter.

2. *Playing havoc with differences of scale*

But if the inside/outside dichotomy does not hold true, what are we going to say about differences of scale which, the reader should be reminded, are at the origin of many discussions in sociology of science, since it is because of this belief in differences of scale that microstudies are accused of missing some essential points? In the example I sketched out above, we are never confronted with a social context on one hand and a science, laboratory, or individual scientist on the other. We do *not* have a context influencing, or not influencing, a laboratory immune from social forces. This view, which is the dominant view among most sociologists, is exactly what is untenable. Of course, many good scholars like Geison could show why the fact that Pasteur is a Catholic, a conservative, a chemist, a Bonapartist, etc., do count (Farley and Geison, 1979). But this sort of analysis, no matter how careful and interesting, would entirely miss the main point: *in his very scientific work, in the depth of his laboratory, Pasteur actively modifies the society of his time and he does so directly* – not indirectly – *by displacing some of its most important actors.*

Here again Pasteur is a paradigmatic example. As a politician he failed so completely that he was unable to get more than a few votes the few times he tried to get elected senator. But he has along with Carnot, and the Republic itself, the greatest number of streets bearing his name in all French villages and towns. This is also a nice symbol of the studies about Pasteur. If you look for examples of his 'politicking' politics, you will of course find them but they are poor, disappointing, and never in keeping with the importance of his scientific work. The poverty of your findings will make readers say that 'there is something else in Pasteur, in his scientific achievements, that escapes all social or political explanation'. People who would utter this cliché would indeed be right. A poor critical explanation always protects science. This is why the more radical scientists write against science, the more science is mystified and protected.

To study Pasteur as a man acting on society, it is not necessary to search for political drives, for some short-term monetary or symbolic profits or for long-term chauvinistic motives. It is no use looking for unconscious ideologies or devious drives (drives which, by some mystery, are clear only to the analyst's eyes). It is no use muckraking. You just have to look at what he does in his laboratory as a scientist. To summarize a long study in a nutshell (Latour, 1981a), Pasteur adds to all the forces that composed French society at the time a new force for which he is the only credible spokesman – the microbe. You cannot build economic relations without this 'tertium quid' since the microbe, if unknown, can bitter your beer, spoil your wine, make the mother of your vinegar sterile, bring back cholera with your goods, or kill your factotum sent to India. You cannot build a hygienist social movement without it, since no matter what you do for the poor masses crowded in shanty towns, they will still die if you do not control this invisible agent. You cannot establish even innocent relations between a mother and her son, or a lover and his mistress, and overlook the agent that makes the baby die of diptheria and has the client sent to the mad house because of syphilis. You do not need to muckrake or look for distorted ideologies to realize that a group of people, equipped with a laboratory – the only place where the invisible agent is made visible – will easily be situated everywhere in all these relations, wherever the microbe can be seen to intervene. If you reveal microbes as essential actors in all social relations, then you need to make room for them, and for the people who show them and can eliminate them. Indeed the more you want to get rid of the microbes, the more room you should grant Pasteurians. This is not false consciousness, this is not looking for biased world views, this is just what the Pasteurians *did* and the way they were *seen* by all the other actors of the time.

The congenital weakness of the sociology of science is its propensity to look for obvious stated political motives and interests in one of the only places, the laboratories, where sources of fresh politics as yet unrecognized as such are emerging. If by politics you mean elections and law, then Pasteur, as I have said, was not driven by political interests, except in a few marginal aspects of his science. Thus his science is protected from enquiry and the myth of the autonomy of science is saved. If by politics you mean to be the spokesman of the forces you
mould society with and of which you are the only credible and legiti- 158
mate authority, then Pasteur is a fully political man. Indeed, he endows himself with one of the most striking fresh sources of power ever. Who can imagine being the representative of a crowd of invisible, dangerous forces able to strike anywhere and to make a shambles of the present state of society, forces of which he is by definition the only credible interpreter and which only he can control? Everywhere Pasteurian laboratories were established as the only agency able to kill the dangerous actors that were until then perverting efforts to make beer, vinegar, to perform surgery, to give birth, to milk a cow, to keep a regiment healthy and so on. It would be a weak conception of sociology if the reader were only to say that microbiology 'has an influence' or 'is influenced by the nineteenth-century social context'. *Microbiology laboratories are one of the few places where the very composition of the social context has been metamorphosed.* It is not a small endeavour to transform society so as to include microbes and microbe-watchers in its very fabric. If the reader is not convinced, then he can compare the sudden moves made at the same time by socialist politicians, talking on behalf of another crowd of new, dangerous, undisciplined and disturbing forces for whom room should be made in society: the labouring masses. The two powers are comparable in this essential feature: they are fresh sources of power for modifying society and cannot be explained by the state of the society at the time. Although the two powers were mixed together at the time (Rozenkranz, 1972), it is clear that in political terms the influence of Pasteurian laboratories reached further, deeper, and more irreversibly since they could intervene in the daily details of life – spitting, boiling milk, washing hands – and at the macroscale – rebuilding sewage systems, colonizing countries, rebuilding hospitals – without ever being clearly seen as a stated political power.

This transformation of what is the very composition of society can in no way be defined through distinctions of scales and of levels. Neither the historian nor the sociologist can distinguish the macrolevel of French society and the microlevel of the microbiology laboratory, since the latter is helping to redefine and displace the former. The laboratory positioning, as I insisted on earlier, was in no way inevitable. Pasteur could have failed to link his work on microbes to his many clients' interests. Had he failed, then I agree that the distinction of levels would hold true: there would indeed be French agricultural, medical, social, political interests on the one hand, and the insulated laboratory of a disinterested scientist at the École Normale Supérieure
on the other. Claude Bernard had such a laboratory. But this was in no 159
way Pasteur's strategy, and still less that of the larger Institut Pasteur,

which was always situated in such a way that all the interested commercial, colonial, and medical interests had to pass through their laboratories to borrow the technics, the gestures, the products, the diagnostic kits that were necessary to further their own desires. Laboratories were set up everywhere: on the front line during the first world war in the trenches they largely made possible; before the colonists arrived in the tropics, allowing the very survival of the white colonists and their soldiers; in the surgery ward that was transformed from a teaching amphitheatre into a laboratory (Salomon-Bayet, 1982); in the plants of the food industries in many public health services; inside the small offices of general practitioners; in the midst of farms, and so on. Give us laboratories and we will make possible the Great War without infection, we will open tropical countries to colonization, we will make France's army healthy, we will increase the number and strength of her inhabitants, we will create new industries. Even blind and deaf analysts will see these claims as 'social' activity, but on condition that laboratories are considered places where society and politics are renewed and transformed.

III. How the weakest becomes the strongest

What I have said about the example treated in Part I now leads us to the more general problem of laboratory practice and of the relevance of microstudies for understanding the 'large-scale' problems raised by the field known as Science, Technology and Society (STS). If I were to summarize the argument presented in Part II I could say that a sociology of science hamstrings itself from the start: if, that is, it takes for granted the difference of levels or of scale between the 'social context' on the one hand and the laboratory or the 'scientific level' on the other; and if it fails to study *the very content* of what is being done inside the laboratories. I claim that, on the contrary, laboratories are among the few places where the differences of scale are made irrelevant and where the very content of the trials made within the walls of the laboratory can alter the composition of society. The methodological consequence of this argument is, of course, that we were right in starting with on-the-spot laboratory studies and looking for a sociology of the *contents* of science (Latour and Woolgar, 1979). It is not only the key to a sociological understanding of science that is to be found in lab studies, it is also, I believe, the key to a sociological understanding of society itself, since it is in laboratories that most new sources of power are generated. Sociology of science cannot always be borrowing from sociology or social history the categories and concepts to reconstruct the 'social context' inside which science should be understood. On the contrary, it is time for sociology of science to show sociologists and social historians how societies are displaced and reformed with and through the very contents of science. But to do so, sociologists of scientific practice should avoid being shy and sticking only to the level of the laboratory (for this level does not exist) and being proud of diving inside laboratory walls, because laboratories are the places where the inside/outside relations are reversed. In other words, since laboratory practices lead us constantly inside/outside and upside/down, we should be faithful to our field and follow our objects throughout all their transformations. This is simply good methodology. But to do so without getting dizzy, we should understand in more detail the strange topology that laboratory practices present.

The most difficult problem for understanding this positioning of laboratory practice is to define precisely why it is that in the laboratory and only there new sources of strength are generated. Using the metaphor of the lever, why is a laboratory a solid lever and not a soft straw? In asking this question we are back to the problem of understanding what has been achieved through microstudies of science. Many answers were given by epistemologists before lab studies started pouring in. It was said that scientists had special methods, special minds, or in more culturalist forms of racism, some kind of special culture. It was always in something 'special', usually of a cognitive quality, that this source of strength was explained. Of course, the moment sociologists walked into laboratories and started checking all these theories about the strength of science, they just disappeared. Nothing special, nothing extraordinary, in fact nothing of any cognitive quality was occurring there. Epistemologists had chosen the wrong objects, they looked for mental aptitudes and ignored the material local setting, that is, laboratories. The same thing happened with most of the so-called Mertonian sociology. No special sociological relations could explain anything about the strength of science. The 'norms' faded away like the 'invisible college' and the 'precapitalist recognition of debt', and went into the limbo where 'falsification', and the 'angels' sexes' are put for a well-deserved eternal rest. The first sociologists made the same mistake as the epistemologists. They looked for something special everywhere except in the most obvious and striking place: the settings. Even scientists themselves are more aware of what makes them special than many analysts. Pasteur, for instance, a better sociologist and epistemologist than most, wrote a kind of treatise on sociology of science simply pointing to the laboratory as the cause of the strength gained by scientist over society (Pasteur, 1871).

Laboratory studies have been successful, but so far only in the negative sense of dissipating previous beliefs surrounding science. Nothing special is happening in the cognitive and in the social aspect of laboratory practice. Knorr-Cetina has reviewed this (this volume, ch. 5) and there is nothing much else to add, nothing except that we now have to explain what happens in laboratories that makes them such an irreplaceable source of political strength, strength which is *not* explained by any cognitive or social peculiarities.

In earlier work (Latour and Fabbri, 1977; Latour and Woolgar, 1979), I have indicated a line of enquiry to answer this most tricky of all questions. This approach can be summed up by the sentence: *look at the inscription devices*. No matter if people talk about quasars, gross national products, statistics on anthrax epizootic microbes, DNA or subparticle physics; the only way they can talk and not be undermined by counter-arguments as plausible as their own statements is if, and only if, they can make the things they say they are talking about easily readable. No matter the size, cost, length, and width of the instruments they build, the final end product of all these inscription devices is always a written trace that makes the perceptive judgment of the others *simpler*. The race for the invention of these inscription devices and for the simplification of the inscriptions provided leads either to simple forms (dots, bands, peaks and spots) or, even better, to another written text directly readable on the surface of the inscription. The result of this exclusive interest in inscriptions is a text that limits the number of counter-arguments by displaying, for each difficult displacement, one of these simplified inscriptions (diagrams, tables, pictures). The purpose of the construction of this double text that includes arguments and inscriptions is to alter the modalities a reader could add to the statements. Moving a modality from 'it is probable that A is B', to 'X has shown that A is B', is enough to obtain a scientific 'fact' (Latour and Woolgar, 1979: ch. 2).

This kind of enquiry had the immense advantage of revealing special features of the laboratory – obsession for inscription devices and writing specific types of texts – which left the rest of the setting completely ordinary. To take up Feyerabend's saying: 'in the laboratory anything goes, except the inscription devices and the papers'.

162 Scientific fact is the product of average, ordinary people and settings, linked to one another by no special norms or communication forms, but who work with inscription devices. This argument which at first appeared reductionist and too simple has since received much more support and is now well established. Semiotics (Bastide, 1981) has demonstrated how far one can go in the content of science by looking at this matter of the text itself, but it is from cognitive anthropology, cognitive psychology, and history of science that stronger support is now coming. The technology of inscribing (writing, schooling, printing, recording procedures) is seen by more and more analysts as the main cause of what was attributed in earlier times to 'cognitive' or 'vague cultural' phenomena. The books of Jack Goody (1977), and above all of Elizabeth Eisenstein (1979), show well the extraordinary fecundity of looking at this material level that had escaped the attention of epistemologists, historians, sociologists and anthropologists alike because inscription technology seemed to them to be too obvious and too 'light'. This mysterious thinking process that seemed to float like an inaccessible ghost over social studies of science at last has flesh and bones and can be thoroughly examined. The mistake before was to oppose heavy matter (or 'large-scale' infrastructures like in the first 'materialist' studies of science) to spiritual, cognitive or thinking processes instead of focusing on the most ubiquitous and lightest of all materials: the written one (Havelock, 1981; Dagognet, 1973).

But if we accept this approach, are we not back again to the micro-level and far from the macroconcerns of all the other analysts in STS, preoccupied by serious things like disarmament, technology transfer, sociology of innovation or history of science? Looking at the inscriptions is interesting one could say, but it leaves us with a long way to go to explain how the strength is gained in laboratories to transform or displace societies. This is precisely why the first laboratory study I made was weak; it was weak for a simple methodological reason. I focused on one laboratory, taking for granted its existence as a unit and its relevance to the outside. So I had no occasion to watch the most puzzling procedure of all, how a set of inscription procedures are made to be relevant to issues which at first sight seem utterly foreign and much too grandiose, complicated or disorderly ever to end up on the top of a desk in a few easily read diagrams and charts discussed quietly by a few white-coated PhDs. The last point of this chapter will be to formulate, thanks to Pasteur's strategy, the simple answer to this puzzle, so simple indeed that it had escaped my attention.

The answer is visible if we bring together the three threads of my
163 argument: the dissolution of the inside/outside boundary; the inversion of scales and levels; and finally the process of inscription. These three themes point to the same problem: how a few people gain strength and go inside some places to modify other places and the life of the multitudes. Pasteur, for instance, and his few collaborators cannot tackle the anthrax problem by moving all over France and gathering an intimate

knowledge of all the farms, farmers, animals and local idiosyncrasies. The only place where they are able and good workers is in their laboratory. Outside they are worse at farming than the farmers and worse at veterinary medicine than the veterinarians. But they are expert inside their own walls at setting up trials and instruments so that the invisible actors – which they call microbes – show their moves and development in pictures so clear that even a child would see them. The invisible becomes visible and the 'thing' becomes a written trace they can read at will as if it were a text. This expertise, in their case, is already obtained by a complete modification of the scale. As has been previously explained, the microbe is invisible as long as it is not cultivated in isolation from its other competitors. As soon as it grows uninhibited on an aptly chosen medium, it grows exponentially and makes itself large enough to be counted as small dots on the Petri dish. I don't know what a microbe is, but counting dots with clear-cut edges on a white surface is simple. The problem now is to link this expertise to the health field. I showed the solution earlier by these three-pronged movements that displace the laboratory. The consequence is clear. By these moves an epizootic occurs inside the laboratory walls that is deemed relevant to the macroproblems outside. Again the scale of the problem is reversed, but this time it's the 'macro' that is made small enough to be dominated by the Pasteurians. Before this displacement and inversion that allowed Pasteurians to hook an expertise in setting up inscription devices onto the health field, no one had ever been able to master the course of an epidemic. This 'mastery' means that each event – the inoculation, the outbreak of an epidemic, the vaccination, the counting of the dead and of the living, the timing, the places – becomes entirely readable by a few men who could agree among themselves because of the simplicity of each perceptive judgment they were able to make about simple diagrams and curves.

The strength gained in the laboratory is not mysterious. A few people much weaker than epidemics can become stronger if they change the scale of the two actors – making the microbes big, and the epizootic small – and others dominate the events through the inscription devices that make each of the steps readable. The change of scale entails an acceleration in the number of inscriptions you can get. Obtaining data on anthrax epidemics on the scale of France was a slow, painstaking, and uncertain process. But in a year Pasteur could multiply anthrax outbreaks. No wonder that he became stronger than veterinarians. For every statistic they had, he could mobilize ten of them. Before Pasteur, their statements could be interrupted by any number of other statements just as plausible as theirs. But when Pasteur comes out of his lab with this many figures who is able to mount a serious attack against him? Pasteur has gained strength simply by modifying the scale. So, in discussions about anthrax, Pasteur has two sources of strength: the epizootic and the microbes. His opponents and predecessors had to work 'outside' on a 'large scale', constantly stabbed in the back haphazardly by the invisible agent that made their statistics look random. But Pasteur, by building his laboratory and inserting it in the farms as we have seen, dominates the microbe – that he made bigger – and the epizootic – that he made smaller – and multiplies the experiments at small cost *without leaving his laboratory*. This concentration of forces makes him so much stronger than his competitors that they cannot even think of a counter-argument except in the few cases where, like Koch, they are equipped as well as he is.

To understand the reason why people pay so much for laboratories which are actually ordinary places, one just has to consider these places as nice technological devices to invert the hierarchy of forces. Thanks to a chain of displacements – both of the laboratory and of the objects – the scale of what people want to talk about is modified so as to reach this best of all possible scales: the inscription on a flat surface written in simple forms and letters. Then everything they have to talk about is not only visible, but also readable, and can be easily pointed at by a few people who by doing this dominate. This is as simple and as sufficient as Archimedes's point about moving the earth and making the weakest the strongest. It is simple indeed because making simple moves is what this device is about. 'Accumulated knowledge' people say with admiration, but this acceleration is made possible by a change of scale, which in turn makes possible the multiplication of trials and errors. Certainty does not increase in a laboratory because people in it are more honest, more rigorous, or more 'falsificationist'. It is simply that they can make as many mistakes as they wish or simply more mistakes than the others 'outside' who cannot master the changes of scale. Each mistake is in turn archived, saved, recorded, and made easily readable again, whatever the specific field or topic may be. If a great many trials are recorded and it is possible to make a sum of their inscriptions, that sum will always be more certain if it decreases the possibility of a competitor raising a statement as plausible as the one you are defending. That is enough. When you sum up a series of mistakes, you are stronger than anyone who has been allowed fewer mistakes than you.

This vision of the laboratory as a technological device to gain strength by multiplying mistakes, is made obvious if one looks at the difference between a politician and a scientist. They are typically contrasted on cognitive or social grounds. The first is said to be greedy, full of self-interest, short-sighted, fuzzy, always ready to compromise, and shaky. The second is said to be disinterested, far-sighted, honest, or at least rigorous, to talk clearly and exactly and to look for certainty. These many differences are all artificial projections of one, simple, material thing. The politician has no laboratory and the scientist has one. So the politician works on a full scale, with only one shot at a time, and is constantly in the limelight. He gets by, and wins or loses 'out there'. The scientist works on scale models, multiplying the mistakes inside his laboratory, hidden from public scrutiny. He can try as many times as he wishes, and comes out only when he has made all the mistakes that have helped him gain 'certainty'. No wonder that one does not 'know' and the other 'knows'. The difference, however, is not in 'knowledge'. If you could by chance reverse the positions, the same greedy, short-sighted politician, once in a laboratory, is going to churn out exact scientific facts, and the honest, disinterested, rigorous, scientist put at the helm of a political structure that is full scale and with no mistakes allowed will become fuzzy, uncertain and weak like everyone else. The specificity of science is not to be found in cognitive, social or psychological qualities, but in the special construction of laboratories in a manner which reverses the scale of phenomena so as to make things readable, and then accelerates the frequency of trials, allowing many mistakes to be made and registered.

That the laboratory setting is the cause of the strength gained by scientists is made still clearer when people want to establish elsewhere conclusions as certain as those reached in the laboratory. As I have shown above, it can be said that there is no outside to laboratories. The best thing one can do is to extend to other places the 'hierarchy of forces' that was once favourable inside the first laboratory. I showed

this for anthrax but it is a general case. The mystification of science comes most often from the idea that scientists are able to make 'predictions'. They work in their labs and, sure enough, something happens outside that verifies these predictions. The problem is that no one has ever been able to verify these predictions without extending first the conditions of verification that existed in the laboratory. The 166
vaccine extends on the condition that farms are transformed into an annex of Pasteur's lab and that the very statistical system that made anthrax visible in the first place is used to verify if the vaccine had any effect. We can watch the extension of laboratory conditions, and the repetition of the final trial that was favourable, but we cannot watch predictions of scientists extending themselves beyond laboratory walls (Latour and Woolgar, 1979: ch. 4).

If this seems counter-intuitive to the reader, a little reasoning will convince him that every counter-example he can think of in fact conforms to the position stated here. No one has ever seen a laboratory fact move outside unless the lab is first brought to bear on an 'outside' situation and that situation is transformed so that it fits laboratory prescriptions. Every counter-example is a belief that such a thing is possible. But a belief is not a proof. If the proof is given then the two conditions I stated will always be verified. My confidence in this answer is not based on presumption but on a simple scientific belief, shared by all my fellow scientists, that magic is impossible and that action at a distance is always a misrepresentation. Scientists' predictions or previsions are always post-dictions or repetitions. The confirmation of this obvious phenomenon is shown in scientific controversies when scientists are forced to leave the solid ground of their laboratories. The moment they really get 'outside' they know nothing, they bluff, they fail, they get by, they lose all possibility to say anything that is not immediately counter-attacked by swarms of equally plausible statements.

The only way for a scientist to retain the strength gained inside his laboratory by the process I have described is not to go outside where he would lose it at once. It is again very simple. The solution is in *never going out*. Does that mean that they are stuck in the few places where they work? No. It means that they will do everything they can to extend to every setting some of the conditions that make possible the reproduction of favourable laboratory practices. Since scientific facts are made inside laboratories, in order to make them circulate you need to build costly networks inside which they can maintain their fragile efficacy. *If this means transforming society into a vast laboratory, then do it.* The spread of Pasteurian laboratories to all the places that a few decades before had nothing to do with science is good example of this network building. But a look at systems of Standard Weights and Measures, called 'métrologie' in French, is still more convincing. Most of the work done in a laboratory would stay there for ever if the principal physical constants could not be made constant everywhere 167
else. Time, weight, length, wavelength, etc., are extended to ever more localities and in ever greater degrees of precision. Then and only then, laboratory experiments can be brought to bear on problems occurring in factories, the tool industry, economics or hospitals. But if you just try in a thought experiment to extend the simplest law of physics 'outside', without first having extended and controlled all the main constants, you just could not verify it, just as it would have been impossible to know the existence of anthrax and to see the efficacy of the vaccine without the health statistics. This transformation of the

whole of society according to laboratory experiments is ignored by sociologists of science.

There is no outside of science but there are long, narrow networks that make possible the circulation of scientific facts. Naturally the reason for this ignorance is easy to understand. People think that the universality of science is a given, because they forget to take into account the size of the 'métrologie'. Ignoring this transformation that makes all displacements possible is like studying an engine without the railway or the freeway networks. The analogy is a good one since the seemingly simple work of maintaining the physical constants constant in a modern society is evaluated to be three times more than the effort of all the science and technology themselves (Hunter, 1980). The cost of making society conform to the inside of laboratories so that the latter's activity can be made relevant to the society is constantly forgotten, because people do not want to see that universality is a social construction as well (Latour, 1981b).

Once all these displacements and transformations are taken into account, the distinction between the macrosocial level and the level of laboratory science appears fuzzy or even non-existent. Indeed, laboratories are built to destroy this distinction. Once it is dissolved, a few people can inside their insulated walls work on things that can change the daily life of the multitudes. No matter if they are economists, physicists, geographers, epidemiologists, accountants, microbiologists, they make all the other objects on such a scale – maps, economic models, figures, tables, diagrams – that they can gain strength, reach incontrovertible conclusions, and then extend on a larger scale the conclusions that seem favourable to them. *It is* a political process. *It is not* a political process. It is since they gain a source of power. It is not since it is a source of fresh power that escapes the routine and easy definition of a stated political power. 'Give me a laboratory and I will move society', I said, parodying Archimedes. We now know why a
168 laboratory is such a good lever. But if I now parody Clausewitz's motto, we will have a more complete picture: 'science is politics pursued by other means'. It is not politics since a power is always blocked by another counter-power. What counts in laboratory sciences are the other means, the fresh, unpredictable sources of displacements that are all the more powerful because they are ambiguous and unpredictable. Pasteur, representing the microbes and displacing everyone else, is making politics, but by other, unpredictable means that force everyone else out, including the traditional political forces. We can now understand why it was and is so important to stick to laboratory microstudies. In our modern societies most of the really fresh power comes from sciences – no matter which – and not from the classical political process. By staking all social explanations of science and technology on the classical view of politics and economics – profit, stated power, predictable evils or goods – analysts of science who claim to study the macrolevels fail to understand precisely what is strong in science and technology. In speaking of scientists who make politics by other means, their boring and repetitive critique is always that they 'just make politics', period. Their explanation falls short. The shortness of it is in the period – they stop where they should start. Why though are the means different? To study these other means, one must get inside the contents of the sciences, and inside the laboratories where the future reservoirs of political power are in the making. The challenge of laboratories to sociologists is the same as the challenge of laboratories to society. They can displace society and recompose it by the very

content of what is done inside them, which seemed at first irrelevant or too technical. The careful scrutiny of laboratory scientists cannot be ignored and no one can jump from this 'level' to the macropolitical level since the latter gets all its really efficient sources of power from these very laboratories that have just been deemed uninteresting or too technical to be analyzed.

But we can also understand why students of laboratory practices should not be shy and accept a vision of their own method that would limit them to the laboratory, whereas the laboratory is just a moment in a series of displacements that makes a complete shambles out of the inside/outside and the macro/micro dichotomies. No matter how divided they are on sociology of science, the macroanalysts and the microanalysts share one prejudice: *that science stops or begins at the laboratory walls*. The laboratory is a much trickier object than that, it is a much more efficient transformer of forces than that. That is why by remaining faithful to his method, the microanalyst will end up tackling macroissues as well, exactly like the scientist doing lab experiments on microbes who ends up modifying many details of the whole of French society. Indeed, I think an argument could be made to show that the existence of the macrolevel itself, the famous 'social context', is a consequence of the development of many scientific disciplines (Callon and Latour, 1981). It is already clear to me that this is the only way that sociology of science can be rebuilt in keeping with the constraints now set by laboratory studies. I also think that it is one of the few ways that sociology of science can teach something to sociology instead of borrowing from it categories and social structures that the simplest laboratory is destroying and recomposing. It would be high time, since the laboratory is more innovative in politics and in sociology than most sociologists (including many sociologists of science). We are only just starting to take up the challenge that laboratory practices present for the study of society.

References

Armatte, Michel (1981) *Ca marche, les traductions de l'homme au travail, Mémoire de DEA*, Paris: CNAM-STS.

Bastide, Françoise (1981) 'Le Foie Lavé, analyse sémiotique d'un texte scientifique', *Le Bulletin*, 2: 35–82.

Black, Max (1961) *Models and Metaphors*, Ithaca, NY: Cornell University Press.

Callon, Michel (1975) 'Les Opérations de Traductions', in P. Roqueplo (ed.), *Incidence des Rapports Sociaux sur le Développement Scientifique*, Paris: CNRS.

Callon, Michel (1981) 'Struggles and Negotiations to Define What is Problematic and What is Not: The Sociologic Translation', in K. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook*, vol. 4, Dordrecht: D. Reidel.

Callon, Michel (1982) 'La Mort d'un Laboratoire Saisi par l'Aventure Technologique' (in preparation).

Callon, Michel and Latour, Bruno (1981) 'Unscrewing the Big Leviathan, or How do Actors Macrostructure Reality?', in K. D. Knorr-Cetina and A. Cicourel (eds), *Advances in Social Theory and Methodology Toward an Integration of Micro- and Macro-Sociologies*, London: Routledge and Kegan Paul.

Collins, H. M. (1975) 'The Seven Sexes: A Study in the Sociology of a Phenomenon or the Replication of Experiments in Physics', *Sociology*, 9 (2): 205–24.

Collins, H. M. (1982) 'Stages in the Empirical Programme of Relativism', *Social Studies of Science*, 11 (1): 3–10.

Dagognet, François (1973) *Ecriture et Iconographie*, Paris: Vrin.

Eisenstein, Elizabeth (1979) *The Printing Press as an Agent of Change*, Cambridge: Cambridge University Press.

Farley, John and Geison, Gerald (1974) 'Science, Politics and Spontaneous Generation in 19th Century France: The Pasteur-Pouchet Debate', *Bulletin of the History of Medicine*, 48 (2): 161–98.

Geison, Gerald (1974) 'Pasteur', in G. Gillispie (ed.), *Dictionary of Scientific Biography*, New York: Scribners.

Goody, Jack (1977) *The Domestication of the Savage Mind*, Cambridge: Cambridge University Press.

Havelock, Eric A. (1981) *Aux Origines de la Civilisation Ecrite en Occident*, Paris: Maspéro.

Hunter, J. S. (1980) 'The National System of Scientific Measurement', *Science*, 210: 869–75.

Knorr-Cetina, K. D. (1981) *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Oxford: Pergamon Press.

Knorr-Cetina, K. D. and Cicourel, A. (eds) (1981) *Advances in Social Theory: Toward an Integration of Micro- and Macro-Sociologies*, London: Routledge and Kegan Paul.

Latour, Bruno (1981a) 'Qu'est-ce qu'être Pastorien?' (in preparation).

Latour, Bruno (1981b) *Irréductions: Tractatus Scientifico-Politicus*, Paris: Chezloteur.

Latour, Bruno and Fabbri, Paolo (1977) 'Pouvoir et Devoir dans un Article de Sciences Exactes', *Actes de la Recherche*, 13: 82–95.

Latour, Bruno and Woolgar, Steve (1979) *Laboratory Life: The Social Construction of Scientific Facts*, London and Beverly Hills: Sage.

Leonard, Jacques (1977) *La Vie Quotidienne des Médecins de L'Ouest au 19° Siècle*, Paris: Hachette.

Lynch, Michael (1982) *Art and Artefact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*, London: Routledge and Kegan Paul.

McNeil, John, (1976) *Plagues and People*, New York: Doubleday.

Nelkin, Dorothy (ed.) (1979) *Controversy, Politics of Technical Decisions*, London and Beverly Hills: Sage.

Pasteur, Louis (1871) *Quelques Réflexions sur la Science en France*, Paris.

Rosenkranz, Barbara (1972) *Public Health in the State of Massachusetts 1842-1936, Changing Views*, Harvard: Harvard University Press.

Salomon-Bayet, Claire (1982) 'La Pasteurisation de la Médecine Française' (in preparation).

Serres, Michel (1974) *Hermès III, La Traduction*, Paris: Editions de Minuit.

Serres, Michel (1980) *Le Parasite*, Paris: Grasset.

Woolgar, Steve (1981) 'Interests and Explanation in the Social Study of Science', *Social Studies of Science*, 11 (3): 365–94.

Karin D. Knorr-Cetina and Michael Mulkay (eds), *Science observed: perspectives on the social studies of science*, Sage Publications, London, 1983, pp. 141-170.

1a

THE PROBLEM OF EXPERIMENT

Steven Shapin

BOYLE'S LITERARY TECHNOLOGY

1a.1(a)

The Mechanics of Fact-Making 483

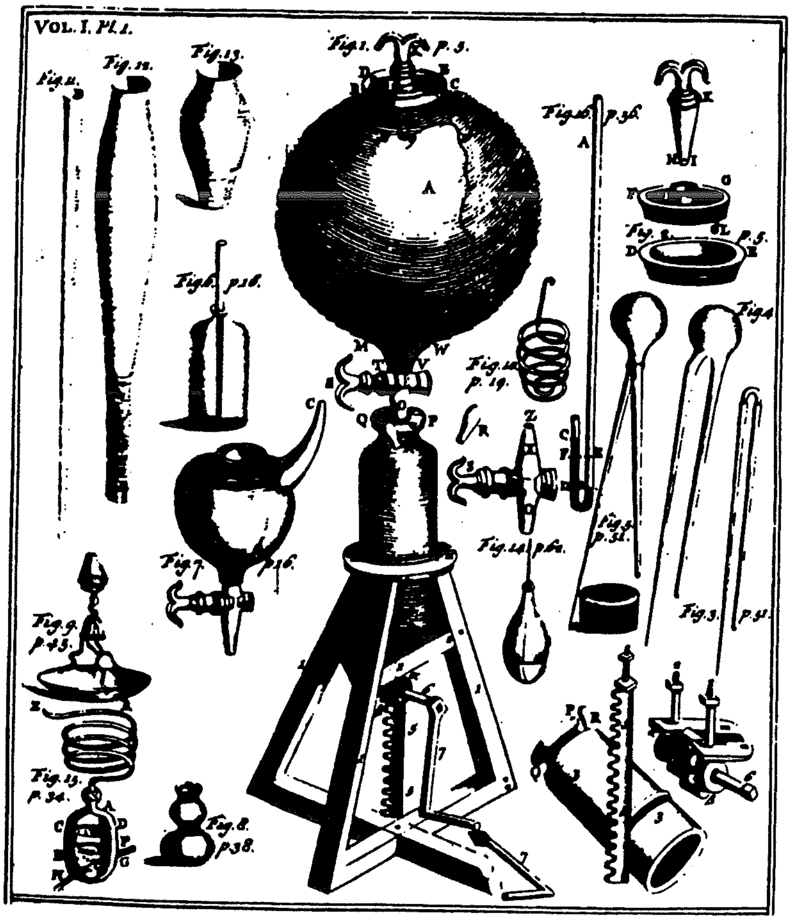
Boyle proposed that matters of fact be generated by a multiplication of the witnessing experience. An experience, even of an experimental performance, that was witnessed by one man alone 484 was not a matter of fact. If that witness could be extended to many, and in principle to all men, then the result could be constituted as a matter of fact. In this way, the matter of fact was at once an epistemological and a social category. The foundational category of the experimental philosophy, and of what counted as properly grounded knowledge generally, was an artefact of communication and of whatever social forms were deemed necessary to sustain and enhance communication. I argue that the establishment of matters of fact utilized three technologies: a *material technology* embedded in the construction and operation of the air-pump; a *literary technology* by means of which the phenomena produced by the pump were made known to those who were not direct witnesses; and a *social technology* which laid down the conventions natural philosophers should employ in dealing with each other and considering knowledge-claims.[6] Given the concerns of this paper, I shall be devoting most attention to Boyle's literary technology: the expository means by which matters of fact were established and assent mobilized. Yet the impression should not be given that we are dealing with three distinct technologies: each embedded the others. For example, experimental practices employing the material technology of the air-pump crystallized particular forms of social organization; desired forms of social organization were dramatized in the exposition of experimental findings; the literary reporting of air-pump performances provided an experience that was said to be essential to the propagation of the material technology or even to be a valid substitute for direct witness. In studying Boyle's literary technology we are not, therefore, talking about something which is merely a 'report' of what was done elsewhere; we are dealing with a most important form of experience and the means for extending and validating experience.

The Material Technology of the Air-Pump

We start by noting the obvious: Boyle's matters of fact were *machine-made*. In his terminology, performances using the air-pump counted as 'unobvious' or 'elaborate' experiments, contrasted to either the 'simple' observation of nature or the 'obvious' experiments involved in reflecting upon common artefacts like the gardener's watering-pot.[7] The air-pump (or

485

Figure 1
Boyle's Air Pump of 1660



1. R. Boyle, 'New Experiments Physico-Mechanical, touching the Spring of the Air . . .', in Boyle, *Works*, ed. T. Birch, 6 Vols. (London, 1772), Vol. I, 1-117. (All subsequent references to Boyle's writings are to this edition and will be cited as *RBW*.)

486 'pneumatic engine') constructed for Boyle in 1659 (largely by Robert Hooke) was indeed an elaborate bit of scientific machinery (see Figure 1).[8] It consisted of a glass 'receiver' of about 30-quarts volume, connected to a brass 'cylinder' ('3') within which plied a wooden piston or 'sucker' ('4'). The aim was to evacuate the receiver of atmospheric air and thus to achieve a working vacuum. This was done by manually operating a pair of valves: on the downstroke, valve 'S' (the stop-cock) was opened and valve 'R' was inserted; the sucker was then moved down by means of a rack-and-pinion device ('5' and '7'). On the upstroke, the stop-cock was closed, the valve 'R' removed, and a quantity of air drawn into the cylinder was expelled. This operation was repeated many times until the effort of moving the sucker became too great, at which point a working vacuum was deemed to have been attained. Great care had to be taken to ensure that the pump was sealed against leakage, for example at the juncture of receiver and cylinder and around the sides of the sucker. Experimental apparatus could be placed into the receiver through an aperture at the top of the receiver ('B-C'), for instance a barometer or simple Torricellian apparatus. The machine was then ready to produce matters of fact. Boyle used the pump to generate phenomena which he interpreted in terms of 'the spring of the air' (its elasticity) and the weight of the air (its pressure).

Boyle's air-pump was, as he said, an 'elaborate' device; it was also temperamental (difficult to operate properly) and very expensive: the air-pump was seventeenth-century 'Big Science'. To finance its construction on an individual basis it helped mightily to be a son of the Earl of Cork. Other natural philosophers, almost as well supplied with cash, shied away from the cost of having one built, and a major justification for founding scientific societies in the 1660s and afterwards was the collective financing of the instruments upon which the experimental philosophy was deemed to depend. Air-pumps were not widely distributed in the 1660s. They were scarce commodities: Boyle's original machine was quickly presented to the Royal Society of London; he had one or two re-designed instruments built for him by 1662, operating mainly in Oxford; Christiaan Huygens had one made in The Hague in 1661; there was one at the Montmor Academy in Paris; there was probably one at Christ's College, Cambridge by the mid-1660s, and Henry Power may have possessed one in Halifax from 1661. So far as can be found out, these were all the air-pumps that existed in the decade after their invention.[9]

Thus, air-pump technology posed a problem of access. If knowledge was to be produced using this technology, then the numbers of philosophers who could produce it were limited. Indeed, in Restoration England this restriction was one of the chief recommendations of 'elaborate' experimentation: knowledge could no longer legitimately be generated by alchemical 'secretists' and sectarian 'enthusiasts' who claimed individual and unmediated inspiration from God. Experimental knowledge was to be tempered by collective labour and disciplined by artificial devices. The very intricacy of machines like the air-pump allowed philosophers, it was said, to discern which cause, amongst the many possible, might be responsible for observed effects. This was something, in Boyle's view, that the gardener's pot could not do.[10] However, access to the machine had to be opened up if knowledge-claims were not to be regarded as mere individual opinion and if the machine's matters of fact were not to be validated on the bare say-so of an individual's authority. How was this special sort of access to be achieved?

Witnessing Science

In Boyle's programme the capacity of experiments to yield matters of fact depended not only upon their actual performance but essentially upon the assurance of the relevant community that they had been so performed. He therefore made an important distinction between actual experiments and what are now termed 'thought experiments'.[11] If knowledge was to be empirically based, as Boyle and other English experimentalists insisted it should, then its experimental foundations had to be attested to by eye-witnesses. Many phenomena, and particularly those alleged by the alchemists, were difficult to credit; in which cases Boyle averred 'that they that have seen them can much more reasonably believe them, than they that have not.'[12] The problem with eye-witnessing as a criterion for assurance was one of discipline. How did one police the reports of witnesses so as to avoid radical individualism? Was one obliged to credit a report on the testimony of any witness whatever?

Boyle insisted that witnessing was to be a collective enterprise. In natural philosophy, as in criminal law, the reliability of testimony depended crucially upon its multiplicity:

488 For, though the testimony of a single witness shall not suffice to prove the accused party guilty of murder; yet the testimony of two witnesses, though but of equal credit . . . shall ordinarily suffice to prove a man guilty; because it is thought reasonable to suppose, that, though each testimony single be but probable, yet a concurrence of such probabilities, (which ought in reason to be attributed to the truth of what they jointly tend to prove) may well amount to a moral certainty, i.e. such a certainty, as may warrant the judge to proceed to the sentence of death against the indicted party.[13]

And Thomas Sprat, defending the reliability of the Royal Society's judgements in matters of fact, inquired

> whether, seeing in all Countreys, that are govern'd by Laws, they expect no more, than the consent of two, or three witnesses, in matters of life, and estate; they will not think, they are fairly dealt withall, in what concerns their *Knowledg*, if they have the concurring Testimonies of *threescore or an hundred.*[14]

The thrust of the legal analogy should not be missed. It was not just that one was multiplying authority by multiplying witnesses (although this was part of the tactic); it was that right *action* could be taken, and seen to be taken, on the basis of these collective testimonies. The action concerned the positive giving of assent to matters of fact. The multiplication of witness was an indication that testimony referred to a true state of affairs in nature. Multiple witnessing was counted as an active, and not just a descriptive, licence. Does it not force the conclusion that such and such an action was done (a specific trial), and that subsequent action (offering assent) was warranted?

In experimental practice one way of securing the multiplication of witnesses was to perform experiments in a social space. The 'laboratory' was contrasted to the alchemist's closet precisely in that the former was said to be a public and the latter a private space. The early air-pump trials were routinely performed in the Royal Society's ordinary public rooms, the machine being brought there specially for the occasion.[15] In reporting upon his experimental performances Boyle commonly specified that they were 'many of them tried in the presence of ingenious men', or that he made them 'in the presence of an illustrious assembly of virtuosi (who were spectators of the experiment).'[16] Boyle's collaborator Robert Hooke worked to codify the Society's procedures for the standard recording of experiments: the register was 'to be sign'd by a certain Number of the Persons present, who have been present, and Witnesses of all the said Proceedings, who, by Sub-scribing
489 their Names, will prove undoubted Testimony . . .'[17] And Sprat described the role of the 'Assembly' in 'resolv[ing] upon the matter of *Fact*' by collectively correcting individual idiosyncracies of observation and judgement.[18] In reporting experiments that were particularly crucial or problematic, Boyle named his witnesses and stipulated their qualifications. Thus, the experiment of the original air-pump trials that was 'the principal fruit I promised myself from our engine' was conducted in the presence of 'those excellent and deservedly famous Mathematic Professors, Dr *Wallis,* Dr *Ward,* and Mr *Wren* . . ., whom I name, both as justly counting it an honour to be known to them, and as being glad of such judicious and illustrious witnesses of our experiment . . .' Another important experiment was attested to by Wallis 'who will be allowed to be a very competent judge in these matters.' And in his censure of the alchemists Boyle generally warned natural philosophers not 'to believe chymical experiments . . . unless he, that delivers that, mentions his doing it upon his own particular knowledge, or upon

the relation of some credible person, avowing it upon his own experience.' Alchemists were recommended to name the putative author of these experiments 'upon whose credit they relate' them.[19] The credibility of witnesses followed the taken-for-granted conventions of that setting for assessing individuals' reliability and trustworthiness: Oxford professors were accounted more reliable witnesses than Oxfordshire peasants. The natural philosopher had no option but to rely for a substantial part of his knowledge on the testimony of witnesses; and, in assessing that testimony, he (no less than judge or jury) had to determine their credibility. This necessarily involved their moral constitution as well as their knowledgeableness, 'for the two grand requisites, of a witness [are] the knowledge he has of the things he delivers, and his faithfulness in truly delivering what he knows.' Thus, the giving of witness in experimental philosophy transitted the social and moral accounting systems of Restoration England.[20]

Another important way of multiplying witnesses to experimentally produced phenomena was to facilitate their replication. Experimental protocols could be reported in such a way as to enable readers of the reports to perform the experiments for themselves, thus ensuring distant but direct witnesses. Boyle elected to publish several of his experimental series in the form of letters to other experimentalists or potential experimentalists. The *New Experiments* of 1660 was written as a letter to his nephew Lord Dungarvan; the various tracts of the *Certain Physiological Essays* 490
of 1661 were written to another nephew Richard Jones; the *History of Colours* of 1664 was originally written to an unspecified friend. The purpose of this form of communication was explicitly to proselytize. The *New Experiments* was published so 'that the person I addressed them to might, without mistake, and with as little trouble as possible, be able to repeat such unusual experiments . . .'. The *History of Colours* was designed 'not barely to relate [the experiments], but . . . to teach a young gentleman to make them.'[21] Boyle wished to encourage young gentlemen to 'addict' themselves to experimental pursuits and, thereby, to multiply both experimental philosophers and experimental facts.

Replication, however, rarely succeeded, as Boyle himself recognized. When he came to prepare the *Continuation of New Experiments* seven years after the original air-pump trials, Boyle admitted that, despite his care in communicating details of the engine and of his procedures, there had been few successful replications:

> . . . in five or six years I could hear but of one or two engines that were brought to be fit to work, and of but one or two new experiments that had been added by the ingenious owners of them . . .[22]

This situation had not notably changed by the mid-1670s. In the seven or eight years after the *Continuation*, Boyle said that he heard 'of very few experiments made, either in the engine I used, or in any other made after the model thereof.' By this time a note of despair began to appear in Boyle's statements concerning the replication of his air-pump experiments. He

> was more willing to set down divers things with their minute circumstances; because I was of opinion, that probably many of these experiments would be never either re-examined by others, or re-iterated by myself. For though they may be easily read . . . yet he, that shall really go about to repeat them, will find it no easy task.[23]

The Literary Technology of Virtual Witnessing

The third way by which witnesses could be multiplied is far more important than the performance of experiments before direct witnesses or the facilitating of actual replication: it is what I shall
491 call 'virtual witnessing'. The technology of virtual witnessing involves the production in a reader's mind of such an image of an experimental scene as obviates the necessity for either its direct witness or its replication. Through virtual witnessing the multiplication of witnesses could be in principle unlimited. It was therefore the most powerful technology for constituting matters of fact. The validation of experiments, and the crediting of their outcomes as matters of fact, necessarily entailed their realization in the laboratory of the mind and the mind's eye. What was required was a technology of trust and assurance that the things had been done and done in the way claimed.

The technology of virtual witnessing was not different in kind to that used to facilitate actual replication. One could deploy the same linguistic resources in order to encourage the physical replication of experiments or to trigger in the reader's mind a naturalistic image of the experimental scene. Of course, actual replication was to be preferred, for this eliminated reliance upon testimony altogether. Yet, because of natural and legitimate suspicion amongst those who were neither direct witnesses nor replicators, a greater degree of assurance was required to produce assent in virtual witnesses. Boyle's literary technology was crafted to secure this assent.

Prolixity and Iconography

In order to understand how Boyle deployed his literary technology of virtual witnessing we have to reorientate some of our common ideas about the status of the scientific *text*. We usually think of an experimental report as a narration of some prior visual experience: it points to sensory experience that lies behind the text. This is correct. However, we should also appreciate that the text itself constitutes a visual source. It is my task here to see how Boyle's texts were constructed so as to provide a source of virtual witness that was agreed to be reliable. The best way to fasten upon the notion of the text as this kind of source might be to start by looking at some of the pictures that Boyle provided alongside his prose.

Figure 1, for example, is an engraving of his original air-pump, appended to the *New Experiments*. Producing these kinds of images was an expensive business in the mid-seventeenth century and natural philosophers used them sparingly. As we see, Figure 1 is not a schematized line-drawing but an attempt at detailed
492 naturalistic representation, complete with the conventions of shadowing and cut-away sections of parts. This is not a picture of the 'idea' of an air-pump but of a particular existing air-pump.[24] The same applies to Boyle's pictorial representations of his particular pneumatic experiments: in one, we are shown a mouse lying dead in the receiver; in another, images of the experimenters. Boyle devoted great attention to the manufacture of these engravings, sometimes consulting directly with artist and engraver, sometimes by way of Hooke.[25] Their role was to be a supplement to the imaginative witness provided by the words in the text. In the *Continuation* Boyle expanded upon the relationships between the two sorts of exposition. He told his readers that 'they who either were versed in such kind of studies or have any peculiar facility of imagining, would well enough conceive my meaning only by words,' but others required visual assistance. He apologized for the

relative poverty of the images, 'being myself absent from the engraver for a good part of the time he was at work, some of the cuts were misplaced, and not graven in the plates.'[26]

Thus, visual representations, few as they necessarily were in Boyle's texts, were mimetic devices. By virtue of the density of *circumstantial* detail that could be conveyed through the engraver's laying of lines, the images imitated reality and gave the viewer a vivid impression of the experimental scene. The sort of naturalistic images that Boyle favoured provided a greater density of circumstantial detail than would have been proffered by more schematic representations. The images served to announce that 'this was really done' and that it was done in the way stipulated; they allayed distrust and facilitated virtual witnessing. Therefore, understanding the role of pictorial representations offers a way of appreciating what Boyle was trying to achieve with his literary technology.[27]

In the introductory pages of the *New Experiments,* Boyle's first published experimental findings, he directly announced his intention to be 'somewhat prolix'. His excuses were three-fold: first delivering things 'circumstantially' would, as we have already seen, facilitate replication; second, the density of circumstantial details was justified by the fact that these were 'new' experiments, with novel conclusions drawn from them: it was therefore necessary that they be 'circumstantially related, to keep the reader from distrusting them'; third, circumstantial reports such as these offered the possibility of virtual witnessing. As Boyle said, 'these narratives [are to be] as standing records in our new pneumatics, and [readers] need not reiterate themselves an experiment *to have as distinct an idea of it,* as may suffice them to ground their reflexions and speculations upon'.[28] If one wrote an experimental report in the correct way, the reader could take on trust that these things happened. Further, it would be as if that reader had been present at the proceedings. He would be recruited as a witness and be put in a position where he could validate experimental phenomena as matters of fact.[29] Therefore, attention to the writing of experimental reports was of equal importance to doing the experiments themselves.

In the late 1650s Boyle devoted himself to laying down the rules for the literary technology of the experimental programme. Stipulations about how to write proper scientific prose are dispersed throughout his experimental reports of the 1660s, but he also composed a special tract on the subject of 'experimental essays'. Here Boyle offered extended apologia for his 'prolixity': 'I have,' he understated, 'declined that succinct way of writing'; he had sometimes 'delivered things, to make them more clear, in such a multitude of words, that I now seem even to myself to have in divers places been guilty of verbosity . . .' Not just his 'verbosity' but also Boyle's ornate sentence-structure, with appositive clauses piled on top of each other, was, he said, part of a plan to convey circumstantial details and to give the impression of verisimilitude:

> . . . I have knowingly and purposely transgressed the laws of oratory in one particular, namely, in making sometimes my periods [i.e., complete sentences] or parentheses over-long: for when I could not within the compass of a regular period comprise what I thought requisite to be delivered at once, I chose rather to neglect the precepts of rhetoricians, than the mention of those things, which I thought pertinent to my subject, and useful to you, my reader.[30]

Elaborate sentences, with circumstantial details encompassed within the confines of one grammatical entity, might mimic that immediacy and simultaneity of experience afforded by pictorial representations.

Boyle was endeavouring to constitute himself as a reliable purveyor of experimental testimony and to offer conventions by means of which others could do likewise. The provision of circumstantial details of experimental scenes was a way of assuring readers that real experiments had yielded the findings stipulated. It was also necessary, in Boyle's view, to offer readers circumstantial
494 accounts of *failed* experiments. This performed two functions: first, it allayed anxieties in those neophyte experimentalists whose expectations of success were not immediately fulfilled; second, it assured the reader that the relator was not wilfully suppressing inconvenient evidence, that he was in fact being faithful to reality. Complex and circumstantial accounts were to be taken as undistorted mirrors of complex experimental performances, in which a wide range of contingencies might influence outcomes.[31] So, for example, it was not legitimate to hide the fact that air-pumps sometimes did not work properly or that they often leaked: '. . . I think it becomes one, that professeth himself a faithful relator of experiments not to conceal' such unfortunate contingencies.[32] It is, however, vital to keep in mind that the contingencies proffered in Boyle's circumstantial accounts represent a selection of possible contingencies. There was not, nor can there be, any such thing as a report which notes all circumstances which might affect an experiment. Circumstantial, or stylized, accounts do not, therefore, exist as pure forms but as publicly acknowledged moves towards or away from the reporting of contingencies.

The Modesty of Experimental Narrative

The ability of the reporter to multiply witnesses depended upon readers' acceptance of him as a provider of reliable testimony. It was the burden of Boyle's literary technology to assure his readers that he was such a man as should be believed. He therefore had to find the means to make visible in the text the accepted tokens of a man of good faith. One technique has just been discussed: the reporting of experimental failures. A man who recounted unsuccessful experiments was such a man whose objectivity was not distorted by his interests. Thus, the literary display of a certain sort of morality was a technique in the making of matters of fact. A man whose narratives could be credited as mirrors of reality was a 'modest man'; his reports should make that modesty visible.

Boyle found a number of ways of displaying modesty. One of the most straightforward was the use of the form of the experimental essay. The essay, (that is, the piece-meal reporting of experimental trials) was explicitly contrasted to the natural philosophical system. Those who wrote entire systems were identified as
495 'confident' individuals, whose ambition extended beyond what was proper or possible. By contrast, those who wrote experimental essays were 'sober and modest men', 'diligent and judicious' philosophers, who did not 'assert more than they can prove.' This practice cast the experimental philosopher into the role of intellectual 'under-builder', or even that of 'a drudge of greater industry than reason'. This was, however, a noble character, for it was one that was freely chosen to further 'the real advancement of true natural philosophy' rather than personal reputation.[33] The public display of this modesty was an exhibition that concern for individual celebrity did not cloud judgement and distort the integrity of one's reports. In this connection it is absolutely crucial to remember who it was that was portraying himself as a mere 'under-builder'. He was the son of the Earl of Cork, and everyone

knew that very well. Thus, it was plausible that such modesty could have a noble character, and Boyle's presentation of self as a role model for experimental philosophers was powerful.[34]

Another technique for displaying modesty was Boyle's professedly 'naked way of writing'. He would eschew a 'florid' style; his object was to write 'rather in a philosophical than a rhetorical strain'. This plain, puritanical, unadorned (yet convoluted) style was identified as *functional*. It served to exhibit, once more, the philosopher's dedication to community service rather than to his personal reputation. Moreover, the 'florid' style to be avoided was a hindrance to the clear provision of virtual witness: it was, Boyle said, like painting 'the eye-glasses of a telescope'.[35]

The most important literary device Boyle employed for demonstrating modesty acted to protect the fundamental epistemological category of the experimental programme: the matter of fact. There were to be appropriate moral postures, and appropriate modes of speech, for epistemological items on either side of the crucial boundary that separated matters of fact from the locutions used to account for them: theories, hypotheses, speculations, and the like. Thus, Boyle told his nephew,

> in almost every one of the following essays I . . . speak so doubtingly, and use so often, *perhaps, it seems, it is not improbable,* and such other expressions, as argue a diffidence of the truth of the opinions I incline to, and that I should be so shy of laying down principles, and sometimes of so much as venturing at explications.

Since knowledge of physical causes was only 'probable', this was 496
the correct moral stance and manner of speech, but things were otherwise with matters of fact, and here a confident mode was not only permissible but necessary:

> . . . I dare speak confidently and positively of very few things, except of matters of fact.[36]

It was necessary to speak confidently of matters of fact because, as the foundations of proper philosophy, they required protection. And it was proper to speak confidently of matters of fact, because they were not of one's own making; they were, in the empiricist model, discovered rather than invented. As Boyle told one of his adversaries, experimental facts can 'make their own way' and 'such as were very probable, would meet with patrons and defenders . . .'[37] The separation of modes of speech, and the ability of facts to make their own way, was made visible on the printed page. In *New Experiments* Boyle said he intended to leave 'a conspicuous interval' between his narratives of experimental findings and his occasional 'discourses' upon their interpretation. One might then read the experiments and the 'reflexions' separately.[38] Indeed, the construction of Boyle's experimental essays makes manifest the proper balance between the two categories: *New Experiments* consists of a sequential narrative of 43 pneumatic experiments; *Continuation* of 50; and the second part of *Continuation* of an even larger number of disconnected experimental observations, only sparingly larded with interpretative locutions.

The confidence with which one ought to speak about matters of fact extended to stipulations about the proper use of authorities. Citations of other writers should be employed to use them not as 'judges, but as witnesses', as 'certificates to attest matters of fact.' If this practice ran the risk of identifying the experimental philosopher as an ill-read philistine, it was, however, necessary:

'. . . I could be very well content to be thought to have scarce looked upon any other book than that of nature.'[39] The injunction against citing of authorities performed a significant function in the mobilization of assent to matters of fact. It was a way of displaying that one was aware of the workings of the Baconian 'Idols' and was taking measures to mitigate their corrupting effects on knowledge-claims.[40] A disengagement between experimental narrative and the authority of systematists served to dramatize the author's lack of
497 preconceived expectations and, especially, of theoretical investments in the outcome of experiments. For example, Boyle several times insisted that he was an innocent of the great theoretical systems of the seventeenth century. In order to reinforce the primacy of experimental findings, 'I had purposely refrained from acquainting myself thoroughly with the intire system of either the Atomical, or the Cartesian, or any other whether new or received philosophy . . .' And, again, he claimed that he had avoided a systematic acquaintance with the systems of Gassendi, Descartes, and even of Bacon, 'that I might not be prepossessed with any theory or principles . . .'[41]

Boyle's 'naked way of writing', his professions and displays of humility, and his exhibition of theoretical innocence all complemented each other in the establishment and the protection of matters of fact. They served to portray the author as a disinterested observer and his accounts as unclouded and undistorted mirrors of nature. Such an author gave the signs of a man whose testimony was reliable. Hence, his texts could be credited and the number of witnesses to his experimental narratives could be multiplied indefinitely.

Scientific Discourse and the Community

I have said that the matter of fact was a social as well as an intellectual category. And I have argued that Boyle deployed his literary technology so as to make virtual witnessing a practical option for the validation of experimental performances. I want in this section to examine the ways in which Boyle's literary technology dramatized the social relations proper to a community of experimental philosophers. Only by establishing right rules of discourse between individuals could matters of fact be generated and defended, and only by constituting these matters of fact into the agreed foundations of knowledge could a moral community of experimentalists be created and sustained. Matters of fact were to be produced in a public space: a particular space in which experiments were collectively performed and directly witnessed and an abstract space constituted through virtual witnessing. The problem of producing this kind of knowledge was, therefore, the problem of maintaining a certain form of discourse and a certain form of social solidarity. In the following sections I will discuss the
498 ways in which Boyle's literary technology worked to create and maintain this social solidarity amongst experimental philosophers.

The Linguistic Boundaries of the Experimental Community

In the late 1650s and early 1660s, when Boyle was formulating his experimental and literary practices, the English experimental community was still in its infancy. Even with the founding of the Royal Society, the crystallization of an experimental community centred on Gresham College, and the network of correspondence organized by Henry Oldenburg, the experimental programme was far from securely institutionalized. Criticisms of the experimental way of producing physical knowledge emanated from English philosophers (notably Hobbes) and from Continental writers committed to rationalist methods and to the practice of physics as a demonstrative discipline. Experimentalists were made into figures of fun on the Restoration stage: Thomas Shadwell's *The Virtuoso* dramatized the absurdity of weighing the air, and scored most of its good jokes by parodying the convoluted language of Sir Nicholas Gimcrack (Boyle).[42] The practice of experimental philosophy, despite what numerous historians have assumed, was not overwhelmingly popular in Restoration England.[43] In order for experimental philosophy to be established as a legitimate activity, several things needed to be done. First, it required *recruits*: experimentalists had to be enlisted as neophytes, and converts from other forms of philosophical practice had to be obtained. Second, the social role of the experimental philosopher and the linguistic practices appropriate to an experimental community needed to be defined and publicized.[44] What was the proper nature of discourse in such a community? What were the linguistic signs of competent membership? And what uses of language could be taken as indications that an individual had transgressed the conventions of the community?

The entry fee to the experimental community was to be the communication of a candidate matter of fact. In *The Sceptical Chymist,* for instance, Boyle extended an olive-branch even to the alchemists. The solid experimental findings produced by some alchemists could be sifted from the dross of their 'obscure' speculations. Since the experiments of the alchemists (and of the Aristotelians) frequently 'do not evince what they are alleged to prove', the former could be accepted into the experimental philosophy by stripping away the theoretical language with which they happened to be glossed. As Carneades (Boyle's mouthpiece) said, 499

> . . . your hermetic philosophers present us, together with divers substantial and noble experiments, theories, which either like peacocks feathers make a great shew, but are neither solid nor useful; or else like apes, if they have some appearance of being rational, are blemished with some absurdity or other, that, when they are attentively considered, make them appear ridiculous.[45]

Thus, those alchemists who wished to be incorporated into a legitimate philosophical community were instructed what linguistic practices could secure their entry. The same principles were laid down with respect to any practitioner: 'let his opinions be never so false, his experiments being true, I am not obliged to believe the former, and am left at liberty to benefit myself by the latter.'[46] By arguing that there was only a contingent, not a necessary, connection between the language of matters of fact and theoretical language, Boyle was defining the linguistic terms upon which existing communities could join the experimental enterprise. They were liberal terms, which might serve to maximize potential membership.[47]

There were other natural philosophers Boyle despaired to recruit. Hobbes, notably, was the kind of philosopher who, on no account, ought to be admitted, for he denied the value of systematic and elaborate experimentation, the foundational status of the matter of fact, and the distinction between causal and descriptive language. Of Hobbes's *Dialogus physicus,* Boyle asked 'What new experiment or matter of fact Mr *Hobbes* has therein added to enrich the history of nature . . .?' In his criticisms of Boyle's experiments Hobbes 'does not, that I remember, deny the truth of any of the matters of fact I have delivered.' According to Boyle, both Hobbes and another critic, the Jesuit Franciscus Linus, had not 'seen cause to deny any thing that I deliver as experiment.'[48] One could not be regarded as a competent member of the experimental community if one failed to communicate experimental matters of fact, or if one did so in a manner that failed to recognize the linguistic boundaries between factual and causal locutions.

500 *Linguistic Boundaries within the Experimental Community*

Just as linguistic categories were used to manage entry to the experimental community, distinctions between the language of facts and that of theories were deployed to regulate discourse within it. In broad terms, Boyle insisted upon a separation between 'physiological' and 'metaphysical' languages: experimental discourse was to be confined to the former. One of the central categories of Boyle's 'new pneumatics' also happened to be a major preoccupation of the old physics — namely, vacuism versus plenism, and the judgement whether a vacuum was possible in nature. How was it proper to speak of the contents of the receiver of an evacuated air-pump? And how did this speech relate to traditional usages of the term 'vacuum'?

A practical problem was posed by the fact that the lexicon of the new philosophy was largely compiled out of the usages of old discursive practices. Old words had to be given new meanings. Thus, it was proper to apply the term 'vacuum' to the contents of the exhausted receiver, but it was improper to take this to mean that the space was absolutely devoid of all matter. Such an absolutely void space was the 'vacuum' of metaphysical discourse. What Boyle meant by the air-pump's 'vacuum' was 'not a space, wherein there is no body at all, but such as is either altogether, or almost totally devoid of air.'[49] If contemporary plenists maintained that this vacuum might be filled by a subtle form of matter, or 'aether', Boyle could reply with a series of experiments which showed that such an aether could not be made 'sensible', that is, it had no physical manifestations. And speech of entities that were not amenable to sensible experimentation was not permissible within experimental philosophy.[50]

The separation of 'physiological' from 'metaphysical' language was most crucial to Boyle's strategy for dealing with causal inquiry in physical science. In keeping with his probabilist conception of knowledge, Boyle wished to bracket off speech about matters of fact, about which one might be certain, from speech of their physical causes, which were at best probable. In terms of Boyle's air-pump programme, the most important instance of this bracketing concerned the notion which was the main product of these experiments: the 'spring of the air'. Boyle said that his 'business' was 'not to assign the adequate cause of the spring of the
501 air, but only to manifest, that the air hath a spring, and to relate some of its effects.' The cause of the air's elasticity *might* be accounted for variously: by Cartesian vortices, or by the real physical existence in the corpuscles of the air of 'slender springs' or of a fleecy structure.[51] The job of the experimental philosopher was to speak of experimentally-produced matters of fact, not to conjecture further than that.[52]

Boyle had considerable problems in diffusing this new mode of speech. Plenist critics persisted in understanding Boyle to be using 'vacuum' in its metaphysical sense, and Boyle was obliged persistently to reiterate its proper usage.[53] Other writers either refused to conceive of a natural philosophy that bracketed off causal speech, or reckoned that Boyle must be committed to some (illegitimate and unacknowledged) causal account of the spring of the air.[54] So far as the 'spring of the air' was concerned, Boyle's stipulation that it had been made experimentally 'manifest' and his disinclination to speak of its cause had an interesting effect. By putting the spring on the other side of the boundary from causal locutions, Boyle constituted the spring, for all practical purposes, into a matter of fact. When it came to labelling the epistemological status of the spring, Boyle variously referred to it as an 'hypothesis' or even as a 'doctrine'. However, by making the spring into something that was made manifest through experiment, and by protecting it from the uncertainties that afflicted epistemological items like causal notions, Boyle treated this 'hypothesis' in the same way that he treated other matters of fact.[55]

The vital difference between matters of fact and all other epistemological categories was the degree of assent one might expect to them. To an authenticated matter of fact all men will assent. In Boyle's system that was taken for granted because it was through the technologies that multiplied witness that matters of fact were constituted. General assent was what made matters of fact, and general assent was therefore mobilized around matters of fact. With 'hypotheses', 'theories', 'conjectures', and the like, the situation was quite different. These categories threatened that assent which could be crystallized in the institution of the matter of fact. Thus, the linguistic conventions of Boyle's experimental programme separated speech appropriate to the two categories as a way of drawing the boundaries between that about which one was to expect certainty and assent and that about which one could expect uncertainty and divisiveness. The idea was not to eliminate
dissent or to oblige men to agree to all items in natural philosophy 502
(as it was for Hobbes); rather, it was to manage dissent and to keep it within safe bounds. An authenticated matter of fact was treated as a mirror of nature; a theory, by contrast, was clearly man-made and could, therefore, be contested. Boyle's linguistic boundaries acted to segregate what could be disputed from what could not. The management of dispute in experimental philosophy was crucial to protecting the foundations of knowledge.

Manners in Dispute

Since natural philosophers were not to be compelled to give assent to all items of knowledge, dispute and controversy was to be expected. How should this be dealt with? The problem of conducting dispute was a matter of intense practical concern in early Restoration science. During the Civil War and Interregnum the divisiveness of 'enthusiasts', sectarians and hermeticists threatened to bring about radical individualism in philosophy. Nor did the various sects of Peripatetic natural philosophers display a public image of a stable and united intellectual community. Unless the new experimental community could exhibit a broadly-based consensus and harmony within its own ranks, it was unreasonable to expect it to secure the legitimacy within Restoration culture that its leaders desired. Moreover, that very consensus was vital to the establishment of matters of fact as the foundational category of the new practice.

By the early 1660s Boyle was in a position to give concrete exemplars of how disputes ought to be conducted; three critics published their responses to his *New Experiments*, and he replied to

each one: Linus, Hobbes and Henry More. But even before he had been engaged in dispute, Boyle laid down a set of rules for how controversies were to be handled by the experimental philosopher. For example, in *A Proëmial Essay* (composed 1657), Boyle insisted that disputes should be about findings and not about persons. It was proper to take a hard view of reports which were inaccurate but most improper to attack the character of those who rendered them: 'for I love to speak of persons with civility, though of things with freedom'. The *ad hominem* style must at all costs be avoided, for the risk was that of making foes out of mere dissenters. This was the key point: potential contributors of matters of fact, however
503 wrong they may be, must be treated as possible converts to the experimental philosophy. If, however, they were bitterly treated, they would be lost to the cause and to the community whose size and consensus validated matters of fact:

> And as for the (very much too common) practice of many, who write, as if they thought railing at a man's person, or wrangling about his words, necessary to the confutation of his opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a philosopher and a Christian, methinks it is as unwise, as it is provoking. For if I civilly endeavour to reason a man out of his opinions, I make myself but one work to do, namely, to convince his understanding; but, if in a bitter or exasperating way I oppose his errors, I increase the difficulties I would surmount, and have as well his affections against me as his judgment: and it is very uneasy to make a proselyte of him, that is not only a dissenter from us, but an enemy to us.[56]

Furthermore, it was impolitic to acknowledge the existence of 'sects' in natural philosophy. One way by which one could hope to overcome sectarianism was to decline public recognition that it existed: 'it is none of my design,' Boyle said, 'to engage myself with, or against, any one sect of Naturalists . . .' The experiments will decide the case. The views of these 'sects' should be noted only insofar as they are founded upon experiment. Therefore, it was right and politic to be harsh in one's writings against those who do not contribute experimental findings, for they have nothing to offer to the constitution of matters of fact. Finally, the experimental philosopher must show that there was point and purpose to legitimately conducted dispute. He should be prepared publicly to renounce positions that were shown to be erroneous. Flexibility followed from fallibilism. As Boyle wrote, 'till a man is sure he is infallible, it is not fit for him to be unalterable.'[57]

The conventions for managing dispute were dramatized in the structure of *The Sceptical Chymist*. These fictional conversations (between an Aristotelian, two varieties of hermeticists, and 'Carneades' as mouth-piece for Boyle) took the form, not of a Socratic dialogue, but of a *conference*.[58] They were a little piece of theatre that exhibited how persuasion, dissensus and, ultimately, conversion to truth ought to be conducted. Several points about Boyle's theatre of persuasion can be briefly made: first, the 'symposiasts' are imaginary, not real. This means that opinions can be confuted without exacerbating relations between real philosophers. Even Carneades, although he is manifestly 'Boyle's
504 man', is not Boyle himself: Carneades is made actually to quote 'our friend· Mr *Boyle*' as a device for distancing opinions from individuals. The author is insulated from the text and from the opinions he may actually espouse. Second, truth is not inculcated from Carneades to his interlocutors; rather it is dramatized as emerging through the conversation.[59] Everyone is seen to have a say in the consensus which is the dénouement.[60] Third, the conversation is, without exception, civil: as Boyle said, 'I am not

sorry to have this opportunity of giving an example, how to manage even disputes with civility . . .'[61] No symposiast abuses another; no ill temper is displayed; no one leaves the conversation in pique or frustration.[62] Fourth, and most importantly, the currency of intellectual discourse, and the means by which agreement is reached, is the experimental matter of fact. Here, as I have indicated, matters of fact are not treated as the exclusive property of any one philosophical sect. Insofar as the alchemists have produced experimental findings, they have minted the real coins of experimental exchange. Their experiments are welcome, while their 'obscure' speculations are not. Insofar as the Aristotelians produce few experiments, and insofar as they refuse to dismantle the 'arch'-like 'mutual coherence' of their philosophical system into facts and theories, they can make little contribution to the experimental conference.[63] In these ways, the structure and the linguistic conventions of this imaginary conversation make vivid the rules for real conversations proper to experimental philosophy.

Real disputes followed hard upon the imaginary ones of *The Sceptical Chymist,* providing Boyle with valuable opportunities of putting his principles into practice. Linus was the adversary who experimented but who denied the power of the 'spring of the air'; Henry More was the adversary whom Boyle wished to be an ally — offering what he regarded as a theologically more appropriate explanation of Boyle's pneumatic findings; and Hobbes was the adversary who denied the value of experiment and the foundational status of the matter of fact. Each carefully crafted response that Boyle produced was labelled as a model for how disputes should be managed by the experimental philosopher.[64]

First, all public disputes had to be justified: the experimental philosopher should be loath to engage in controversy. As Boyle claimed, '. . . I have a natural indisposedness to contention . . .'[65] The justification was not the defence of one's reputation but the protection of what was vital to the collective practice of proper 505
philosophy: the value of systematic experimentation, the matters of fact that experiment produced, the boundaries that separated those facts from less certain epistemological items, and the rules of social life that regulated discourse in the experimental community. As we have seen, Boyle took care to identify the *object* of controversy as interpretations of facts, not the facts themselves. Neither Linus nor Hobbes, he said, denied 'any thing that I deliver as experiment . . ., so that usually . . . they are fain to fall upon the hypotheses themselves.' This was a crucial stipulation, because, if it was accepted, then the arena of disagreement could be so defined as to protect the status of matters of fact. The very phenomenon of public disputation about 'hypotheses' could be contrasted to the absence of controversy about that which Boyle 'deliver[ed] as experiment'.[66]

The importance of protecting experimental practice is evident in the differing tones of Boyle's responses to Linus and to Hobbes. While Linus attacked the spring of the air, the major interpretative resource of Boyle's pneumatics, 'he takes no exceptions at the experiments themselves, as we have recorded them.' Boyle concluded that this 'is no contemptible testimony, that the matters of fact have been rightly delivered . . .' The Jesuit was congratulated for essaying to experiment himself and for his diligence in understanding what Boyle had written.[67] He was a good adversary and was dealt with as a potential convert. With Hobbes the situation was quite different. This adversary, 'not content to fall upon the explications of my experiments, has (by an attempt, for aught I know, unexampeled) endeavoured to disparage unobvious experiments themselves, and to discourage others from making them.'[68] Hobbes was a dangerous adversary; there was no possibility of recruiting such a man to the experimental programme, and his objections had to be publicly exploded.

For all that, Hobbes, no less than Linus and More, had to be dealt with civilly. Boyle aimed, he said, 'to give an example of disputing in print against a provoking, though unprovoked, adversary, without bitterness and incivility . . .' He hoped that his own *Examen* 'will not be thought to have less of reason for having the less of passion . . .'[69] Managing a dispute with Hobbes was a hard case, and, if it could be conducted in a decent tone, it would offer a model of the language of controversy appropriate to a moral community of experimental philosophers. Boyle did not
506 have far to look to find examples of improper disputation, in which the language of controversy acted to exacerbate divisions in natural philosophy. From the mid-1650s Hobbes's natural philosophy and geometry had been attacked by the Oxford professors John Wallis and Seth Ward. Wallis, one of the toughest street-fighters of the new philosophy, had not only shown his adversary's notions to be erroneous, he had punned upon the plebian origins of Hobbes's name and insinuated improper political affiliations and motivations. Hobbes, who professed himself concerned for maintaining good manners in dispute, showed his foes the sharp side of his tongue:

> So go your ways, you *Uncivil Ecclesiastics, Inhuman Divines, Dedoctors of morality, Unasinous Colleagues, Egregious pair of Issachars, most wretched Vindices and Indices Academiarum* . . .[70]

And again, summing up the value of one of Wallis's criticisms,

> . . . all error and railing, that is, stinking wind; such as a jade lets fly, when he is too hard girt upon a full belly.[71]

This is what Boyle wished to avoid. It was not merely a matter of Boyle's individual 'modest' temperament or what he reckoned was owing to fellow Christian philosophers. What was at issue was the creation and preservation of a calm public space in which natural philosophers could heal their divisions, collectively agree upon the foundations of knowledge, and, thereby, establish their credit in Restoration culture. Such a calm space was vital to achieving these goals. As Boyle reminded his readers in the introduction to his *New Experiments*, published in that 'wonderful pacifick year' of the Restoration of the monarchy, 'the strange confusions of this unhappy nation, in the midst of which I have made and written these experiments, are apt to disturb that calmness of mind and undistractedness of thoughts, that are wont to be requisite to happy speculations.'[72] And Sprat recalled the circumstances of the Oxford group of experimentalists that spawned the Royal Society: 'Their first purpose was no more, then onely the satisfaction of breathing a freer air, and of conversing in quiet one with another, without being ingag'd in the passions, and madness of that dismal Age.' He described the difference between 'humane affairs', which 'may affect us, with a thousand various disquiets', and the experimental
507 study of nature: '*that* gives us room to differ, without animosity; and permits us to raise contrary imaginations upon it, without any danger of a *Civil War*.'[73]

This calm space that experimental philosophy was to inhabit would be created and maintained through the deployment within the moral community of appropriate linguistic practices.[74] An appropriate language had to perform several functions. First, it had to be a resource for managing dissent and conflict in such a way as to make it possible for philosophers to express divergent views while leaving the foundations of knowledge intact, and, in fact, buttressing these foundations. We have seen this in the linguistic separation Boyle wished to make between speech of matters of fact and speech of explanatory items. Second, it had to facilitate reconciliation amongst existing sects of philosophers, mobilizing that reconciliation so as to reinforce the foundational status of matters of fact. We have seen this in Boyle's distribution of authentic matters of fact amongst groups with divergent theoretical commitments and in his identification of experimental matters of fact as the medium of exchange in the new practice. Third, such a language had to constitute a vehicle whereby matters of fact could effectively be generated and validated by a community whose size was, in principle, unlimited. And this we have seen in the role played by Boyle's literary technology in multiplying the witnessing experience.

Scientific Knowledge and Exposition: Conclusions

I have shown that three technologies were involved in the production and validation of Boyle's experimental matters of fact: the material, the literary and the social. Although I have concentrated here upon the literary technology, I have also suggested that the three technologies are not distinct: the working of each depends upon and incorporates the others. I want now briefly to develop that point by showing how each technology contributes to a common strategy for constituting matters of fact.

What makes a fact different from an artefact is that the former is not perceived to be man-made. What men make, men may unmake, but a matter of fact is taken to be the very mirror of nature. To identify the role of human agency in the making of an item of knowledge is to identify the possibility of its being otherwise. To shift agency on to natural reality is to stipulate the grounds for universal assent. Each of the three technologies works to achieve the appearance of matters of fact as *given* items: each functions as an objectifying resource.

Take, for example, the role of the air-pump in the production of matters of fact. As I have noted, pneumatic facts were machine-made. The product of the pump was not, as it is for the modern scientific machines studied by Latour, an 'inscription': it was a visual experience that had to be transformed into an inscription by a witness.[75] However, the air-pump of the 1660s has this in common with the gamma counter of the present-day neuroendocrinological laboratory: it stands between the perceptual competences of a human being and natural reality itself. A 'bad' observation taken from a machine need not be ascribed to cognitive or moral faults in the human being, nor is a 'good' observation his personal product. It is the machine that has generated the finding. A striking instance of this usage arose in the 1660s when Christiaan Huygens offered a matter of fact produced by his pump which appeared to conflict with one of Boyle's central explanatory resources. Boyle did not impugn Huygens's integrity or his perceptual and cognitive competences. Instead, he suggested that the fault lay with the machine: '[I] question not his Ratiocination, but only the staunchness of his pump.'[76] The machine constitutes a resource that may be used to factor out human agency in the intellectual product: 'it is not I who says this; it is the machine that speaks,' or 'it is not your fault; it is the machine's.'

Boyle's social technology constituted an objectifying resource by making the production of knowledge visible as a collective enterprise: 'it is not I who says this; it is all of us.' As Sprat insisted, collective performance and collective witness served to correct the natural working of the 'idols': the faultiness, the idiosyncracy or the bias of any individual's judgement and observational ability. The Royal Society advertised itself as a 'union of eyes, and hands'; the space in which it produced its experimental knowledge was stipulated to be a *public space*. It was public in a very precisely defined and very rigorously policed sense: not everyone could come in; not everyone's testimony was of equal worth; not everyone was equally able to influence the official voice of the institution. Nevertheless, what Boyle was proposing, and what the Royal Society was endorsing, was a crucially important *move towards* the public constitution and validation of knowledge. The contrast was, on the one hand, with the private work of the alchemists, and, on the other, with the individual dictates of the systematical philosophers.

509 In the official formulation of the Royal Society, the production of experimental knowledge commenced with individuals' acts of seeing and believing, and was completed when all individuals voluntarily agreed with one another about what had been seen and ought to be believed. This freedom to speak had to be protected by a special sort of discipline. Radical individualism — each individual setting himself up as the ultimate judge of knowledge — would destroy the conventional basis of knowledge, while the disciplined collective social structure of the experimental language game would create and sustain that factual basis. Thus, the experimentalists were on guard against 'dogmatists' and 'tyrants' in philosophy, just as they abominated 'secretists' who produced their knowledge-claims in a private space. No one man was to have the right to lay down what was to count as knowledge. Legitimate knowledge was objective insofar as it was produced by the collective, and agreed to voluntarily by those who comprised the collective. The objectification of knowledge proceeded through displays of the communal basis of generation and evaluation. Human coercion was to have no visible place in the experimental way of life.[77]

It was the function of the literary technology to create that communal way of life, to bound it, and to provide the forms and conventions of social relations within it. The literary technology of virtual witnessing supplemented the public space of the laboratory by extending a valid witnessing experience to all readers of the text. The boundaries stipulated by Boyle's linguistic practices acted to keep that community from fragmenting and served to protect items of knowledge to which one could expect universal assent from items which produced divisiveness. Similarly, Boyle's stipulations concerning proper manners in dispute worked to guarantee that social solidarity which generated assent to matters of fact and to rule out of order those imputations which would undermine the moral integrity of the experimental way of life.

I have attempted to display these linguistic practices in the making, and, within restrictions of space, I have alluded to sources of seventeenth-century opposition to these practices. It is important to understand two things about these ways of expounding scientific knowledge and securing assent: that they are historical constructions and that there have been alternative practices. It is

particularly important to understand this because of the problems 510
of givenness and self-evidence that attend the institutionalization and conventionalization of these practices. Just as the three technologies operate to create the illusion that matters of fact are not man-made, so the institutionalized and conventional status of the scientific discourse that Boyle helped to produce makes the illusion that scientists' speech about natural reality is simply a reflection of that reality. In this instance, and in others like it, the historian has two major tasks: to display the man-made nature of scientific knowledge, and to account for the illusion that this knowledge is *not* man-made. It is one of the recommendations of the sociology of knowledge perspective that analysts often attempt to accomplish these two tasks in the same exercise.[78]

In the late twentieth century scientific papers are rarely, if ever, written with the depth of circumstantial detail which Boyle's reports contained. Why might this be? The answer to this question leads us to the study of linguistic aspects of scientific institutionalization and differentiation. In discussing the characteristics of a *Denkkollektiv,* Ludwik Fleck noted that such a group cultivates 'a certain exclusiveness both formally and in content':

> A thought commune becomes isolated formally, but also absolutely bonded together, through statutory and customary arrangements, sometimes a separate language, or at least special terminology . . . The optimum system of a science, the ultimate organization of its principles, is completely incomprehensible to the novice [or, Fleck might have added, to any non-member].[79]

Fleck was suggesting that the linguistic conventions of a body of practitioners constitute an answer to the question 'Who may speak?' The language of an institutionalized and specialized scientific group is removed from ordinary speech, and from the speech of scientists belonging to another community, both as a sign and as a vehicle of the group's special and bounded status. Not everyone may speak; the ability to speak entails the mastering of special linguistic competences; and the use of ordinary speech is taken as a sign of non-membership and non-competence. Such a group gives linguistic indications that the generation and validation of its knowledge does not require the mobilizing of belief, trust and assent outwith its own social boundaries. (Yet, when external support or subvention is required, special *occasional* modes of speech may be resorted to, including the various languages of 'popularization'.)

By contrast, Boyle's circumstantial reporting was a means of 511
involving a wider community and soliciting its participation in the making of factual experimental knowledge. His circumstantial language was a way of bringing readers into the experimental scene, indeed of making the reader an actor in that scene. The reader was to be shown not just the products of experiments but their mode of construction and the contingencies affecting their performance, *as if he were present*. Boyle aimed to accomplish this, not by inventing a totally novel language (although it was novel to the natural philosophical community of the time), but, it could be argued, by incorporating aspects of ordinary speech and lay techniques of validating knowledge-claims. The language of early Restoration experimental science was, in this sense, a public language. And the use of this public language was, in Boyle's work, essential to the creation of both the knowledge and the social solidarity of the experimental community. Trust and assent had to be won from a public that might crucially deny trust and assent.

6. The use of the word 'technology' in reference to the 'software' of literary practices and social relations may appear jarring, but it is in fact etymologically justified, as Carl Mitcham nicely shows: C. Mitcham, 'Philosophy and the History of Technology', in G. Bugliarello and D.B. Doner (eds), *The History and Philosophy of Technology* (Urbana, Ill.: University of Illinois Press, 1979), 163-201, esp. 172 ff. The Greek *techne* has behind it the Indo-European stem *tekhn*, probably meaning 'woodwork' or 'carpentry'. However, in early Plato *techne* was also conceived as a kind of knowledge. In *Gorgias* Socrates distinguishes two types of *techne*: one which consists mainly of physical work and another which is closely associated with speech. By using 'technology' to refer to social and literary practices, as well as to hardware, I wish to stress that all three are *knowledge-producing tools*.

7. See, for example, Boyle, 'An Examen of Mr. T. Hobbes his Dialogus Physicus de Natura Aëris . . .', in *RBW*, Vol. I, 186-242, at 241 (orig. publ. 1662); Boyle, 'Animadversions upon Mr. Hobbes's Problemata de Vacuo', in *RBW*, Vol. IV, 104-28, at 105 (orig. publ. 1674). The explication of the behaviour of liquids in the gardener's pot was a set-piece in the mid-seventeenth-century contest between rival physical systems; see T. Hobbes, 'Concerning Body' in *The English Works of Thomas Hobbes*, ed. Sir William Molesworth, 11 Vols. London, 1839-1845), Vol. I, 414-15 (orig. publ. 1656); compare Boyle, 'Examen of Hobbes', 191-93.

8. Boyle described his pump in 'New Experiments', op. cit. note 1, 6-11. One of

512 1672 Optical Controversies: A Study in the Grammar of Scientific Dissent', in Y. Elkana (ed.), *The Interaction between Science and Philosophy* (Atlantic Highlands, NJ: Humanities Press, 1974), 115-42.

4. The usual form in which Boyle phrased this was the statement that God might produce the same effects in nature through very different causes; therefore 'it is a very easy mistake for men to conclude that because an effect may be produced by such determinate causes, it must be so, or actually is so.' Boyle, 'Some Considerations touching the Usefulness of Experimental Natural Philosophy', *RBW*, Vol. II, 1-201, at 45 (orig. publ. 1663). See also L. Laudan, 'The Clock Metaphor and Probabilism: The Impact of Descartes on English Methodological Thought, 1650-65', *Annals of Science*, Vol. 22 (1965), 73-104; G.A.J. Rogers, 'Descartes and the Method of English Science', ibid., Vol. 29 (1972), 237-55; H.G. van Leeuwen, *The Problem of Certainty in English Thought 1630-1690* (The Hague: M. Nijhoff, 1963), 95-96; Shapiro, op. cit. note 2, 44-61.

5. This is especially evident in historians' treatment (or lack thereof) of criticisms of seventeenth-century experimentalism by philosophers who denied both the central role of experimental procedures and the foundational status of the matter of fact. For example, insofar as Thomas Hobbes's criticisms of Boyle's experimental programme have been discussed, historians have preferred to conclude that he 'misunderstood' Boyle, or that he 'failed to appreciate' the power of experimental methods: see, among others, F. Brandt, *Thomas Hobbes' Mechanical Conception of Nature* (Copenhagen: Levin & Munksgaard, 1928), 377-78; M.B. Hall, 'Boyle, Robert', in *Dictionary of Scientific Biography*, Vol. 2 (New York: Charles Scribner's Sons, 1970), 379; L.T. More, *The Life and Works of the Honourable Robert Boyle* (London: Oxford University Press, 1944), 97, 239. Hobbes's anti-experimentalism is fully treated in S. Shapin and S. Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life* (Princeton, NJ: Princeton University Press, forthcoming).

513 the best accounts of the original pump and subsequent designs is still G. Wilson, 'On the Early History of the Air-Pump in England', *Edinburgh New Philosophical Journal*, Vol. 46 (1849), 330-54; see also R.G. Frank, Jr, *Harvey and the Oxford Physiologists: A Study of Scientific Ideas* (Berkeley, Calif.: University of California Press, 1980), 128-30.

9. The only information we have concerning the cost of the Boyle pump indicates that a version of the *receiver* ran to £5: T. Birch, *The History of the Royal Society of London*, 4 Vols. (London, 1756-1757), Vol. II, 184. Given the expense of machining the actual pumping apparatus, an estimate of £25 for the entire engine might be conservative. Thus, an air-pump would have cost more than the annual salary of the Curator of the Royal Society, Robert Hooke, who was the London pump's chief operator. Christiaan Huygens's elder brother Constantijn pulled out of a pump-building project, 'being afraid of the cost': Christiaan Huygens, *Oeuvres complètes*, 22 Vols. (The Hague: M. Nijhoff, 1888-1950), Vol. III, 389. The Accademia del Cimento in Florence did not even try to build a *Machina Boyleana*, even though they had the necessary texts at hand: W.E. Knowles Middleton, *The Experimenters: A Study of the Accademia del Cimento* (Baltimore, Md: The Johns Hopkins University Press, 1971), 263-65. Full details of the career of the air-pump in the 1660s are in Shapin and Schaffer, op. cit. note 5, Chapter 6.

10. Boyle, 'Examen of Hobbes', op. cit. note 7, 193. Both 'elaborate' and systematic experimentation were also recommended as the bases for constructing

well-framed theories. Those theories 'that are grounded but upon few and obvious experiments, are subject to be contradicted' by new findings; see Boyle, 'A Proëmial Essay . . . with Some Considerations touching Experimental Essays in General', in *RBW*, Vol. I, 299-318, at 302 (orig. publ. 1661).

11. See, for example, Boyle, 'The Sceptical Chymist', in *RBW*, Vol. I, 458-586, at 460 (orig. publ. 1661): here Boyle suggests that many 'experiments' reported by the alchemists 'questionless they never tried'. For an insinuation that Henry More may not actually have performed experiments adduced against Boyle's findings, see Boyle, 'An Hydrostatical Discourse, Occasioned by the Objections of the Learned Dr. Henry More', in *RBW*, Vol. III, 596-628, at 607-08 (orig. publ. 1672). Compare the response of Boyle to Pascal's trials and their reporting. Boyle reported the replication of the Puy-de-Dôme experiment in 'New Experiments', op. cit. note 1, 14, 43; and by Power, Towneley and himself in 'A Defence of the Doctrine touching the Spring and Weight of the Air . . . against the Objections of Franciscus Linus', in *RBW*, Vol. I, 118-85, at 151-55 (orig. publ. 1662). Yet Boyle doubted the reality of Pascal's other reports of underwater trials; see 'Hydrostatical Paradoxes, made out by New Experiments . . .', in *RBW*, Vol. II, 738-97, at 745-46 (orig. publ. 1666): '. . . though the experiments [Pascal] mentions be delivered in such a manner, as is usual in mentioning matters of fact; yet I remember not, that he expressly says, that he actually tried them, and therefore he might possibly have set them down, as things that *must* happen, upon a just confidence, that he was not mistaken in his ratiocinations . . . Whether or not Monsieur Pascal ever made these experiments himself, he does not seem to have been very desirous, that others should make them after him.' For the role of thought experiments in the history of science, see A. Koyré, *Galileo Studies* (Atlantic Highlands, NJ: Humanities Press, 1978), 97; T.S. Kuhn, 'A Function for Thought Experiments', in Kuhn, *The Essential Tension* (Chicago: The University of Chicago Press, 1977), 240-65; C.B. Schmitt, 'Experience and Experiment: A Comparison of Zabarella's View with Galileo's in *De motu*', *Studies in the Renaissance*, Vol. 16 (1969), 80-137.

12. Boyle, 'Two Essays, Concerning the Unsuccessfulness of Experiments', in *RBW*, Vol. I, 318-53, at 343 (orig. publ. 1661); Boyle, 'Sceptical Chymist', op. cit. note 11, 486. Cf. Boyle, 'Animadversions on Hobbes', op. cit. note 7, 110: here Boyle rejected Hobbes's claim to have observed a phenomenon that Boyle regarded as implausible; Hobbes 'does not here affirm, that he, or any he can trust, has seen the thing done . . . Wherefore, till I be better informed of the matter of fact, I can scarce look upon what Mr. Hobbes says . . . as other than his conjecture . . .'

13. Boyle, 'Some Considerations about the Reconcileableness of Reason and Religion', in *RBW*, Vol. IV, 151-91, at 182 (orig. publ. 1675); see also L.J. Daston, *The Reasonable Calculus: Classical Probability Theory, 1650-1840* (unpublished PhD dissertation, Harvard University, 1979), 90-91; on testimony: Hacking, op. cit. note 2, Chapter 3; on evidence in seventeenth-century English law, see Shapiro, op. cit. note 2, Chapter 5; S. Schaffer, 'Making Certain (essay review of Shapiro), *Social Studies of Science*, Vol. 14 (1984), 137-52, esp. 146-47 (for the legal analogy of scientific witnessing).

14. T. Sprat, *History of the Royal Society* (London, 1667), 100.

15. One of the ways by which Hobbes attacked the experimental programme was to insinuate that the Royal Society was *not* a public place: not everyone could come to witness experimental displays; see T. Hobbes, 'Dialogus physicus de natura aeris . . .', in Hobbes, *Opera philosophica*, ed. Sir William Molesworth, 5 Vols. (London, 1839-45), Vol. IV, 233-96, at 240 (orig. publ. 1661): 'Cannot anyone who wishes come, since as I suppose they meet in a public place, and give his opinion on the experiments which are seen as well as they? Not at all . . . the place where they meet is not public.' (Translation by Simon Schaffer.) Thomas Birch praised Boyle because 'his laboratory was constantly open to the curious'; see *RBW*, Vol. I, cxlv.

16. Boyle, 'New Experiments', op. cit. note 1, 1; Boyle, 'The History of Fluidity and Firmness', in *RBW*, Vol. I, 377-442, at 410 (orig. publ. 1661); Boyle 'Defence against Linus', op. cit. note 11, 173.

17. R. Hooke, *Philosophical Experiments and Observations* (London, 1726), 27-28.

18. Sprat, op. cit. note 14, 98-99; see also Shapiro, op. cit. note 2, 21-22.

19. Boyle, 'New Experiments', op. cit. note 1, 33-34; Boyle, 'A Discovery of the Admirable Rarefaction of Air . . .', in *RBW*, Vol. III, 496-500, at 498 (orig. publ. 1671); Boyle, 'Sceptical Chymist', op. cit. note 11, 460.

20. Boyle, 'The Christian Virtuoso', in *RBW*, Vol. V, 508-40, at 529 (orig. publ. 1690); see also Shapiro, op. cit. note 2, Chapter 5 (esp. 179). For a study of social accounting systems in the evaluation of observation reports, see R. Westrum, 'Science and Social Intelligence about Anomalies: The Case of Meteorites', *Social Studies of Science*, Vol. 8 (1978), 461-93. Explicit concern for the quality of testimony was much more intense in natural history than it was in experimental philosophy. In the latter, access to experimental devices was disciplined by their cost and location; thus, not everyone could *in practice* offer experimental testimony, while those that did were of known character, reliability and probity. By contrast,

the offering of observation reports was almost completely undisciplined, and the reliability of such testimony was a matter of fundamental concern.

21. M. Boas [Hall], *Robert Boyle and Seventeenth-Century Chemistry* (Cambridge: Cambridge University Press, 1958), 40-41; Boyle, 'New Experiments', op. cit. note 1, 2; Boyle, 'The Experimental History of Colours', in *RBW*, Vol. I,
515 662-778, at 633 (orig. publ. 1663). Cf. 664, where certain 'easy and recreative experiments, which require but little time, or charge, or trouble in the making' were recommended to be tried by ladies. Richard Jones was the 'Pyrophilus' to whom other essays were addressed.

22. Boyle 'A Continuation of New Experiments Physico-Mechanical, touching the Spring and Weight of the Air', in *RBW*, Vol. III, 175-276, at 176. This was written in 1668 and printed a year later. Boyle was not being entirely straightforward here: Huygens's air-pump in The Netherlands had in 1662 produced a matter of fact — the so-called anomalous suspension of water — that seriously troubled Boyle's explanatory schema. Boyle never referred to this finding in print; see Shapin and Schaffer, op. cit. note 5, Chapter 6; S. Schaffer, 'Aethers, Air Pumps and Anomalous Suspension', *British Journal for the History of Science* (forthcoming).

23. Boyle, 'A Continuation of New Experiments, Physico-Mechanical . . . The Second Part', in *RBW*, Vol. IV, 505-93, at 505, 507 (orig. publ. 1680).

24. This practice can be contrasted with the iconography of the anti-experimentalist Hobbes whose natural philosophy texts included only a few images of experimental systems, and these very simple and highly stylized. In giving his account of the air-pump and how it worked, Hobbes deliberately scorned the use of pictures; see Hobbes, op. cit. note 15, 235, 242. For studies of engraving and print-making in scientific texts, see W.M. Ivins, Jr, *Prints and Visual Communication* (Cambridge, Mass.: MIT Press, 1969), esp. 33-36, and E.L. Eisenstein, *The Printing Press as an Agent of Change* (Cambridge: Cambridge University Press, 1979), esp. 262-70, 468-71.

25. Hooke to Boyle, 25 August and 8 September 1664, in *RBW*, Vol. VI, 487-90, and R.E.W. Maddison, 'The Portraiture of the Honourable Robert Boyle, FRS', *Annals of Science*, Vol. 15 (1959), 141-214.

26. Boyle, 'Continuation of New Experiments', op. cit. note 22, 178.

27. Unfortunately, this paper was completed before I was able to read Svetlana Alpers's brilliant *The Art of Describing: Dutch Art in the Seventeenth Century* (London: John Murray; Chicago: The University of Chicago Press, 1983). Alpers analyzes the purposes and the conventions of realistic pictures in seventeenth-century Holland, demonstrating substantial links between English empiricist theories of knowledge and Dutch picturing. Her Chapter on 'The Craft of Representation' is a superb examination of the pictorial conventions for generating realist responses. Evidently, the Dutch were trying to achieve by way of picturing what the English were attempting by way of the reform of prose.

28. Boyle, 'New Experiments', op. cit. note 1, 1-2 (emphases added). The role of circumstantial detail in Boyle's prose and in that of other early Fellows of the Royal Society is treated in Shapiro, op. cit. note 2, Chapter 7. See also two excellent unpublished papers: P. Dear, '*Totius in verba*: The Rhetorical Constitution of Authority in the Early Royal Society', typescript, Program in History of Science, Princeton University (a version will shortly appear in *Isis*); and J.V. Golinski, 'Robert Boyle: Scepticism and Authority in Seventeenth-Century Chemistry', paper delivered to conference on Linguistic Aspects of Science, Leeds University, 10-11 January 1984. I am very grateful to Dear and Golinski for allowing me to see their materials.

29. There is probably a connection between Boyle's justification for circumstantial reporting and Bacon's argument in favour of 'initiative' (as opposed to 'magistral') methods of communication in science: see, for example, D.L.
516 Hodges, 'Anatomy as Science', *Assays*, Vol. 1 (1981), 73-89, esp. 83-84; L. Jardine, *Francis Bacon: Discovery and the Art of Discourse* (Cambridge: Cambridge University Press, 1974), 174-78; K.R. Wallace, *Francis Bacon on Communication & Rhetoric* (Chapel Hill, NC: The University of North Carolina Press, 1943), 18-19. The magistral method, as Bacon said, 'requires that what is told should be believed; the initiative that it should be examined.' Initiative methods display the processes by which conclusions were reached; magistral methods mask those processes. Although Boyle's inspiration may, plausibly, have been Baconian, the 'influence' of Bacon is sometimes much exaggerated (for example, Wallace, 225-27). It is useful to remember that it was Boyle, not Bacon, who actually developed the literary forms of experimental communication; it is hard to imagine two more different forms than Bacon's aphorisms and Boyle's experimental narratives. See also a marvellous speculative paper on the *Cartesian* roots of contrasting styles of scientific exposition: J.W.N. Watkins, 'Confession is Good for Ideas', in D. Edge (ed.), *Experiment: A Series of Scientific Case Histories* (London: BBC, 1964), 64-70, and the better-known paper in the same collection by P.B. Medawar, 'Is the Scientific Paper a Fraud?' (7-12).

30. Boyle, 'Proēmial Essay', op. cit. note 10, 305-06; cf. Boyle, 'New Experiments', op. cit. note 1, 1: R.S. Westfall, 'Unpublished Boyle Papers relating to Scientific Method', *Annals of Science*, Vol. 12 (1956), 63-73, 103-17.

31. Boyle 'Unsuccessfulness of Experiments', op. cit. note 12, 339-40, 353; Recognizing that contingencies might affect experimental outcomes was also a way of tempering inclinations to reject good testimony too readily. If an otherwise reliable authority stipulated an outcome that was not immediately obtained, one was advised to persevere; see ibid., 344-45; Boyle, 'Continuation of New Experiments', op. cit. note 22, 275-76; Boyle, 'Hydrostatical Paradoxes', op. cit. note 11, 743; Westfall, op. cit. note 30, 72-73.

32. Boyle, 'New Experiments', op. cit. note 1, 26. For an example of Boyle reporting an experimental failure, see ibid., 69-70. A critic like Hobbes could capitalize upon Boyle's reported failures, or, more interestingly, deconstruct Boyle's reported successes by identifying further contingencies which affected experimental outcomes; see, for instance, Hobbes, op. cit. note 15, 245-46.

33. Boyle, 'Proēmial Essay', op. cit. note 10, 300-01, 307; cf. 'Sceptical Chymist', op. cit. note 11, 469-70, 486, 584. Several of the less modest personalities of seventeenth-century English science were individuals who lacked the gentle birth that routinely enhanced the credibility of testimony: e.g., Hobbes, Hooke, Wallis and Newton.

34. The best source for Boyle's social situation and temperament is J.R. Jacob, *Robert Boyle and the English Revolution: A Study in Social and Intellectual Change* (New York: Burt Franklin, 1977), Chapters 1-2.

35. Boyle, 'Proēmial Essay', op. cit. note 10, 318, 304. For the importance of the lens and the perceptual model of knowledge in seventeenth-century epistemology, see Alpers, op. cit. note 27, Chapter 3. The goal for Boyle, as for many other philosophers concerned with linguistic reform, was *plain-speaking.* For the linguistic programme of the early Royal Society and its connections with experimental philosophy, see F. Christensen, 'John Wilkins and the Royal Society's Reform of Prose Style', *Modern Language Quarterly*, Vol. 7 (1946), 179-87, 279-90; R.F. Jones, 'Science and Language in England of the Mid-Seventeenth Century', *Journal of English and Germanic Philology*, Vol. 31 (1932), 315-31; Jones, 'Science and English Prose Style in the Third Quarter of the Seventeenth Century', *Publications of the Modern Language Association of America*, Vol. 45 (1930), 977-1009; V. Salmon, 'John Wilkins' *Essay* (1668): Critics and Continuators', *Historiographica Linguistica*, Vol. 1 (1974), 147-63; M.M. Slaughter, *Universal Languages and Scientific Taxonomy in the Seventeenth Century* (Cambridge: Cambridge University Press, 1982), esp. 104-86; H. Aarsleff, *From Locke to Saussure: Essays on the Study of Language and Intellectual History* (London: Athlone Press, 1982), 225-77; M. Hunter, *Science and Society in Restoration England* (Cambridge: Cambridge University Press, 1981), 118-19; Shapiro, op. cit. note 2, 227-46; and the sources cited in notes 28 and 29. For Boyle's attack on the 'confused', 'equivocal' and 'cloudy' language of the alchemists, see 'Sceptical Chymist', op. cit. note 11, 460, 520-22, 537-39; and, for his criticisms of Hobbes's expository 'obscurity', see 'Examen of Hobbes', op. cit. note 7, 227.

36. Boyle, 'Proēmial Essay', op. cit. note 10, 307 (emphases in original). On 'wary and diffident expressions', see also 'New Experiments', op. cit. note 1, 2; and compare Sprat, op. cit. note 14, 100-01; J. Glanvill, *Scepsis scientifica* (London: Kegan, Paul, Trench, 1885; orig. publ. 1665), 200-01. For discussions of Boyle's remarks in the context of probabilist and fallibilist models of knowledge, see Shapiro, op. cit. note 2, 26-27; van Leeuwen, op. cit. note 4, 103; Daston, op. cit. note 13, 164-65.

37. Boyle, 'Hydrostatical Discourse', op. cit. note 11, 596.

38. Boyle, 'New Experiments', op. cit. note 1, 2.

39. Boyle, 'Proēmial Essay', op. cit. note 10, 313, 317.

40. On the 'idols' and fallibilism, see Shapiro, op. cit. note 2, 61-62.

41. Boyle, 'Some Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy. The Preface', in *RBW*, Vol. I, 354-59, at 355 (orig. publ. 1661); Boyle, 'Proēmial Essay', op. cit. note 10, 302. On the corrupting effects of 'preconceived hypothesis or conjecture', see Boyle, 'New Experiments', op. cit. note 1, 47; and for doubts about the correctness of Boyle's professed unfamiliarity with the writings of Descartes and other systematists, see Westfall, op. cit. note 30, 63; Laudan, op. cit. note 4, 82n.; M. Boas [Hall], 'Boyle as a Theoretical Scientist', *Isis*, Vol. 41 (1950), 261-68; Boas, 'The Establishment of the Mechanical Philosophy', *Osiris*, Vol. 10 (1952), 412-541, at 460-61; Frank, op. cit. note 8, 93-97. My concern here is not with the veracity of Boyle's professions but with the reasons why he made them.

42. Shadwell's play was performed in 1676. There is some evidence that Hooke believed *he* was the model for Gimcrack; see R.S. Westfall, 'Hooke, Robert', in *Dictionary of Scientific Biography*, Vol. VI, 481-88, at 483. Charles II, the Royal Society's patron, was also said to have found the weighing of the air rather funny.

43. For the extent to which experimental philosophy was, in fact, popular, see Hunter, op. cit. note 35, Chapters 3, 6.

44. This is not intended as an exhaustive catalogue of the measures necessary for institutionalization. Obviously, patronage was required and alliances had to be forged with existing powerful institutions.

45. Boyle, 'Sceptical Chymist', op. cit. note 11, esp. 468, 513, 550, 584.

46. Boyle, 'Proëmial Essay', op. cit. note 10, 303.

47. Boyle's way of dealing with the hermetics drew on the views of the Hartlib group of the 1640s and 1650s. By contrast, there were those who rejected the findings of late alchemy (e.g., Hobbes) and those who rejected the process of assimilation (e.g., Newton).

518 48. Boyle, 'Examen of Hobbes', op. cit. note 7, 233, 197; Boyle, 'Defence against Linus', op. cit. note 11, 122.

49. Boyle, 'New Experiments', op. cit. note 1, 10.

50. Boyle, 'Continuation of New Experiments', op. cit. note 22, 250-58. Note that in other contexts Boyle encouraged speech of immaterial entities such as spirits; what he said was that such items ought to be purged from the routine discourse of experimental philosophy; see, for example, 'Hydrostatical Discourse', op. cit. note 11, 608.

51. Boyle, 'New Experiments', op. cit. note 1, 11-12; cf. Boyle, 'The General History of the Air . . .', in *RBW*, Vol. V, 609-743, at 614-15 (orig. publ. 1692).

52. These problems were structurally similar to those afflicting Newton later in the century. Newton said that he wished to speak of gravitation as a mathematical regularity, without venturing an account of its physical cause. Newton's allies and enemies alike found it difficult to accept such mathematical statements as the end-product of physical inquiry; see A. Koyré, *Newtonian Studies* (Chicago: The University of Chicago Press, 1968), 115-63, 273-82.

53. Boyle, 'Defence against Linus', op. cit. note 11, 135, 137; Boyle, 'Examen of Hobbes', op. cit. note 7, 191, 207; Boyle, 'Animadversions on Hobbes', op. cit. note 7, 112.

54. Hobbes, op. cit. note 15, 271, 273, 278; for Boyle's reply, see 'Examen of Hobbes', op. cit. note 7, 193-94.

55. Cf. Boas [Hall], 'Boyle', op. cit. note 41; Boas, 'Establishment', op. cit. note 41, 475-77.

56. Boyle, 'Proëmial Essay', op. cit. note 10, 312.

57. Ibid., 311.

58. See R.P. Multhauf, 'Some Nonexistent Chemists of the Seventeenth Century: Remarks on the Use of the Dialogue in Scientific Writing', in A.G. Debus and Multhauf, *Alchemy and Chemistry in the Seventeenth Century* (Los Angeles, Calif.: William Andrews Clark Memorial Library, 1966), 31-50.

59. Boyle, 'Sceptical Chymist', op. cit. note 11, 486. In the preface Boyle says that he will not 'declare my own opinion'; he wishes to be 'a silent auditor of their discourses' (460, 466-67).

60. The consensus that emerges is very like the position from which Carneades starts, but the plot of *The Sceptical Chymist* involves disguising that fact. Interestingly, the consensus is not total (a point nicely made by Golinksi, op. cit. note 28): Eleutherius indicates reservations about Carneades's arguments; and Philoponus (a more 'hard-line' alchemist who is absent for the bulk of the symposium) might not, in Eleutherius's opinion, have been persuaded. The obvious contrast is with the form and function of the dialogue in the writings of Boyle's anti-experimentalist adversary Hobbes, especially the *Dialogus physicus, Problemata physica* and *Decameron physiologicum*. Boyle strongly disapproved of Hobbes's dialogues, in which the 'Hobbes' character demanded, and secured, absolute assent from his interlocutor; see Boyle, 'Animadversions on Hobbes', op. cit. note 7, 105.

61. Boyle, 'Sceptical Chymist', op. cit. note 11, 462.

62. Actually, the great bulk of the talk is between Carneades and Eleutherius. The other two participants inexplicably absent themselves from most of the proceedings. This is possibly an accident due to Boyle's self-confessed sloppiness with his manuscripts; he was continually apologizing for losing pages of his drafts.

63. Boyle, 'Sceptical Chymist', op. cit. note 11, 469.

64. Boyle's responses to his adversaries are closely examined in Shapin and Schaffer, op. cit. note 5, Chapter 5.

519 65. Boyle, 'Examen of Hobbes', op. cit. note 7, 190; cf. Boyle, 'Hydrostatical Discourse', op. cit. note 11, 596-97; Boyle, 'Animadversions on Hobbes', op. cit. note 7, 104-05; Boyle, 'Defence against Linus', op. cit. note 11, 118-21.

66. Boyle, 'Defence against Linus', op. cit. note 11, 122; cf. Boyle, 'Examen of Hobbes', op. cit. note 7, 197, 208, 233.

67. Boyle, 'Defence against Linus', op. cit. note 11, 163, 120.

68. Boyle, 'Examen of Hobbes', op. cit. note 7, 186.

69. Ibid., 188; cf. 190.

70. Hobbes, 'Six Lessons to the Professors of the Mathematics . . . in the University of Oxford', in Hobbes, op. cit. note 7, Vol. VII, 181-356, at 356 (orig. publ. 1656).

71. Hobbes, 'Considerations upon the Reputation, Loyalty, Manners, and Religion of Thomas Hobbes', in Hobbes, op. cit. note 7, Vol. IV, 409-40, at 440 (orig. publ. 1662).

72. Boyle, 'New Experiments', op. cit. note 1, 3. The phrase 'wonderful pacifick year' is from Sprat, op. cit. note 14, 58.

73. Sprat, op. cit. note 14, 53, 56.

74. The focus of the paper has been upon an individual, yet its purpose is not individualistic. Boyle was a major innovator and practitioner of the new linguistic technologies. Nevertheless, he proposed them as routine practices for a *community*, and it is clear that Boyle's proposals were widely applauded and implemented, especially in the early Royal Society. His sentiments on this subject were, as I have briefly indicated, echoed by Sprat, Glanvill and Hooke, among others. For further details on the relevant English community of language users, see Shapiro, op. cit. note 2, Chapter 7.

75. B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage, 1979), Chapter 2; and, for a fine study of the role of instruments in scientific observation reports, see T.J. Pinch, 'Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics', *Social Studies of Science*, Vol. 15 (1985), in press.

76. Boyle to R. Moray, July 1662, in Huygens, op. cit. note 9, Vol. IV, 217-20; cf. Boyle, 'Defence against Linus', op. cit. note 11, 152-53.

77. Sprat, op. cit. note 14, 85 (for 'eyes and hands'), 98-99 (for the individual and the collective), 28-32 (for 'tyrants' in philosophy). For the disciplining of the Royal Society's public, see Jacob, op. cit. note 34, esp. 156, and J.R. Jacob, *Henry Stubbe, Radical Protestantism and the Early Enlightenment* (Cambridge: Cambridge University Press, 1983), 59-63; also some brilliantly perceptive remarks in Y. Ezrahi, 'Science and the Problem of Authority in Democracy', in T.F. Gieryn (ed.), *Science and Social Structure: A Festschrift for Robert K. Merton, Transactions of the New York Academy of Sciences*, Series II, Vol. 39 (New York: New York Academy of Sciences, 1980), 43-60, esp. 46-53.

78. See especially the work of Collins whose metaphor of completed and consensual scientific knowledge as 'the ship in the bottle' nicely crystallizes this point: for example, H.M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205-24; Collins, 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', *Social Studies of Science*, Vol. 11 (1981), 33-62. Cf. Latour and 520
Woolgar, op. cit. note 75, 176: 'Our argument is not just that facts are socially constructed. We also wish to show that the process involves the use of certain devices whereby all traces of production are made extremely difficult to detect.' For an historical study making a similar point, see S. Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge, Sociological Review Monograph* No. 27 (Keele, Staffs: Keele University Press, 1979), 139-78.

79. L. Fleck, *Genesis and Development of a Scientific Fact*, trans. F. Bradley and T.J. Trenn, eds Trenn and R.K. Merton (Chicago: The University of Chicago Press, 1979), 103, 105 (orig. publ. in German, 1935).

Steven Shapin, 'Pump and circumstance: Robert Boyle's literary technology', *Social Studies of Science*, vol. 14, no. 4, 1984, pp. 483-511.

Robert Boyle

1a.1(b)

AIR PRESSURE EXPERIMENT

IT hath been admired by very ingenious men, that if the exquifitely polifhed furfaces of two flat pieces of marble be fo congruous to each other, that from their mutual application there will refult an immediate contact, they will ftick fo faft together, that he, that lifts up the uppermoft, fhall, if the undermoft be not exceeding heavy, lift up that too, and fuftain it aloft in the free air. A probable caufe of this fo clofe adhefion we have elfewhere endeavoured to deduce from the unequal preffure of the air upon the undermoft ftone; for the lower fuperficies of that ftone being freely expofed to the air, is preffed upon by it, whereas the uppermoft furface, being contiguous to the fuperior ftone, is thereby defended from the preffure of the air; which confequently preffing the lower ftone againft the upper, hinders it from falling, as we have elfewhere more fully declared. Upon thefe grounds we conjectured, that in cafe we could procure two marbles exactly ground to one another, and in cafe we could alfo fufficiently evacuate our receiver; the lower ftone would, for want of the wonted and fuftaining preffure of the air, fall from the upper. But the farther trial of this experiment we muft, unlefs your Lordfhip think it worth your making at *Paris*, put off till a fitter opportunity. For where we now are, we cannot procure marbles fo exactly ground, that they will fuftain one another in the air above a minute or two, which is a much fhorter time than the emptying of our receiver requires. We did indeed try to make our marbles ftick clofe together, by moiftening their polifhed furfaces with rectified fpirit of wine, in regard that liquor, by its fudden avolation from marble, if poured thereon, without leaving it moift or lefs fmooth, feemed unable to fuftain them together after the manner of a glutinous body, and yet feemed fufficient to exclude and keep out the air. But this we tried to little purpofe, for having conveyed into the receiver two black fquare marbles (the one of two inches and a third in length or breadth, and fomewhat more than half an inch in thicknefs; the other of the fame extent, but not much above half fo thick) faftened together by the intervention of pure fpirit of wine; and having fufpended the thicker by a ftring from the cover, we found not, that the exfuction of the ambient air would feparate them, though a weight amounting to four ounces were faftened to the lowermoft marble to facilitate its falling off.

I WOULD gladly have the experiment tried with marble fo well polifhed, as to need no liquor whatfoever to make them cohere, and in a veffel, out of which the air may be more perfectly drawn than it was out of ours. But in the mean time, though we will not determine whether the spirit of wine did contribute to the strong cohesion of these stones, otherwise than by keeping even the subtlest parts of the air from getting in between them; yet it seemed, that the not falling down of the lowermost marble might, without improbability, be ascribed to the pressure of the air remaining in the receiver: which, as we formerly noted, having been able to keep a cylinder of water, of above a foot in height, from falling to the bottom of the tube, may well enough be supposed capable of keeping so broad a flat marble from descending. And though this may seem a strange proof of the strength of the spring of the air, even when rarefied, yet it will scarce seem incredible to him, that hath observed, how exceeding strong a cohesion may be made betwixt broad bodies, only by their immediate touching one another. A notable instance of which I met with in this short narrative of the learned *Zucchius*: *Juveni* (saith he) *lacertorum suorum robur jactanti proposita semel est lamina ærea, per ansam in medio extantem apprehensam elevanda è tabula marmorea, cui optime congruebat: qui primo tanquam rem ludicram puero committendam contempsit; tum instantibus amicis manum utramque admovens, cum luctatus diu hærentem non removisset, excusavit impotentiam, objecta peregrini & potentissimi glutinis interpositione, quo fortissime copulante nequiret divelli; donec vidit ab alio per tabulam facillimè laminam deduci, & ad extrema productam, & actam in transversum inde deportari.* But that we may learn from our own engine, that two bodies, though they touch each other but in a small part of their surfaces, may be made to cohere very strongly, only by this, that the air presses much more forcibly upon the inferior superficies of the lowermost body, than upon the upper surface of the same; we will hereunto annex the following experiment, though out of the order, wherein they were made. 70

P. Nic. Zucchius apud Schot. part 1. Mec. Hydraulo-pneum.

• • •

We took two flat round marbles, each of them of two inches and about three quarters in diameter, and having put a little oil between them to keep out the air, we hung at a hook fastened to the lowermost a pound weight, to surmount the cohesion which the tenacity of the oil and the imperfect exhaustion of the receiver might give them; then having suspended them in the cavity of a receiver, at a stick that lay horizontally a-cross it; when the engine was filled and ready to work, we shook it so strongly, that those that were wont to manage it, concluded it would not be near so much shaken by the operation. Then beginning to pump out the air, we observed the marble to continue joined, until it was so far drawn out, that we began to be diffident whether they would separate; but at the 16th suck, upon the turning of the stop-cock (which gave the air a passage out of the receiver into the pump) the shaking of the engine being almost, if not quite over, the marble spontaneously fell asunder, wanting that pressure of the air that formerly had kept them together: which event was the more considerable, not only because they hung parallel to the horizon, but adhered so firmly together when they were put in, that having tried to pull them asunder, and thereby observed how close they stuck together, I foretold it would cost a good deal of pains so far to withdraw the air, as to make them separate; which conjecture your lordship will the less wonder at, if I add that a weight of 80 and odd pounds, fastened to the lowermost marble, may be drawn up together with the uppermost, by virtue of the firmness of their cohesion. 275

N. B. THIS is not the only time that this experiment ſucceeded with us; for ſometimes, when they were not ſo cloſely preſſed together before they were put in, the disjunction was made at the 8th ſuck, or ſooner, and we ſeemed to ourſelves to obſerve, that when we hung but half a pound weight to the lower marble, it required a greater exhauſtion of the receiver to ſeparate them, than when we hung the whole pound.

AFTER having proceeded thus far with the inſtruments we then had, meeting with an artificer that was not altogether unſkilful, we directed him to make (what we wanted before in that place) ſuch a braſs-plate to ſerve for a cover or cap to the upper orifice of receivers open at the top, as we have divers times had occaſion to mention already in giving accounts of ſome of the foregoing trials; by the help of which contrivances we proſecuted the newly related experiment much farther than we could do before, as may appear by the following account.

WE faſtened to the lowermoſt of the two marbles a weight of a very few ounces (for I remember not the preciſe number) and having cemented the capped receiver with the marbles in it, as before to the pump, we did by a ſtring, whereof one end was tied to the bottom of this turning-key, and the other to the uppermoſt marble, and which (ſtring) paſſed through the crank or hook belonging to the braſs cover; we did, I ſay, by the help of this ſtring, and by turning round the key, draw up the ſuperior marble, and by reaſon of their coherence the lowermoſt alſo, together with the weight that hung at it: by which means being ſure that the two marbles ſtuck cloſe together, we began to pump out the air that kept them coherent; and after a while, the air being pretty well withdrawn, the marbles fell aſunder. But we having ſo ordered the matter that the lowermoſt could fall but a little way beneath the other, we were able by inclining and ſhaking the engine to place them one upon another again, and then letting in the air ſomewhat haſtily, that by its ſpring it might preſs them hard together, we found the expedient to ſucceed ſo well, that we were not only able by turning the abovementioned cylindrical key, to make the uppermoſt marble take up the other, and the annexed weight; but we were fain to make a much more laborious and diligent exhauftion of the air to procure the disjunction of the marbles this fecond time, than was neceffary to do it at the firft.

And for further prevention of the objections or fcruples that I forefaw fome prepoffeffions might fuggeft, I thought fit to make this further trial; that when the marbles were thus afunder, and the receiver exhaufted, we did, before we let in the air, make the marbles fall upon one another as before; but the little and highly expanded air that remained in the receiver having not a fpring near ftrong enough to prefs them together, by turning the key we very eafily raifed the uppermoft marble alone, without finding it to ftick to the other as before; whereupon we once more joined the marbles together, and then letting in the external air, we found them afterwards to ftick fo clofe, that I could not without inconvenience ftrain any farther, than I fruitlefly did, to pull them fairly afunder; and therefore gave them to one that was ftronger than I, to try whether he could do it, which he alfo in vain attempted to perform.

And now, my lord, though I had thoughts of adding divers other experiments to thofe I have hitherto entertained you with, yet (upon a review) finding thefe to amount already to fifty, I think it not amifs to make a paufe at fo convenient a number; and the rather, becaufe an odd quartianary diftemper that I flighted fo long, as to give it time to take root, is now grown fo troublefome, that I fear it may have too much influence upon my ftyle; which apprehenfion obliges me as well to avoid abufing or diftreffing your lordfhip's patience, as to allow myfelf fome feafonable refrefhment, to referve the mention of the defigned additions until they can with lefs trouble to us both be prefented you by,

My dear lord,

Your lordfhip's moft humble fervant, and affectionate uncle,

Oxford, March 24.
1667.

ROBERT BOYLE.

Robert Boyle, 'New experiments physico-mechanical, touching the spring of the air', *Works*, ed. T. Birch, London, 1772, Vol. I, pp. 69-70; 'A continuation of new experiments physico-mechanical touching the spring and weight of the air', *Works*, Vol. III, pp. 275-6.

H.M. Collins

GRAVITATIONAL RADIATION AND EXPERIMENTERS' REGRESS

Gravitational radiation: 1972

Gravitational radiation can be thought of as the invisible gravitational equivalent of light or other electromagnetic radiation (see, for example, Davies, 1980). Most scientists agree that Einstein's general theory predicts that moving massive bodies will produce gravity waves: however, they are so weak that their detection is very difficult. For example, no one has so far suggested a way of generating detectable fluxes of gravitational radiation on Earth, at least not within the foreseeable future. Nevertheless, it is now accepted that some sensible proportion of the vast amounts of energy generated in the violent events of the universe should be dissipated in the form of gravitational radiation which may be detectable on Earth. Exploding supernovae, black holes and binary stars should produce sizeable fluxes of gravity waves which would show themselves on Earth as a tiny oscillation in the value of 'G' — the constant that is related to the gravitational pull of one object on another.

Just as the planets are attracted to the Sun and to one another by the force of gravity, so are smaller objects. We know that the Earth's gravitational pull is strong enough to keep us firmly anchored to the ground most of the time, but we are also attracted to one another by gravitational forces. We don't stick together because the forces are almost immeasurably small. It was a triumph of experimental science when, in 1798, Cavendish measured the gravitational attraction between two massive lead balls. The attraction between them comprised only one five hundred millionth of their mass! Looking for gravitational radiation is unimaginably more difficult than looking for this tiny force because the effect of a gravity wave pulse is no more than a minute fluctuation within it. For example, one of the smaller antennae (the detectors are often referred to as antennae) that I was shown was encased in a glass vacuum vessel. The core consisted of, perhaps, one hundred kilograms of metal, yet the impact of the *light* from a small flashgun on the mass of metal was enough to send the recording trace off the measuring scale. And this was a fairly insensitive test for one of these devices.

Design of a gravity wave detector

This, then , was a difficult experiment. The standard technique was developed by Professor Joseph Weber (pronounced 'Whebber') of the University of Maryland. He looked for changes in the length (strains) of a massive aluminium alloy bar caused by the changes in gravitational attraction between its parts. Such a bar, often weighing several tons, could not be expected to change its dimensions by more than a fraction of the radius of an electron as a pulse of gravitational radiation passed. Fortunately, the radiation is an oscillation and, if the dimensions of the bar are just right, it will vibrate, or 'ring' like a bell, at the same frequency as the radiation. This means that the energy in the pulse can be effectively integrated or aggregated into something just barely measurable.

A Weber-bar detector, or antenna, comprises the heavy bar with some means of measuring its vibrations. Most designs used strain sensitive 'piezo-electric' crystals glued, or otherwise fixed, to the bar. These crystals produce electricity when they are deformed. In a gravity wave detector the voltage produced is so small as to be almost undetectable. Thus, a critical part of the design is the signal amplifier. Once amplified the signals can be recorded on a chart recorder, or fed into a computer for immediate analysis.

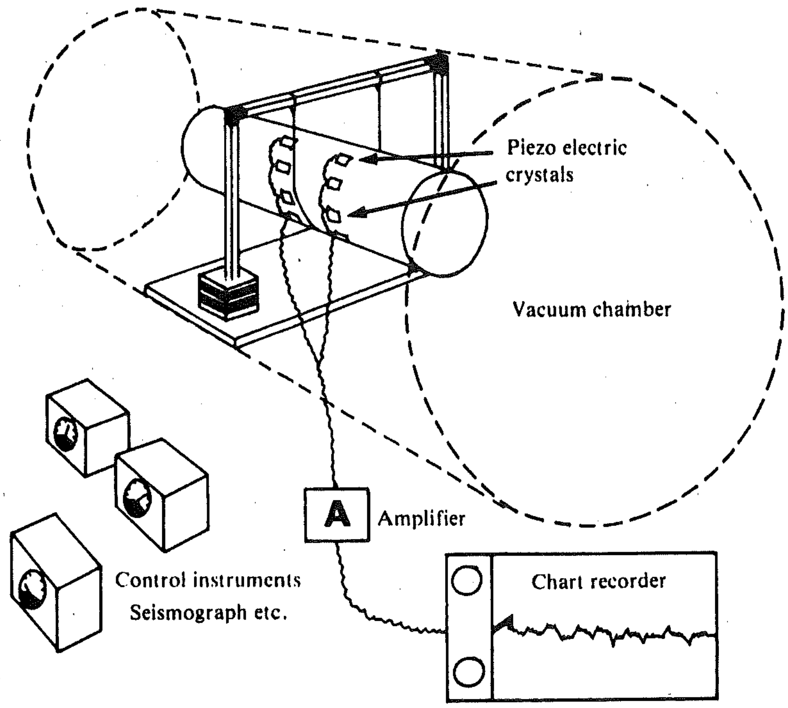
Such devices, of course, cannot distinguish between vibrations due to gravitational radiation and those induced by any other force. Thus, to make a reasonable attempt to detect gravity waves, the bar must be insulated from all other known and potential disturbances such as
electrical, magnetic, thermal, acoustic and seismic forces. Weber 81
attempted to do this by suspending the bar in a metal vacuum chamber on a thin wire. The suspension was insulated from the ground by a series of lead and rubber sheets. (The seismic insulation seems to have been a particularly simple and ingenious solution to what many had thought to be an insoluble problem.)

In spite of these precautions, the bar will not normally be completely quiescent. So long as it is at a temperature above absolute zero, vibrations will be induced in it by the random movements of its own atoms. Thus, the strain gauges will register a continual output of thermal 'noise'. If this were recorded on graph paper by a pen recorder (as it was in many experiments), what will be seen is a spiky wavy line showing random peaks and troughs. A gravity wave would be represented (perhaps) as a particularly high peak, but a decision has to be made about the threshold above which a peak counts as a gravity wave rather than noise. However high the threshold, it must be expected that occasionally a peak due entirely to noise would rise above it. In order to be confident that some gravity waves are being detected, it is necessary to estimate the number of 'accidental' peaks one should obtain as a result of noise alone, then make certain that the total number of above-threshold peaks is still greater. (See technical appendix at end of the chapter for more details of the process of gravity wave detection.) In 1969 Weber claimed to have detected several (about seven) peaks every day which could not be accounted for by noise in the detector.

Status of Weber's claims

Weber's claims are now nearly universally disbelieved. Nevertheless, the search for gravitational radiation goes on, and many current experimental devices are similar to Weber's. Weber's findings were sceptically received because he seemed to find far too much gravitational radiation to be compatible with contemporary cosmological theories. The apparatus now under development to detect fluxes of radiation in line with cosmologists' predictions is meant to be 10^9 (one thousand million) times more sensitive. Thus, though I am going to talk of the extinction of certain claims to have found a new natural phenomenon — gravity waves — it must be understood that I refer only to the phenomenon claimed to have been discovered by Weber — high fluxes of gravity waves.

Weber's detection rate seemed far too great when calculations of the probable sensitivity of his antenna were compared with the amounts of energy, dissipated in the form of gravity waves, that should be generated by cosmic events. If Weber's results were extrapolated, assuming an isotropic (uniform) universe, and assuming that gravitational radiation was not concentrated into the
1661 Hertz (cycles per second) frequency that Weber could best 83
detect, then the amount of energy that was being generated in the cosmos would imply an unreasonably short lifetime. The universe must soon be completely 'burned up' if it were to continue to radiate in this way. These calculations suggested that Weber must be wrong by many orders of magnitude.



Though Weber's first claims were not entirely credible, in the early years of the 1970s he produced a series of ingenious modifications which led other laboratories to attempt to replicate his work. One of the most important new pieces of evidence was that above-threshold peaks could be detected simultaneously on two or more detectors separated by a thousand miles. At first sight, it seemed that only some extraterrestrial disturbance, such as gravity waves, could be responsible for these simultaneous observations. Another piece of evidence was that Weber discovered a periodicity in the disturbances of around twenty-four hours. This suggested that the radiation came from one extraterrestrial direction only. What is more, the periodicity seemed to relate to the Earth's disposition with regard to our galaxy, rather than with regard to the Sun, and this suggested an extra-solar (or galactic) source for the disturbance. (This effect became known as the 'sidereal correlation'; see technical appendix.)

By 1972, at the time of the fieldwork now to be discussed, several other laboratories had built or were building antennae to search for gravitational radiation. Apart from Weber, three others had been operating long enough to be ready to make their own claims. All of these claims were negative.

The experimenters' regress

So far I have described the general principles of the detection of gravitational radiation and a few of its problems. The naïve, but scientifically accomplished, reader might feel that, given some time, he or she now knows how to build a gravity wave detector. What is needed is a vacuum chamber; a heavy bar of aluminium alloy to be suspended within it and insulated from magnetic and electrical forces and from the ground with a lead and rubber pile. Piezo-electric strain gauges must be attached to the bar and their signals amplified and recorded. The whole device can be built in a year or two at the cost of less than £50,000.

• • •

89 **Gravitational radiation: 1975**

After 1972, events favoured Weber's claims less and less. In July 1973 negative results were published by two separate groups (one two weeks after the other) in *Physical Review Letters*. In December 1973, a third group published negative results in *Nature*. Further articles claiming negative results at increased sensitivity were subsequently published by these groups and also by three other groups. No one has since concluded that they found anything that would corroborate Weber's findings.

After 1972 the thrust of experimental activity changed along with the growth of certainty that Weber's results were incorrect. Whereas in 1972 about a dozen groups were engaged in active experimentation directed at Weber's findings, by 1975 no one but Weber was still working in this direction, and even he faced severe funding problems. However, about seven groups were building, or considering the design of, antennae of several orders of magnitude greater sensitivity in the hope of detecting the small, theoretically predicted, radiation flux.

In 1972, a few scientists believed in the existence of high fluxes of gravity waves, and hardly any would openly commit themselves to
their non-existence. By 1975, a number of scientists had spent time 90
and effort actively prosecuting the case against Weber Most of the others accepted that he was wrong and only one scientist other than Weber thought the search for high fluxes worth pursuing. It might be fair to refer to 1975 as, in the words of one of my respondents, belonging to 'the post-Weber era'.

Closure of the debate

The details of the next phase of arguments concerning the existence of gravity waves — the arguments that led to the virtual extinction of the credibility of the high flux claim — have been described at length elsewhere (Collins, 1981c). Here I will give only a brief summary.

While nearly all scientists agreed by 1975 that the fluxes did not exist and that Weber's experiment was not adequate, their reasons differed markedly. Some had become convinced because Weber had made a rather glaring error in his computer programme at one point; others thought that the error had been satisfactorily corrected before too much damage was done. Some thought that the statistical analyses of the level of background noise and the number of residual peaks was inadequate, but others did not think this was a decisive point. Weber had also made a grave mistake when he claimed to have found coincident signals between his own detector and that of an entirely independent laboratory. These coincidences were extracted from the data by comparing sections of tape from the two detectors. Unfortunately for Weber, it turned out that the two sections of tape he compared had been recorded more than four hours apart so that he was effectively conjuring a signal out of what should have been pure noise. Once more, though, it was not hard to find scientists who thought that the damage had not been too great since the level of signal reported was scarcely statistically significant.[2]

Another factor considered important by some was that Weber did not manage to increase the signal-to-noise ratio of his results over the years. In fact, considering that his apparatus was undergoing continual improvement, the net signal seemed to be going down. This was not felt to be typical of new scientific work. What is more, the initially reported sidereal correlation faded away. Again, however, these criticisms were thought to be decisive by only one or two scientists.

Finally, it almost goes without saying that the almost uniformly negative results of other laboratories were an important point. However, all but one of the roughly (when is an experiment not an experiment?) six negative experiments were trenchantly criticized by one or more of Weber's critics! This is not to mention Weber himself who saw all six as inadequate. This should come as no surprise given
the analysis in earlier sections.[3] Only one experiment remained 91
immune to criticism by Weber's critics and this was an experiment designed to be as near as possible a carbon-copy of the original Weber design. Weber's criticism of it turned on differences in the signal processing algorithm (Collins, 1981c).

Thus, one or more of his critics, along with Weber himself, found fault with every one of the arguments and pieces of evidence directed against the high flux claim. Only in the case of one experiment was Weber without an ally in his criticisms of aspects of design.

The demise of high fluxes of gravity waves

Under these circumstances it is not obvious how the credibility of the high flux case fell so low. In fact, it was not the single uncriticized experiment that was decisive: scientists rarely mentioned this in discussion. Obviously the sheer weight of negative opinion was a factor, but given the tractability, as it were, of all the negative evidence, it did not *have* to add up so decisively. There was a way of assembling the evidence, noting the flaws in each grain, such that outright rejection of the high flux claim was not the necessary inference. After all, Weber had spent more time, effort and dedication on the work than anyone else, as some other scientists recognized. One respondent reported:

> ... about that time [1972] Weber had visited us and he made the comment, and I think the comment was apt, that 'it's going to be a very hard time in the gravity wave business,' because he felt that he had worked for ten or twelve years to get signals, and it's so much easier to turn on an experiment and if you don't see them, you don't look to find out why you don't see them, you just publish a paper. It's important, and it just says, 'I don't see them'. So he felt that things were going to fall to a low ebb...

Another experimenter, who had worked with Weber, and was sympathetic to him commented:

> ... [a major difference between Weber and the others is that Weber] spends hours and hours of time per day per week per month, living with the apparatus. When you are working with, and trying to get the most out of things you will find that, [for instance] a tube that you've selected, say one out of a hundred, only stays as a good noise tube for a month if you are lucky, but a week's more like it. Something happens, some little grain falls off the cathode and now you have a spot that's noisy, and the procedures for finding this are long and tedious. Meanwhile, your system to the outside looks just the same.
>
> So lots of times you can have a system running, and you think it's working fine, and it's not. One of the things that Weber gives his system, that none of the others do, is dedication — personal dedication — as an electrical engineer which most of the other guys are not ...
>
> Weber's an electrical engineer, and a physicist, and if it turns out that he's seeing gravity waves, and the others just missed it, that's the answer, that they weren't really dedicated experimenters ... Living with the apparatus is something that I found is really important. It's sort of like getting to know a person — you can, after a while, tell when your wife is feeling out of sorts even though she doesn't know it.

Weber himself remarked that an important factor

> is having someone who is dedicated, who wants to work on the experiment until he's sure it's working properly. I think that's a key issue. I can't recall ever having set up a complex experiment which worked well when it was first turned on ... With the sort of atmosphere and the sort of situation [which we have now] people aren't likely to put themselves out to confirm the earlier results ...

Scientists' awareness of this aspect of experimental work might make them reluctant to put an overall negative construction on a set of negative results. It is clear that the possibility that the high flux claim might still be maintained in the face of this negative evidence was a significant motivating force behind the work of one critic.[4]

Crystallizing the evidence

I will refer to this critic as 'Q'. He had built one of the smallest antennae, though he argued that it was at least as sensitive as Weber's because of its sophisticated design. Nevertheless, other critics nearly always discussed the Q- experiment with reservations because it was so small. But its impact was high because of the way it was presented. As one scientist put it:

...as far as the scientific community in general is concerned, it's probably Q's publication that generally clinched the attitude. But in fact the experiment they did was trivial — it was a tiny thing ... But the thing was, the way they wrote it up ... Everybody else was awfully tentative about it ... It was all a bit hesitant ... And then Q comes along with this toy. But it's the way he writes it up you see.

Another scientist said:

Q had considerably less sensitivity so I would have thought he would have made less impact than anyone, but he talked louder than anyone and he did a very nice job of analysing his data.

And a third:

[Q's paper] was very clever because its analysis was actually very convincing to other people and that was the first time that anybody had worked out in a simple way just what the thermal noise from the bar should be ... It was done in a very clear manner and they sort of convinced everybody.

The first negative results had been reported with circumspection. Scientists discussed all the logical possibilities that could account for
the discrepancy. That Weber's results were spurious was not a 93
publishable certainty. Following closely came Q's outspoken second experimental report with its careful data analysis and the claim that its own results were 'in substantial conflict with those reported by Weber'. Then, as one respondent put it, 'that started the avalanche and after that nobody saw anything.'

The picture that emerges is that the series of experiments made strong and confident disagreement with Weber's results openly publishable but that this confidence came only after what one might call a 'critical mass' of experimental reports had been built up. This mass was 'triggered' by scientist Q.

Q believed from the beginning that Weber was mistaken, and he acted on that belief. Only the most superficial reading would lead to the conclusion that Q's actions were any less sincere than Weber's. It should also be noted that Q had prepared a strategy if high fluxes of gravity waves were found and thus was less closed-minded than a quick reading of the following would suggest. These qualifications should be borne in mind as Q's actions are analysed

These were the other important interventions by Q: the person who actually discovered Weber's computer programming mistake was prepared to allow the matter to remain private so long as Weber cleared things up quickly. However, Q forced discussion out into the open at a conference.

Their discoverer remarked of these mistakes:

As regards the [ ] conference, Q forced my hand, I went to the [ ] conference not intending to mention the computer error unless Weber made a mis-statement ... But when I got there Q presented me with a copy of his remarks already written up, and since I was heading off the session ... I didn't get any lunch that day putting in to what I was going to say what happened, in what I felt was an accurate way without being emotional ... that was the first public announcement.

Another scientist commented:

... I felt that was a very inflammatory issue. It was clearly a case where Weber had tripped himself up because of his data analysis and I felt that it spoke for itself and that those few people who knew about it were enough. But 'Q' did not feel that way and he went after Weber ... and I just stood on the sidelines covering my eyes because I'm not really interested in that kind of thing, because that's not science.

Q also wrote a 'letter' to a popular physics journal which included the paragraph:

[it was shown] that in a ... [certain tape] ... nearly all the so-called 'real' coincidences ... were created individually by this single programming error. Thus not only some phenomenon besides gravity waves *could*, but in
fact *did*, cause the zero-delay excess coincidence rate [in this data]. (Q's 94
stress)

and that

> ... the Weber group has published no credible evidence at all for their claim of detection of gravitational radiation.

Q explained his experimental strategy to me as follows:

> ... what we could have done in the beginning was simply to have analysed Weber's performance and to have shown in principle that he couldn't have detected the gravity waves that he said he was detecting ... We could have argued from the abstract that he couldn't have been detecting them even under ideal circumstances. But we felt that we wouldn't have any credibility if we did that ... and that the only way we could get standing was to have a result of our own.

After completing work and publishing the report on their 'tiny' antenna, the Q-group built a second antenna of greater size and sensitivity but small enough to utilize the same peripheral equipment (vacuum chamber etc). I was interested in their reasons for going ahead with this, if they considered that their first antenna, though small, was large enough to do the job of legitimizing their disproof of Weber's results,

Q himself answered simply in terms of maximum utilization of available equipment. The new experiment cost next to nothing and pushed the upper bound of possible gravity waves down still further. However, another of Q's group answered:

> ... well we knew what was going to happen. We knew that Weber was building a bigger one and we just felt that we hadn't been convincing enough with our small antenna. We just had to get a step ahead of Weber and increase our sensitivity too.
>
> At that point it was not doing physics any longer. It's not clear that it was ever physics, but it certainly wasn't by then. If we were looking for gravity waves we would have adopted an entirely different approach. [eg, an experiment of sufficient sensitivity to find the theoretically predicted radiation] ... there's just no point in building a detector of the [type] ... that Weber has. You're just not going to detect anything, [with such a detector — you know that both on theoretical grounds and from knowing how Weber handles his data] and so there is no point in building one, other than the fact that there's someone out there publishing results in *Physical Review Letters* ... it was pretty clear that [another named group] were never going to come out with a firm conclusion ... so we just went ahead and did it ... we knew perfectly well what was going on, and it was just a question of getting a firm enough result so that we could publish in a reputable journal, and try to end it that way.

The last phrase in the above quotation is particularly significant. Q's group had circulated a paper by Irving Langmuir (1953) to other scientists and to Weber himself. This paper was quoted to me also. The Langmuir paper deals with several cases of 'pathological science' — 'the science of things that aren't so.' Q believed that Weber's work was typical of this genre; he tried to persuade Weber and others of the similarities. Most of the cases cited by Langmuir took many years to settle. As a member of Q's group put it:

> We just wanted to see if it was possible to stop it immediately without having it drag on for twenty years.

They were worried because they knew that Weber's work was incorrect but they could see that this was not widely understood. Indeed the facts were quite the opposite. To quote again:

> Furthermore Weber was pushing very hard. He was giving an endless number of talks ... we had some graduate students — I forget which university they were from — came around to look at the apparatus ... They were of the very firm opinion that gravity waves had been detected and were an established fact of life and we just felt something had to be done to stop it ... It was just getting out of hand. If we had written an ordinary paper, that just said we had a look and we didn't find, it would have just sunk without trace.

In sum, Q and his group set out to kill Weber's findings in the shortest possible time. There is no reason to believe that they had anything but the best motives for these actions but they pursued their aim in an unusually vigorous manner. They did their experiment with the intention of developing a position from which they could more effectively destroy Weber's claims. They would probably not have bothered to carry out any experimental work if it hadn't been that they: 'looked at what some other people were planning to do and decided that there wasn't anybody who was going to make this confrontation.'

Thus, Q acted as though he did not think that the simple presentation of results with only a low key comment would be sufficient to destroy the credibility of Weber's results. In other words, he acted as one might expect a scientist to act who realized that evidence and arguments alone are insufficient to settle unambiguously the existential status of a phenomenon.

I have indicated how the experimenters' regress was resolved in this case. The growing weight of negative reports, all of which were indecisive in themselves, were crystallized, as it were, by Q. Henceforward, only experiments yielding negative results were included in the envelope of serious contributions to the debate. After Q had made his contribution to the transformation in socially acceptable opinion there simply were no high fluxes of gravity waves. Henceforward, all experiments that produced positive results, such as Weber's, must, by that very fact, be flawed. Owning a gravity wave detector was now much more like owning a TEA-laser.

That was one way in which the field changed between 1972 and 1975. We will now look at some other changes.

Content of arguments and the nature of gravitational radiation

Scientists were ready to offer explanations for the differences in the results of the various gravity wave experiments; these ranged from estimates of relative sensitivity through to the personal qualities of the respective experimenters. The complete list of variables that were suggested to me in 1972 as candidates for explaining the differences in results was as follows:

One: *The means of detecting vibration in the bar*

As has been mentioned, Weber used piezo-electric crystals glued around the centre, but other possibilities in use at the time included sandwiching the crystals between parts of the vibrating mass, in different ways, or using a capacitor whose plate separation changed with changes in length of the bar.

Two: *The material of which the bar was constructed*

Some materials are more efficient than others. Some of the latest experiments use single huge crystals of sapphire as the vibrating mass, but in 1972 most experiments used aluminium alloy. The bars did seem to vary according to who had manufactured them and how they had been treated. One experimenter used pure aluminium, which should have been more efficient than alloy, but he had extreme problems with 'creep'; this produced a high level of 'noise'.

Three: *The electronics used to process the signals*

It was suggested that the electronic circuits could be producing the 'signals' themselves, or swamping them in their own noise, or contributing to the appearance of simultaneity in signals from different detectors, or acting as receivers for spurious non-gravitational disturbances.

Four: *The statistical techniques used to extract 'signal' from 'noise'*

As has been pointed out, decisions have to be made regarding the criterion which separates out 'signals' from noise. In the most crude systems, decisions are made by looking at a 'print-out' which will show a number of 'peaks'. Peaks above a certain predetermined level will be counted as 'signals'. This selection process may be done by panels of judges, by the experimenter himself, or by other scientists. Alternatively, computers may be used to do this analysis or an analysis using more sophisticated statistical techniques.

Arguments about the efficiency of these different techniques figured, and were to figure, largely in the debate.

97 Five: *The estimates made of the frequency of 'accidental' peaks and frequency of sensitive states of detector*

As has already been explained, a certain number of spurious peaks are to be expected, due entirely to noise in the system. Estimates must be made of this frequency in order to estimate the number of genuine peaks. Even where separated detectors are looking for coincident signals a certain number of coincidences will be spurious.

However, it is also the case that not all genuine gravity waves will register as coincident peaks. This is because, if the algorithm for extracting genuine peaks is based upon registering only those peaks above a certain threshold, gravity waves which pass when the noise in the bar is at a (random) low point may not excite the bar sufficiently to raise the level of energy above the threshold. Thus coincident peaks, caused by gravity waves, will occur only when both detectors are, by chance, in a sensitive state when the wave passes. Hence the number of coincidences on two detectors must be expected to be less than the number of gravity wave pulses.

Similarly, where more than two detectors are in use, the number of coincidences registered will be still less. Estimates of all these factors affect the conclusions about the number of gravity waves being registered. A high estimate of accidental signals and coincidences will leave no signals to be accounted as gravity waves.

Six: *The frequency of the radiation and the sensitive frequency of the bar*

As has been explained, the resonant bar type devices are most sensitive at their resonant frequency. Not all the experiments used the same resonant frequency so that such a difference might explain differences in results.

Seven: *The length of the bursts of radiation*

Different detectors and statistical algorithms are more or less sensitive to pulses of radiation of different length and wave form. Thus some detectors would not 'notice' very short bursts even if these bursts contained a lot of energy. This was an argument that was to figure largely later on in the debate.

Eight: *Calibration of the apparatus*

In 1972 some scientists complained that Weber had not given sufficient details of the calibration of his detector and thus that it was impossible to be certain about its sensitivity. This is an argument which grew very greatly in importance and, as will be seen below, has significance for the experimenters' regress.

Other arguments were used to try to explain how Weber's findings could be made compatible with broad cosmological considerations or why they might be spurious.

Nine: *The proximity of the source of radiation*

If the source of the radiation were near then large amounts of
98 gravitational energy might be detectable on Earth without implying the embarrassingly large estimates of absolute energy associated with distant sources. The intensity of gravitational radiation, it is assumed, varies over distance in accordance with the inverse square law.

Ten: *The band width of the radiation*

If the incoming radiation were all centred on a narrow band around the 1661 Hertz looked at by Weber then, again, embarrassingly high estimates of energy would be avoided.

Eleven: *Focusing of gravitational radiation*

If the assumption of an isotropic universe is dropped, and it is assumed that gravitational energy is being focused in some way toward the Earth, then again the large estimates can be avoided.

Twelve: *Spurious effects*

Scientists suggested (in 1972) that coincidences between Weber's detectors might be explained by currents in the ionosphere, neutrino fluxes, electric storms and sun spots. By 1975 broadcast television or radio waves, among other things, had been added to the list.

Some less orthodox explanations were also put forward in 1972. These included:

Thirteen: *In some way pulses of gravitational radiation are triggering the release of energy stored in the bar.*

This is a developed version of the generalized notion that gravity waves are coupled to material more strongly than had been thought.

Fourteen: *Explanation of the results of these experiments might require reference to a 'fifth force'.*

That is, some force in addition to the currently known magnetic, gravitational, strong and weak forces.

Fifteen: *The gravity wave findings are solely products of mistakes, deliberate lies, or self-deception.*

Sixteen: *The explanation might require reference to psychic forces.*

This suggestion attributes the non-accidental peaks to (for instance) the desires of an experimenter operating through psychokinesis — the power of mind over matter. A rumour circulated that Weber consulted with J. B. Rhine (the figurehead of scientific research into the paranormal), though both parties deny this. Another experimenter had seen a signal on his detector for the first time immediately after a telephone conversation with Weber and had toyed with the idea that some psychokinetic effect had been involved. Another two or three experimenters had taken an interest in research on 'extra sensory perception' and related effects. Some members of the Parapsychological Association were most interested in the work, and were delighted because they believed that some of the scientists involved were considering the psychokinesis hypothesis seriously. (This was reported to me while I was engaged on research into parapsychology — see Chapter Six. At the time I had no idea that there was any connection between the two fields; far from being a response to any question of mine, the report came as a total surprise to me.) Finally, an experiment was being planned with the collaboration of one of the secondary experimenters to test the ability of a gifted psychic on the apparatus.

None of this later group of explanations ever appeared in print — at least, not under the author's name.

By 1975, the vast majority of these candidate explanations for the discrepancies between one experiment and another had disappeared from the world of scientific discourse. The last four seemed quite bizarre and the range of discussion was restricted, as we have described, to questions of statistical error and the like.

This is exactly the sort of change we would expect to take place as the field reached consensus. As the disturbance brought about in the scientific community by the initial claims was smoothed away, there was no further need to dig deep into the background of 'cherished beliefs' to try to bring a new order to physical reality. Weber was simply wrong. The still, deep waters of everyday life lapped back over the volcano that had thrust through their surface. Today its place is marked by scarcely a ripple.

Consider if the argument had gone another way. Suppose, for a moment, that it was devices that detected gravity waves that had come to be defined as the competent designs. In that case high fluxes of gravitational radiation, or whatever it was that was causing the coincidences in Weber's detectors, would have been *defined* as something that could be seen on an apparatus like Weber's but not seen on the apparatus of his critics. The differences between the two sets of antennae — those that could detect the phenomenon and those that could not — would now *explain* the nature of whatever it was that was causing the coincidences. That is to say, whatever was causing the coincidences must be something of a nature that could affect Weber's antenna but could not affect the antennae of his critics.

For example, take point six above: if the critics were working on a different frequency range, then we would know something about the frequency distribution of the radiation: it must be restricted to the Weber waveband.

If point seven were the crucial difference then they would provide clues about the pulse shape of the radiation, a point which Weber tried to establish.

If the different performances were explained in some way by the catalogue of possibilites under point twelve, then some new, non-gravitational, phenomenon might be what had been discovered by Weber.

100 Finally, discovering that only points thirteen or sixteen could account for the difference in performance between the two sets of antennae would entail something like a revolution in physics. (To talk about 'discovering' such things is disingenuous; one should rather talk of establishing or 'negotiating'. In Chapter Six these different negotiating strategies will be discussed at length.)

As I have explained, the radical possibilities that Weber's work suggested had disappeared from the collective consciousness of physicists by 1975. By then the nature of Weber's claims had been settled: they were simply a mistake of no significance. In the counter-factual circumstances something more startling might have been revealed. I am arguing here that just as the process of deciding whether gravity waves had been detected was coextensive with deciding which set of results was to be believed, so the detailed *nature* of gravity waves was settled at the same time. Different decisions about the quality of the experiments would have gone hand-in-hand with different decisions about the nature of gravity waves. This can be summed up as a ninth proposition:

> *Proposition Nine:* Decisions about the existence of phenomena are coextensive with the 'discovery' of their properties.[6]

An attempt to break the regress: the calibration of experiments

Though the demise of gravity waves has been largely explained, it is worth examining other attempts that were made to break the experimenters' regress. Various non-experimental and 'non-scientific' activities can be seen in this way; if viewed in this way, the conspiratorial activities of some of the scientists trying to discredit high fluxes of gravity waves by discrediting Joseph Weber himself seem much less surprising. The reader should now turn back to the first, third and fifth set of comments of gravity wave researchers in 1972 and look at them this way; it is a matter of reaching for anything in the absence of independent criteria. These 'unscientific' solutions serve a similar purpose to the most abstract theoretical arguments about general relativity or the nature of the cosmos. Both conspiracy and *a priori* theories are attempts to break the regress.[7]

Another episode of the gravity wave story not only illustrates Proposition Nine but shows the circular nature of the regress very clearly. This was scientists' attempts to institute a 'test of a test'as a way of settling the argument. This test was to be the calibration of the competing antennae. If it could be shown that Weber's relative sensitivity was not as great as he claimed, then negative experimental results would have more credibility.

The calibration of instruments is a familiar procedure. Imagine that
101 a prototype voltmeter has been constructed. It consists of a needle which swings across a scale but as yet the scale is blank. To calibrate the instrument known voltages are applied to the terminals and the corresponding positions at which the needle comes to rest are recorded. Thus, marks corresponding to known voltages can be inscribed on the scale. Henceforward the meter may be used to measure unknown voltages; the unknown voltage is applied to the terminals and the mark against which the needle comes to rest gives the answer.

The assumption built into this procedure is that the unknown voltage acts upon the meter in the same way as the standard voltages which were applied to calibrate it. This is so slight an assumption as hardly to be worthy of the name. After all, a voltage is a voltage is a voltage! Nevertheless, it would be correct to say that during the calibration of a voltmeter, standardized voltages are used as a surrogate for as yet unmeasured signals. In more controversial science the assumptions underlying the process of calibration are of greater moment.

Calibrating gravity waves

Some of Weber's critics, in an attempt to short circuit arguments about the relative sensitivity of different experiments, physically calibrated their antennae. They did this by injecting pulses of energy into the bar via an electrostatically energized 'end-plate'. The end-plate could inject tiny vibrations into the bar in a well-understood way. What this calibration procedure amounted to was the use of the antennae to detect a well-understood surrogate phenomenon. It was clear to all that what counted as a well-designed instrument, as defined by this test, was one that would detect the electrostatic pulses; there was no question as to these pulses' existence.

Weber was initially unwilling to calibrate his own antenna in this way. A critic of Weber described the situation as follows:

> We had calibrated our own antennae in a unique way which depended in no way on calculation. So we knew what our sensitivity was and at that time we could only calculate what Weber's sensitivity was. So you're right in saying that the relative sensitivity was something that was on the one hand calculated and on the other hand known to absolute accuracy . . . Soon thereafter we did get an opportunity to go down and calibrate Weber's antenna and we found . . . our calculations were correct . . .

As this respondent suggests, the outcome of the tardy electrostatic calibration of Weber's apparatus was seen by most to be a vindication of the critics' calculations. It was felt to be a decisive demonstration that the sensitivity of the critics' antennae was at least as great as Weber's. In particular, an argument concerning the correct way to process incoming signals seemed to be settled. Weber insisted that the maximum net sensitivity was to be obtained by a non-linear, or energy, algorithm (the algorithm relates to the circuitry and computer program which processes the raw signal). Weber's critics insisted that a linear, or amplitude, algorithm was the best and uniformly made use of the linear algorithm. As one respondent put it:

> For a signal with a sine wave underlying . . . it turns out that a system which is linear can be shown theoretically and quite soundly to be the best system for detecting things. But Weber's always used the non-linear system and so his initial claim was that it was just clearly superior because he finds gravity waves with it whereas people with the linear systems don't. Despite the fact that you can prove rigorously that it's not so.
>
> Well Weber was pushed very hard on this and he finally implemented both systems . . . and he hooked up to the same detector both a linear system and a non-linear system . . . and what he found was that he did indeed find gravity waves more often with his system. However finally after much pushing he put calibrators on — things that could simulate gravity waves — and it turned out that the linear system was about twenty times better at finding the calibrator signal. . . .

In this quotation it is the last phrase that is the most important. Weber did not accept his critics' interpretation of the calibration results. Instead, he claimed that the form of the calibration was inappropriate. Thus:

> *Collins:* In reading your 1974 publication I understand that you did a calibration experiment using both algorithms and that you got a better result with the linear algorithm?
>
> *Weber:* No that's not right. The linear algorithm used by other people is unquestionably superior for short pulses — let me make that absolutely clear. There are certain arguments given for use of the linear algorithm. These arguments are applicable to short pulses and in my opinion they are correct arguments. And the fact that the linear algorithm is not in fact more sensitive is giving us information about the character of the pulse. It means that the character of the pulses do not fit the assumptions which went into that method of analysis . . . so far we think of several kinds of signals that would give results somewhat similar to the ones we see.

Weber's critics read this claim in a less positive light: one remarked:

> What he did was to change the nature of the signals. He said: 'Well, the signals must not be of the form which we've been assuming. They've got to be something else now.' Some strange waveform of which he failed to give a single example. 'And so my algorithm is now best again.' In fact that resolved a lot of difficulties for him. He was wondering why we didn't see his signals. And he said, 'Now I know why. The signals are of a weird form.'

Another respondent, remarking on the failure of Weber's algorithm in the calibrator test, said:

> 103 . . . you have this incredible conflict that when you look for gravity waves the other system seems to do a better job — that's a perfect example of a negative experiment done by the author. It demonstrates that there's nothing there.

One might describe these arguments as turning on the appropriateness of the surrogate signal used for calibration purposes; the assumption that is hardly worth calling an assumption when calibration is carried out in 'normal science' takes on considerable salience in this case.

The force of assumption

To go straight to the end of the story, Weber's interpretation of the calibration results were greeted with scepticism. Weber did manage to invent hypothetical signals compatible with the calibration test; they had a pulse profile such that they would be more easily detected by his antenna, using his algorithm, than by his critics' methods. However, the existence of such signals was thought unlikely by most scientists. According to one respondent, signals with such a profile were 'pathological and uninteresting'. In other words, it would be difficult to think of cosmological scenarios that would give rise to signals with such strange and exact signatures. In the current state of cosmology Weber's hypothesized signal shapes were too 'implausible' to be considered seriously.

To sum up, because of the implausibility of Weber's account of the reasons for the unsuitability of the surrogate calibration signal, the calibration episode made a contribution to the closure of the debate and helped to bring about the demise of high fluxes of gravity waves.[8]

There is, however, a little more that can be said about the case. It was not only the failure of Weber's *ad hoc* hypotheses that allowed the closure, but also the very act of calibration itself. In retrospect, Weber would have served his case better to have maintained his refusal to use electrostatic calibration — not just because the results proved unfavourable but because of the assumptions taken on board by the act of calibration and the restrictions of interpretation imposed as a result.

In bowing to the pressure to calibrate electrostatically, Weber set at least two assumptions beyond question. First he accepted that gravitational radiation would interact with the substance of his antenna in the same way as electrostatic forces. This is certainly a slight assumption, yet, as has been shown in this chapter, there were times when informal discussion took place as to whether gravitational force might be coupling more effectively than expected with the matter of the bar through the release of latent energy via a mysterious mechanism.

104 More importantly, Weber put it beyond question, at least for the time being, that the insertion of a localized pulse into one end of a bar antenna would have a similar effect to the insertion of energy into the bar as a whole from a source at a great distance. Again, this might seem a slight assumption — clearly it was one that Weber did not feel able to dispute — yet more recent events show that it is not inviolable.

An alternative surrogate

An experimenter working on a more modern antenna, whom I interviewed in 1980, planned a different type of calibration. He intended to use as a surrogate not electrostatic force but the fluctuating gravitational attraction induced by a small spinning bar of material located close to the antenna. The rapid changes in gravitational attraction between the material of the antenna and the material of the bar, as their relative dispositions changed, was intended to mimic the high frequency changes in the gravitational attraction of objects for one another symptomatic of gravitational radiation. Though this respondent's apparatus was of a more complex, non-resonant, design than Weber's, his answers to questions about calibration methods are germane:

> *Collins:* What is the advantage of the spinning bar calibration over electrostatic calibration?
> *Respondent:* Well since it couples gravitationally to the antenna it does give you a somewhat more basic measurement — if you like — it's still not really what you want. It still doesn't duplicate the effect of gravitational radiation because it's a near-field effect and the spinning bar really only couples to one end of the thing instead of coupling uniformly to the entire antenna. So this is the limitation of this sort of approach. The spinning bar is more appropriate with something like a Weber resonant antenna where you can more nearly couple to the antenna...
> *Collins:* How certain can you be that electrostatic calibration pulses are acting as an exact analogue of gravity?
> *Respondent:* Oh, they're not. They're certainly not ... From simple measurement [using electrostatic calibration] ... I know precisely the force that I'm applying ... and I can calculate the size of the signal that I ought to get out of the transducers and that's all. But it doesn't mimic the effect of a gravitational wave on the antenna. And that's true whether it's this sort of antenna or whether it's a resonant bar. The fact is that the gravitational wave interacts with all parts of the antenna, with all of the mass of the thing, and there's just no way of reproducing that — at least there's no way I'm able to think up of producing that effect...
> What you are trying to do with electrostatic calibration is to check your theoretical calculations ... What you can't test in this way is the theoretical calculation that tells you precisely what happens when a gravitational wave of a certain amplitude hits the antenna...

For this respondent, with his more complex antenna and his idea for a different method of calibration, the assumptions underlying 105
electrostatic calibration were worth analysing and circumventing if possible. He had thought of a way of circumventing the need for electrostatic impulses using, instead, changes in the gravitational attraction of a local mass. He was still unhappy with the need to use a localized source rather than a powerful distant source that would more nearly mimic effects of gravitational radiation on his antenna. Though he felt that electrostatic calibration would not be as poor a substitute in the case of a Weber bar as it was for his own apparatus, this was only because the analysis relating localized forces to distributed forces seemed simple and plausible in the case of the Weber bar. As he put it, 'there's no argument about it.'

That there was no argument is literally true as I have pointed out above. Weber in accepting electrostatic calibration chose not to argue on these fronts. My respondent's decision to open up the range of possibilities for calibration signals reveals that such an argument might not have been entirely implausible.

Calibration is the use of a surrogate signal to standardize an instrument. The use of calibration depends on the assumption of near identity of effect between the surrogate signal and the unknown signal that is to be measured (detected) with the instrument. Usually this assumption is too trivial to be noticed. In controversial cases, where calibration is used to determine relative sensitivities of competing instruments, the assumption may be brought into question. Calibration can only be performed provided this assumption is not questioned too deeply. In fact, the questioning is constrained only by what seems plausible within the state of the art of the science in question. But the very act of using a calibration surrogate may help to establish the limits of plausibility.

Weber, in accepting the scientific legitimacy of electrostatic calibration for his gravitational antennae, thus accepted constraints on his freedom to interpret results. The act of electrostatic calibration ensured that it was henceforward implausible to treat gravitational forces in an exotic way. They were to be understood as belonging to the class of phenomena which behaved in broadly the same way as the well-understood electrostatic forces. After calibration, freedom of interpretation was limited to pulse profile rather than the quality or nature of the signals.

The anomalous outcome of Weber's experiments could have led toward a variety of heterodox interpretations with widespread consequences for physics. They could have led to a schism in the scientific community or even a discontinuity in the progress of the science. Making Weber calibrate his apparatus with electrostatic pulses was one way in which his critics ensured that gravitational radiation remained a force that could be understood within the ambit of physics as we know it. They ensured physics' continuity — the maintenance of the links between past and future. Calibration is not simply a technical procedure for closing debate by providing an external criterion of competence. In so far as it does work in this way, it does so by controlling interpretative freedom. It is the control on interpretation which breaks the circle of the experimenters' regress, not the 'test of a test' itself.

Notes

2. Interestingly, the level was 2.6 standard deviations. This would count as a high level in the social sciences, but appears to have been counted as inadequate in this area of physics. The reader may recall, from Chapter Two, that choosing an appropriate level of statistical significance called for the invocation of a mysterious murine rule.

3. Production of a 'correct' result is only a necessary, not a sufficient condition for the ascription of competence. Scientists feel free to criticize their colleagues even when their results accord with their own dispositions so long as there are still other negative results to which to refer. A similar phenomenon will be reported in Chapter Five and discussed in Chapter Six.

4. A completely disinterested, honourable and impersonal motive, let me hasten to add, since this critic was firmly convinced that Weber was wrong. His reasons were drawn from physical theory and his exceptionally wide experience as an experimenter in difficult areas.

5. In a talk given to the AAAS in 1978, Kip Thorne referred to this sort of consideration as 'physicists' cherished beliefs'. These are not arbitrary beliefs of course; they are cherished only because giving any of them up would involve giving up so much more of what has proved successful in the physicists' network of concepts. This will be discussed at greater length in Chapter Six.

6. An important consequence of Proposition Nine is that the success of one party to a dispute of this sort *cannot* be explained by their superior grasp of the nature of the phenomenon under investigation. It is this that is being discovered (determined) by the debate itself (cf, Farley and Geison, 1974; Roll-Hansen, 1984).

7. Rosenthal's 'replication accounting' system (Chapter Two) can be thought of in the same way. In spite of his declared intentions of merely adding results irrespective of their origin, Rosenthal had to fall back on measures of experimental quality. For example he did calculations that rested especially heavily on supervised doctoral dissertation work and on studies that contained special controls for minimizing cheating and error. Irrespective of the quality of these categories of experiment (student work is not normally thought of as being among the best), the point is that simply to aggregate experiments touches not at all on the regress; it simply ignores the question of quality and is not a satisfactory solution.

8. One respondent's remarks show that it is only a matter of implausibility, not techical impossibility. He said:

> . . . there is one logical possibility, in a sense, and that is that gravity waves don't behave anything like we think they behave, the whole theory is complete hogwash, and that they have some screwball properties, and by some fantastic chance Weber had just happened to build a detector that somehow or other, in some mysterious fashion, picks these up. . . .

Technical appendix

To detect gravitational radiation, a signal must be separated from noise. Most antennae recorded their data in the form of a spiky chart recorder line. The following appendix explains the techniques for extracting the signal and some of the developments which led sceptical scientists to treat Weber's claims seriously.

Techniques and Innovations in the Search for (hf) Gravity Waves

Figure 1
Signals as Peaks above Threshold

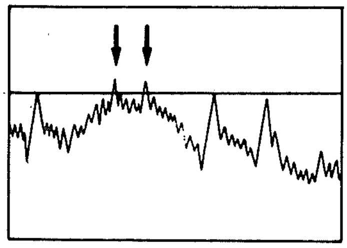
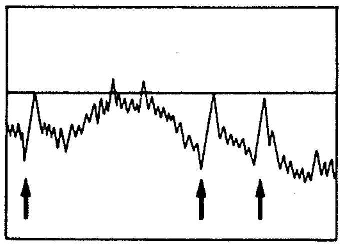


Figure 2
Signals as Sudden Changes in Energy



Even the most well isolated detector will produce a 'noisy' output because of thermal noise in the aluminium alloy bar. Some method of extracting signal from noise has to be used. In the early days Weber counted each peak above a predetermined threshold as signifying a wave pulse (Figure 1). An alternative is to look for sudden changes in the energy of the bar, irrespective of whether or not a threshold is crossed (Figure 2). The latter seems to be a more efficient method. Weber's early analyses of his output were done by 'eyeball'. This was a widely distrusted aspect of his design, though it can be defended. (After all, the eye is much better at pattern recognition than is a computer.) All the later experiments used a computer to do a 'hands off' analysis of data.

H.M. Collins, 'Detecting gravitational radiation: The experimenters' regress', *Changing Order: Replication and Induction in Scientific Practice*, Sage Publications, London, 1985, pp. 79-83, 89-107.

David Bloor

TRUTH AND CONVENTION

26 EXPERIENCE AND BELIEF

The valuable insight of empiricism is its claim that our physiology ensures that some responses to our material environment are common and constant. These responses are called our perceptions. Cultural variation is plausibly thought of as imposed on a stratum of biologically stable sensory capacities. To work with the assumption that the faculty of perception is relatively stable is no retreat from the view that its deliverances do not, and cannot, in themselves, constitute knowledge. This is because experience always impinges on a state of prior belief. It is a cause which brings about an alteration of that state of belief. The resulting state will always arise by compounding the fresh influence with
27 the old state of affairs. This means that experience may bring about change but does not uniquely determine the state of belief.

One way of holding this picture in mind is to draw an analogy with the effect of a force impinging on a system of forces. It will influence but not uniquely determine the resultant force. Think here of the parallelogram of forces. The analogy is illustrated in Figure 1.

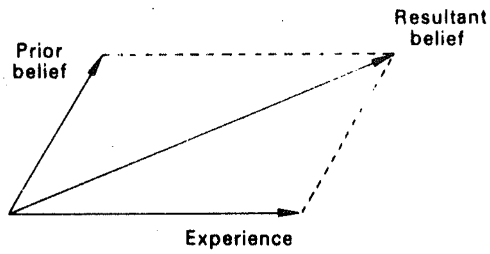


FIGURE 1

As the component which represents experience is made to vary so is the resultant belief. Clearly no value of the experience component corresponds to a unique value of the resultant belief without first fixing the state of the prior belief. This always needs to be taken into account when thinking about what effect an experience will have. Nor does any pattern or sequence of changing experiences in itself determine a unique pattern of changing belief. No wonder that simply observing the world does not allow men to agree about what is the true account that is to be given of it.

Consider the following simple example. A primitive tribesman consults an oracle by administering a herbal substance to a chicken. The chicken dies. The tribesman can clearly see its behaviour and so can we. He says the oracle has answered 'no' to his question. We say the chicken has been poisoned. The same experience impinging on different systems of belief evokes different responses. This applies both at the superficial level of what we might casually say about the event and also at the deeper level of what we believe its meaning to be, and how we would act subsequently.

Scientific examples of the same kind are easy enough to find. The most obvious is perhaps the different meanings which at different times have been put on the daily movement of the sun. The subjective experience of the sun's movement is one in which the horizon acts as a stable frame against which the movement appears to take place. It is plausible and testable to assume that this will be the same for all observers. What is believed about the actual relative positions of the sun and the earth, however, is very different for followers of Ptolemy and followers of Copernicus.

The social component in all this is clear and irreducible. 28
Processes such as education and training must be invoked to explain the enplanting and the distributing of the states of prior belief. They are absolutely necessary if experience is to have a determinate effect. These processes are also necessary for an understanding of how the resultant beliefs are sustained and to account for the patterns of relevance that connect experiences to some beliefs rather than others. Although this view incorporates some of the insights of empiricism it entails that no belief falls outside the sociologist's purview. There is a social component in all knowledge.

Empiricism is currently out of favour in many quarters so is it not ill-advised to incorporate such a blatantly empiricist component into the sociology of knowledge? Should not the sociologist eschew views which have been subject to extensive philosophical criticism? If this means that the sociologist should resolutely keep himself at arm's length from philosophical fashion then it is a sound instinct. But if it means that he should fight shy of ideas just because they are out of favour with philosophers, then it is a recipe for cowardice. Rather, the sociologist and psychologist should exploit whatever ideas are of use to them and put upon them whatever construction suits the purposes in hand.

The version of empiricism that is here being incorporated into the sociology of knowledge is really a psychological theory. It says that our perceptual and thinking faculties are two different things and that our perceptions influence our thinking more than our thinking influences our perceptions. This form of empiricism makes biological and evolutionary sense but it is as much despised by modern empiricists as it is by the modern critics of empiricism. Contemporary philosophers have turned this psychological thesis about two faculties into a claim about the existence and nature of two different languages: the data language and the theoretical language. Or again, they talk of the status of two different sorts of belief: those that are immediately given by experience and are certainly true and those only indirectly connected with experience whose truth is problematic. These are the claims that are currently subject to philosophical debate. The absolute certainty, or even the high probability, of beliefs allegedly derived immediately from experience has been questioned, and more recently so has the whole conception of two different languages (Hesse (1974)).

Let these issues of justification, and of logic and language, be negotiated by philosophers how they will. All that matters for a naturalistic study of knowledge is that it has a plausible and substantial picture of the role of sensory experience. If this happens to be in the same idiom as an old-fashioned, psychological empiricism then so much the better for our philosophical heritage. It shows that it is being taken in the spirit in which it was offered (Bloor (1975)).

MATERIALISM AND SOCIOLOGICAL EXPLANATION 29

No consistent sociology could ever present knowledge as a fantasy unconnected with men's experiences of the material world around him.

Men cannot live in a dream world. For consider how such a fantasy would have to be transmitted to new members of society. It would depend on education, training, indoctrination, social influence and pressure. All of these presuppose the reliability of perception and the ability to detect, retain and act upon perceived regularities and discriminations. Human bodies and voices are part of the material world and social learning is part of learning how the material world functions. If men have the equipment and the propensity to learn from one another they must in principle have the ability to learn about the regularities of the non-social world. In all cultures they do precisely this in order to survive. If social learning can rely on the organs of perception then so can natural or scientific knowledge. No sociological account of science can place the reliability of sense-perception any lower when it is used in the laboratory or on the field trip then when it is used in social interaction or collective action. The whole edifice of sociology presumes that men can systematically respond to the world through their experience, that is, through their causal interaction with it. Materialism and the reliability of sense experience are thus presupposed by the sociology of knowledge and no retreat from these assumptions is permissible.

To illustrate the role of such factors consider the interesting comparison made by J.B. Morrell (1972) of two early nineteenth-century research schools. Morrell compared Thomas Thomson's laboratory at Glasgow with Justus Liebig's at Giessen. Both men pioneered university schools of practical chemistry during the 1820s. Liebig's flourished and became world famous. Thomson's ultimately faded into obscurity and left little mark on the history of the subject. The problem Morrell set himself was to compare and contrast the factors which produced the markedly different fates of the schools despite their similarity in so many respects.

His analysis is conspicuously symmetrical and causal. He proceeds by setting up an 'ideal type' of a research school which incorporated all the factors and parameters which bear upon their organisation and success. Once this model has been erected it then becomes clear how different the cases of Glasgow and Giessen were, despite their common structure. The factors to be taken into account were the psychological make-up of the director of the school; his financial resources and his power and status in his university; his ability to attract students and what he could offer them in terms of motivation and career; the reputation of the director in the scientific community; his choice of field and research programme and the techniques he had perfected for further research.

Thomson was a possessive, sarcastic man who tended to treat the products of his students' labours as if they were his own property.
30 Whilst of course acknowledging their contribution, they would be published in books under Thomson's own name. Liebig could also be a difficult and aggressive man but he was venerated by his students. He encouraged them to publish work under their own names and controlled a journal which provided an outlet for this work. He also offered his students the degree of Ph.D. and other help in their academic and industrial careers. No such useful, rounded educational process was offered in Thomson's laboratory.

At first both directors had to finance the running of their school out of their own pockets. Liebig was the more successful of the two in getting others to finance his laboratory, its materials and staff. He was able to shift this burden on to the state, something that was quite unthinkable in laissez-faire Britain. After some initial difficulty over his status Liebig established himself as a professor at a small university with no distractions from his main work. Thomson was a Regius rather than a College professor at Glasgow and felt an outsider. He was burdened with teaching in the large medical school and dissipated his energy in university chores and politics.

The two directors made markedly different choices in the field of their research. Thomson was quick to see the value and interest of

Dalton's atomic theory and devoted himself to a programme of finding atomic weights and the chemical composition of salts and minerals. One of his major concerns was Prout's hypothesis: that all atomic weights are whole number multiples of the atomic weight of hydrogen. Thomson, then, went into inorganic chemistry. This was a well-worked field, and some of the best practitioners of the time, such as Berzelius and Gay-Lussac were well established in it. Furthermore the techniques involved demanded the very highest skill, and the task of inorganic analysis was beset with many practical problems and complexities. It was difficult to achieve stable, repeatable and useful results.

Liebig chose the new field of organic chemistry. He developed an apparatus and a technique of analysis capable of routinely producing reliable, repeatable findings. Moreover the apparatus could be used by an average, competent and industrious student. In short he was able to set up something like a factory, and it was a factory which produced what nobody in the area had produced before.

Thomson's findings and those of his students frequently ran into the problem that they differed from those of others, and their work was criticised by Berzelius. The school's results sometimes contradicted one another and they were not seen as revealing or useful. Thomson was convinced of the accuracy of his findings but to others they often appeared merely adventitious and unilluminating. By contrast, nobody could gainsay Liebig and his students.

The crucial methodological issue in the present context is to decide what examples such as this say about the role of men's experience of the material world in sociological explanations of science. I shall argue that taking into account the way the material world behaves does not interfere with either the symmetry or the causal character of 31
sociological explanations.

There is no denying that part of the reason why Liebig was a success was because the material world responded with regularity when subject to the treatment given it in his apparatus. By contrast if anyone behaves towards the material world in the precise way in which Thomson did then no such regularity will appear. His procedures presumably cut across and tangled together the physical and chemical processes at work within the substances he examined. The pattern, both of human behaviour and the consequent feedback of experience, is different in the two cases.

The overall style of explanation of the fate of the two research schools is nevertheless identical in the two cases. Both cases have to be understood by reference to an 'input' from the world. Both cases start from the behavioural confrontation of the scientist with a selected part of his environment. In this sense and thus far the two explanations are symmetrical with one another. The account then went on, still quite symmetrically, to deal with the system of existing beliefs, standards, values and expectations on to which these results impinged. Clearly there are different causes at work in the two cases otherwise there would not be different effects. The symmetry resides in the types of causes.

The differences in laboratory findings is just part of the overall causal process which culminated in the different fate of the two schools. It is not in itself a sufficient explanation for these facts. It would not be adequate to say that the facts of chemistry explain why the one programme failed and other succeeded. Given exactly the same laboratory behaviour and the same experimental outcomes the fates of the two schools could have been the other way round. For example, suppose that nobody had been very interested in organic chemistry. Liebig's efforts would have been frustrated, just as the biologist Mendel was frustrated. He would have been ignored. Or conversely suppose that inorganic chemistry had not been so actively studied when Thomson set up his school. His contribution would have stood out more prominently. With the opportunities and encouragement that this higher status would have given, his school may have flourished and gone on to make very different and more lasting contributions. It too may have become a successful factory with reliable methods of production.

There is one situation in which it might be permissible to say that the chemistry alone was the cause of a difference, whether in belief, theory, judgment or, as in this case, the fate of two research schools. This would be where all the social, psychological, economic and political factors were identical, or only differed in minor or irrelevant ways. Even this situation would not really constitute any retreat from the strong programme. It would not make the sociological factors irrelevant for the overall explanation. They would still be vitally active, but merely left unattended to for the moment because they are evenly
32 balanced or 'controlled'. The full structure of the explanation, even in these cases would be just as causal and symmetrical.

TRUTH, CORRESPONDENCE AND CONVENTION

Truth is a very prominent concept in our thinking but so far little has been said about it. The strong programme enjoins sociologists to disregard it in the sense of treating both true and false beliefs alike for the purposes of explanation. It may appear that the discussion in the last section violated this requirement. Put bluntly, didn't Liebig's laboratory flourish because it really discovered truths about the world, and didn't Thomson's fail because of the errors in his findings? The fate of these enterprises surely depended on matters of truth and falsity, so these appear to play a central role after all. The link between truth and the strong programme must be clarified, especially for those parts of the programme which stress the causal promptings of the world as they appear in experimental results and sensory experiences.

There is little doubt about what we mean when we talk of truth. We mean that some belief, judgment or affirmation corresponds to reality and that it captures and portrays how things stand in the world. Talk of this kind is probably universal. The need to reject what some men say, and affirm what others say, is basic to human interaction. It may seem unfortunate, then, that this common conception of truth should be so very vague. The relation of correspondence between knowledge and reality on which it hinges is difficult to characterise in an illuminating way. A variety of words like 'fit', 'match', or 'picture' suggest themselves, but one is hardly better than another. Instead of trying to define the concept of truth more sharply a different approach will be adopted. This is to ask to what use the concept of truth is put and how the notion of correspondence functions in practice. It will transpire that the vagueness of the concept of truth is neither surprising nor any hardship.

To make the issue tangible consider again the example of the phlogiston theory. Phlogiston was tentatively identified with the gas we call hydrogen. The chemists of the eighteenth century knew how to prepare this gas but their conception of its properties and behaviour was very different to ours. They believed, for example, that phlogiston would be absorbed by a substance they called 'minium' or 'lead calx' - or what we would call 'lead oxide'. Furthermore they believed that when it absorbed phlogiston the minium would turn into lead (cf. Conant (1966)).

Joseph Priestley was able to provide a convincing demonstration of this theory. He took an inverted gas jar filled with phlogiston which was trapped over water (see Figure 2). Floating on the water was a crucible containing some minium. This was heated by using the sun's rays concentrated by a burning glass. The result was exactly what he expected. The minium turned into lead, and as an indication that it had
33 absorbed the phlogiston the water level in the gas jar rose dramatically. Here surely was a demonstration that the theory corresponded with reality.

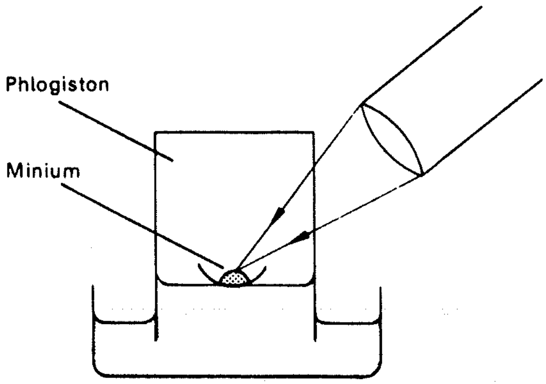


FIGURE 2 The absorption of phlogiston by lead calx

An empiricist would rightly point out that we can see the water level rise but we do not actually see the phlogiston absorbed into the minium. There is no experience of seeing the gas rush into tiny pores or crevices in its surface, as we might see bath water rush down a plughole. So the reality that the theory postulates is not visibly in accord with the theory. We do not have access to this area of the physical world so we cannot see the correspondence with the theory.

The indicator of truth that we actually use is that the theory works. We are satisfied if we achieve a smoothly operating theoretical view of the world. The indicator of error is the failure to establish and maintain this working relationship of successful prediction. One way of putting this point would be to say that there is one sort of correspondence that we do indeed use. This is not the correspondence of the theory with reality but the correspondence of the theory with itself. Experience as interpreted by the theory is monitored for such internal consistency as is felt important. The process of judging a theory is an internal one. It is not internal in the sense of being detached from reality, for obviously the theory is connected to it by the way we designate objects, and label and identify substances and events. But once the connections have been established the whole system has to maintain a degree of coherence; one part must conform to another.

The experiment described above in fact threw up problems as well as support for the phlogiston theory. Priestley eventually noticed that some drops of water had formed inside the gas jar during the experiment. Since he had done the experiment over water these may have passed unnoticed at first. They had certainly not been expected and their presence indicated trouble for the theory. Nothing in the theory had said that water would be formed, but repeating the experiment over mercury made it quite clear that it was. Now a lack of correspondence had emerged.

No glimpse behind the scenes was needed to evoke this awareness of non-correspondence. Reality had not deemed the theory false because of a lack of correspondence with its inner workings. What had happened was that an anomolous situation had emerged within a given theoretical conception of the experiment. What Priestley did was to remove the anomoly by elaborating the theory. Once again, it was not reality that was his guide here but the theory itself, it was an internal process. He reasoned that the minium must have contained some water that nobody had realised was there. When it was heated this water emerged and appeared on the sides of the gas jar. He had made a discovery about the role of water, and correspondence was now re-established.

It is interesting to compare Priestley's analysis of this experiment with our version, because as far as we are concerned his theory, and even more so its adjusted version, does not correspond with reality at all. We do not say that the phlogiston was absorbed into the minium or that the water emerged from the minium. We say that the gas in the jar is hydrogen and that the minium is lead oxide. On heating, the oxygen comes out of the oxide leaving the lead. This oxygen then combines with the hydrogen to form water. During this formation the gas is used up and so the level of either mercury or water in the gas jar rises.

We see exactly what Priestley saw but conceive it theoretically in a quite different way. We, no more than Priestley, have been permitted access to the hidden aspects of reality, so our view is just as much a theory. Doubtless we are fully justified in preferring our theory because its internal coherence can be maintained over a wider range of theoretically interpreted experiments and experiences.

It is now possible to see why the relation of correspondence between a theory and reality is vague. At no stage is this correspondence ever perceived, known or, consequently, put to any use. We never have the independent access to reality that would be necessary if it were to be matched up against our theories. All that we have, and all that we need, are our theories and our experience of the world; our experimental results and our sensory-motor interactions with manipulatable objects. No wonder that the terminology which refers to this inscrutable relation is vague, but a supposed link which plays no real part in our thinking can afford to be left vague for nothing is lost.

The processes of scientific thought can all proceed, and have to proceed, on the basis of internal principles of assessment. They are moved by the perception of error as it crops up within the terms of our theories, purposes, interests, problems and standards. Had Priestley not been concerned to develop a detailed account of all the events that he could detect in a chemical reaction he would have thought no more about
35 a few drops of water if he noticed them. Similarly had we not been intent on getting more and more general theories we could have stayed content with Priestley's version. It corresponds to reality well enough for some purposes. This correspondence is only disturbed if it runs up against our requirements. The motor of change is internal to these requirements, and our theories and experience. There are as many forms of correspondence as there are requirements.

This poses a problem about the notion of truth, for why not abandon it altogether? It should be possible to see theories entirely as conventional instruments for coping with and adapting to our environment. Given that they are subject to our varying requirements of accuracy and utility, their use and development would appear to be fully explicable. What function does truth, or talk of truth, play in all this? It is difficult to see that much would be lost by its absence. There is no doubt however that it is a terminology which comes naturally and is felt to be peculiarly apt.

Our idea of truth does a number of jobs which are worth noting if only to show that they are compatible with the strong programme and the pragmatic and instrumental idea of correspondence which has emerged in the discussion. First, there is what may be called the discriminatory function. Men are under the necessity to order and sort their beliefs. They must distinguish those which work for them from those which do not. 'True' and 'false' are the labels typically used and are as good as any, although an explicitly pragmatic vocabulary would function just as well.

Second there is the rhetorical function. These labels play a role in argument, criticism and persuasion. If our knowledge were purely under the control of stimulation from the physical world there would be no problem about what to believe. But we do not mechanically adapt to the world because of the social component in our knowledge. This conventional and theoretical apparatus presents a continuing problem of maintenance. The language of truth is intimately connected with the problem of cognitive order. On the one hand, men talk of truth in general so that they may recommend this or that particular claim. On the other hand, truth is invoked precisely as an idea of something potentially different from any received opinion. It is thought of as something that transcends mere belief. It has this form because it is our way of putting a question mark against whatever we wish to doubt or change or consolidate. Of course, when men affirm truth or detect and denounce error, there is no need for them to have privileged access or ultimate insight into these things. The language of truth has never needed this. It was as available, and as legimately available to Priestley with his phlogiston theory, as it is to us.

This is all very similar to the discriminatory function except that now the labels can be seen taking on overtones of transcendence and authority. The nature of the authority can be identified immediately. In as far as any particular theoretical view of the world has authority this can only derive from the actions and opinions of men. This is precisely where 36
Durkheim located the obligatory charecter of truth when he criticised the pragmatist philosophers (see the selections in Wolff(1960) and Giddens (1972)). Authority is a social category and only men can exert it. They endeavour to transmit it to their settled opinions and assumptions. Nature has power over us, but only men have authority. In some measure the transcendence associated with truth will have the same social source, but it also points to the third function of the notion of truth.

This is what may be called the materialist function. All our thinking instinctively assumes that we exist within a common external environment that has a determinate structure. The precise degree of its stability is not known, but it is stable enough for many practical purposes. The details of its working are obscure, but despite this, much about it is taken for granted. Opinions vary about its responsiveness to our thoughts and actions, but in practice the existence of an external world-order is never doubted. It is assumed to be the cause of our experience, and the common reference of our discourse. I shall lump all this under the name of 'materialism'. Often when we use the word 'truth' we mean just this: how the world stands. By this word we convey and affirm this ultimate schema with which we think. Of course this schema is filled out in many different ways. The world may be peopled with invisible spirits in one culture and hard, indivisible (but equally invisible) atomic particles in another. The label of materialism is appropriate in as far as it emphasises the common core of people, objects and natural processes which play such a prominent role in our life. These common and prominent examples of an external nature provide the models and exemplars by which we give sense to more esoteric cultural theories. They provide our most enduring, public and vivid experience of externality.

This third function of the notion of truth can be used to overcome an objection that may be pressed against my analysis. I have said that men choose or question or affirm and that they count as true whatever is the outcome of these processes. This may appear to be arguing in a circle for can these processes be described without presupposing the notion of truth? Don't men question in the name of truth, and affirm what they think is true? Surely it is wrong to use the notion of affirming to explain the notion of truth; rather the idea of truth is needed to make sense of affirmation. The answer is that what is needed to make sense of affirmation is the instinctive but purely abstract idea that the world stands somehow or other, that there are states of affairs which can be talked about. This is what is provided by the schema of ideas that I have called the materialist presupposition of our thinking. All matters of substance, all issues of particular content, have to be fought out in their own terms and independently. Whoever wins these struggles for power helps himself to the victor's crown. In practice, therefore, the choices and affirmations do have priority.

(The general idea of truth should never be confused with the 37
standards that are used in any particular context to judge whether a particular claim is to be accepted as true. This would be to assume that the mere notion of truth can act as a substantial criterion of truth. This mistake is central to Lukes's (1974) anti-relativist claims.)

That men should sort and select beliefs, that they should affirm them and garland concensus with authority, and that they should instinctively relate beliefs to an external environment of causes is all very easy to accept. And it is all in conformity to the strong programme. In particular the assumption of a material world with which men establish a variety of different adaptations is exactly the picture presupposed by the pragmatic and instrumentalist notion of correspondence. The points that have emerged can now be related, quickly, to the problem posed by Liebig and Thomson.

When we invoke truth and falsity to explain the differential success of Liebig and Thomson we are using these terms to label the different circumstances in which these men found themselves. Liebig could generate repeatable results. He had hit upon a way of eliciting a regular response from nature. Thomson had not. If one man can grow apples with no grubs in them and another cannot then, of course this may explain their differing economic fortunes - given a certain backdrop of market preferences. Using the language of truth and falsity in order to mark such a distinction in the case of scientific work is customary and acceptable. It is an amalgam of the functions that have just been spelled out. It highlights causally relevent circumstances and their relation to cultural preferences and purposes. It would be a disaster for the strong programme if it were at odds with this usage of the language of truth and falsity. But it is not. The use to which it is opposed is quite different viz. making an evaluation of truth and falsity and then, contingent upon that evaluation, adopting different styles of explanation for true and false beliefs. For example, using causal explanations for error but not for truth. This is a very different matter. It assimilates the notion of truth to a teleological framework rather than leaving it within the causal idiom of our everyday thinking.

The idea that scientific theories, methods and acceptable results are social conventions is opposed by a number of typical arguments which must now be examined. It is often assumed that if something is a convention then it is 'arbitrary'. To see scientific theories and results as conventions is said to imply that they become true simply by decision and that any decision could be made. The reply is that conventions are not arbitrary. Not anything can be made a convention, and arbitrary decisions play little role in social life. The constraints on what may become a convention, or a norm, or an institution, are social credibility and practical utility. Theories must work to the degree of accuracy and within the scope conventionally expected of them. These conventions are neither self-evident, universal or static. Further, scientific theories and procedures must be consonant with other conventions and purposes prevalent in a social group. They face a 'political' problem of acceptance like any other policy recommendation.

The question may be pressed: does the acceptance of a theory by a social group make it true? The only answer that can be given is that it does not. There is nothing in the concept of truth that allows for belief making an idea true. Its relation to the basic materialist picture of an independent world precludes this. This schema permanently holds open the gap between the knower and the known. But if the question is rephrased and becomes: does the acceptance of a theory make it the knowledge of a group, or does it make it the basis for their understanding and their adaptation to the world? - the answer can only be positive.

Another objection to seeing knowledge resting on any form of social consensus derives from the fear that critical thought is endangered. It has been said that on such views radical criticism is impossible (Lukes (1974)). What the theory in fact predicts is that the radical criticism of the knowledge of a social group will only be possible in certain situations. These are, first, that more than one set of standards and conventions are available, and more than one definition of reality can be conceived; the second is that some motives exist for exploiting these alternatives. In a highly differentiated society the first condition will always be satisfied. In science however the second condition will not always be satisfied. Sometimes scientists will calculate that more is to be gained by conformity to normal procedures and theories than by deviance. The factors which enter into that calculation constitute a sociological and psychological problem in their own right.

A simple example will serve to convey the general point that conventions do not stand in the way of radical criticism. Indeed without them such criticism would be impossible. Francis Bacon was one of the great propagandists of science. He along with others was a bitter critic of what he saw as the degenerate scholasticism of the universities. In its place he wanted to see the form of knowledge associated with the craftsman and artisan which was useful, practical and active. He thus used the standards, habits and interests and conventions of one section of society as the yardstick with which to measure other types of learning. He did not search for, nor would he have found, any supra-social standards. There is no Archimedian point.

If the condition of reflexivity is to be satisfied it ought to be possible to apply this whole account to the sociology of knowledge itself without in any way undermining it. This certainly is possible. There is no reason why a sociologist or any other scientist should be ashamed to see his theories and methods as emanating from society, that is, as the product of collective influences and resources and as peculiar to his culture and its present circumstances. Indeed if a sociologist tried to evade this realisation he would be denigrating the subject-matter of his own science. There is certainly nothing about such an admission that entails that science should be unresponsive to experience or careless of facts. After all, what are the conventional requirements currently imposed by the social milieu on any science? They are what we take for granted as the scientific method as it is practised in the various disciplines.

To say that the methods and results of science are conventions does not make them 'mere' conventions. This would be to commit the unspeakable blunder of thinking that conventions are things that are trivially satisfied and essentially undemanding. Nothing could be more mistaken. Conventional demands frequently stretch men to the very limit of their physical and mental capacities. An extreme case will serve as a reminder: think of the feats of endurance that North American Indian males were said to undergo in order to be fully initiated warriors of their tribe. That theories and scientific ideas be properly adapted to the conventional requirements that are expected of them means, among other things, that they make successful predictions. This is a harsh discipline to impose on our mental constitution, but it is no less a convention.

Doubtless the feeling will linger that some form of lewdness has been committed. It will still be said that truth has been reduced to mere social convention. This feeling is the motive force behind all the detailed arguments against the sociology of knowledge that have been examined in the last two chapters. These arguments have been faced and rejected, but perhaps the feeling remains. Let us therefore take it as a phenomenon in its own right and try to explain its presence. Its very existence may reveal something interesting about science - for something in the nature of science must provoke this protective and defensive response.

David Bloor, *Knowledge and Social Imagery*, Routledge & Kegan Paul, London, 1976, pp. 26-39.

1a.4

Steven Shapin and Simon Schaffer

HOBBES VERSUS BOYLE

... the laws of inference can be said to compel us; in the same sense, that is to say, as other laws in human society.
WITTGENSTEIN, Remarks on the Foundations of Mathematics.

ROBERT Boyle's *New Experiments Physico-Mechanical* was published in the summer of 1660. Following the Restoration of the King in May 1660 and the gathering of "many Worthy Men" in London during the summer of that year, the Royal Society received a formal constitution at Gresham College in November 1660.[1] Hobbes was now faced, not with experiment merely as a useful adjunct to the pursuit of natural philosophy, but with a fully developed experimental programme for natural philosophy. Publications on trials of the air-pump and on other related experiments were shortly to come from Henry Power, Robert Hooke, John Wallis, and, of course, from Boyle himself, who began to produce a profusion of tracts on the new experimental philosophy.[2] Boyle and his colleagues now argued that no philosophy of nature could hope to establish a solid foundation for assent unless it was grounded in experimental practices: the procedures set forth in *New Experiments* and the essays that quickly followed its publication. In December

[1] The phrase "many Worthy Men" is Thomas Sprat's description of the returning royalist exiles. He continues: they "began now to imagine some greater thing; and to bring out experimental knowledge, from the *retreats*, in which it had long hid it self, to take its part in the *Triumphs* of that universal Jubilee. And indeed Philosophy did very well deserve that Reward: having been always Loyal in the worst of times." Sprat, *History*, pp. 58-59. Hobbes, however, reckoned himself ("the first of all that fled") to be at least as loyal as the experimental philosophers: Hobbes, "Considerations on the Reputation of Hobbes," p. 414.

[2] These tracts include: Cowley, *Proposition for the Advancement of Experimental Philosophy* (1661); Hooke, *Attempt for the Explication of the Phaenomena* (1661); idem, *Micrographia* (1665; commissioned March 1663); Power, *Experimental Philosophy* (1664; written by August 1661); Wallis, *Hobbius heauton-timorumenos* (1662); plus all the writings that Boyle published in 1660-1662. See also M. B. Hall, "Salomon's House Emergent," esp. pp. 180-182.

1660 the Society meeting at Gresham announced that it would limit its membership to fifty-five, plus those of the rank of baron and above, and that it had received royal approval from Charles II.[3] Hobbes immediately responded to Boyle and these changed circumstances: the *Dialogus physicus de natura aeris* was published in August 1661.[4]

Hobbes's criticisms of Boyle's work and of the experimental programme took several major forms:

- He was sceptical about the allegedly public and witnessed character of experimental performances, and, therefore, of their capacity to generate consensus, even within experimental rules of the game.

- He regarded the experimental programme as otiose. It was pointless to perform systematic series of experiments, since if one could, in fact, discern causes from natural effects, then a single experiment should suffice.
- He denied the status of "philosophy" to the outcome of the experimental programme. "Philosophy" for Hobbes was the practice of demonstrating how effects followed from causes or of inferring causes from effects. The experimental programme failed to satisfy this definition.
- He systematically refused to credit experimentalists' claims that one could establish a procedural boundary between observing the positive regularities produced by experiment (facts) and identifying the physical cause that accounts for them (theories).
- He persistently treated experimentalists' "hypotheses" and "conjectures" as statements about real causes.
- He contended that, whatever hypothetical cause or state of nature Boyle adduced to explain his experimentally produced phenomena, an alternative and superior explanation could be proffered and was, in fact, already available. In particular, Hobbes stipulated that Boyle's explanations invoked vacuism. Hobbes's alternatives proceeded from plenism.
- He asserted the inherently defeasible character of experimental

[3] Three months later, on 20/30 March 1661, it "was resolved, that the number of the members of the society be enlarged," and by 20/30 May 1663 there were 115 Fellows: Birch, *History*, vol. 1, pp. 5, 19, 239-240.

[4] Hobbes's two other specifically anti-experimentalist treatises were *Problemata physica* (1662) and *Decameron physiologicum* (1678). The former appeared only in Latin in Hobbes's lifetime, and was republished in Amsterdam in 1668. *Problemata physica* was translated and published in English as *Seven Philosophical Problems* (1682). We quote from the English Molesworth edition.

systems and therefore of the knowledge experimental practices 112
produced. Hobbes noted that all experiments carry with them a set of theoretical assumptions embedded in the actual construction and functioning of the apparatus and that, both in principle and in practice, those assumptions could always be challenged.[5]

Experimental Spaces

In his dedication of the *Dialogus physicus* to Samuel Sorbière Hobbes identified his opponents as a collectivity and the air-pump as their emblematic device:[6]

> Those Fellows of Gresham who are most believed, and are as masters of the rest, dispute with me about physics. They display new machines, to show their vacuum and trifling wonders, in the way that they behave who deal in exotic animals which are not to be seen without payment. All of them are my enemies.[7]

Who were these Fellows at Gresham, how many were they, and how did they come to be in this place? The "experimentalist" interlocutor in Hobbes's dialogue replied. They were

[5] The resonance with the "Duhem-Quine" thesis is intentional. We shall see that Hobbes's particular objections to Boyle's experimental systems provide a concrete exemplar of this "modern" thesis concerning the impossibility of crucial experiments; see Duhem, *Aim and Structure of Physical Theory*, chap. 6; Quine, *From a Logical Point of View*, esp. pp. 42-44.

[6] Samuel Sorbière (1615-1670) was a French physician who had been involved in the founding of the Montmor Academy and, later, of the Académie Royale des

Sciences. He had translated some of Hobbes's work and had corresponded with Huygens, with whom he was elected to the Royal Society in June 1663. On his return to France Sorbière wrote *Relation d'un Voyage en Angleterre* (Paris, 1664), which angered the Royal Society by describing it as divided into sects. The Society considered cancelling Sorbière's membership, and Sprat replied with *Observations on Monsieur de Sorbière's Voyage* (London, 1665). On Sorbière and these episodes, see Cope and Jones, "Introduction [to Sprat, *History*]," pp. xviii; Sorbière to Oldenburg, 5/15 December 1663, in Oldenburg, *Correspondence*, vol. II, pp. 133-136, esp. p. 135n; Birch, *History*, vol. II, pp. 456-459; Guilloton, *Autour de la 'Relation' du Voyage de Sorbière*; "Memoirs for the Life of Sorbière," in Sorbière, *Voyage to England*, pp. i-xix.

7 Sorbière, *Voyage to England*, pp. 236-237. Note that the sentence "All of them are my enemies" was not in the original 1661 text, but was added for the 1668 Amsterdam edition of Hobbes's *Opera philosophica*. This indicates that Hobbes's view of the Royal Society and the experimental programme had, if anything, hardened as a result of his exchanges with Boyle.

113 About fifty men of philosophy, most conspicuous in learning and ingenuity [*ingenio*], [who] have decided among themselves to meet each week at Gresham College for the promotion of natural philosophy. When one of them has experiences [*experientiae*] or methods [*artis*] or instruments for this matter, then he contributes them. With these things new phenomena are revealed and the causes of natural things are found more easily.

Hobbes proceeded directly to the question of whether this new experimental space was in fact open and public. He asked, why just fifty men? "Cannot anyone who wishes come, since, as I suppose, they meet in a public place, and give his opinion on the experiments [*experimenta*] which are seen, as well as they?" Interlocutor answered: "Not at all." Hobbes persisted: "By what law would they prevent it? Is this Society not constituted by public privilege?" He forced out of interlocutor the telling admission that "the place where they meet is not public." And Hobbes concluded, therefore, that its experiments were not, in practice, available to be witnessed by everyone, but only by a self-selected few: "If it pleased the master of the place, they could make one hundred men from the fifty."[8]

This was a damning judgment for two reasons: first, Hobbes showed that the experimentalists did not, as they appeared to claim, occupy a public space. Access was in fact restricted, and, because of that, the witnessing of experiments, upon which the making of matters of fact depended, was a private and, possibly, a partial affair. How do we know that these *are* authentic matters of fact if they are generated within a private space? Second, Hobbes insisted that the space occupied by the Gresham experimentalists had a "master." It had a master who decided who could come in and who could not. And it also had "masters of the rest": those "who are most believed." Hobbes had a vivid image of what it might mean to be a "master" of a philosophical place. He recalled his personal experiences of the meetings in Paris in the 1640s "at the convent of the Minims." Father Mersenne presided, and "whoever might have demonstrated a problem, would produce it for him to be

8 Hobbes, "Dialogus physicus," p. 240. We shall discuss Hobbes's use of the dialogue form below. For the present, note that it is the experimentalist interlocutor (B) who describes the Greshamites as "men of philosophy," not Hobbes (A). In view of the date at which the *Dialogus* was probably composed, Hobbes was correct about the Society's limit of "about fifty"; see note 3 above.

114 examined by him and by others." "I think," Hobbes said to his interlocutor, "you also do the same."[9] Thus Hobbes disputed the social character of the space the Greshamites said they had created. He said they had a "master" who exercised his authority in the

constitution of their knowledge; they said they were free and equal men, whose matters of fact mirrored the structure of reality.

In denying the Society's stipulations about the nature of its organization and the audience it provided for experimental displays, Hobbes was undermining the justifications which the Greshamites offered for the integrity of experimental findings. These findings, Hobbes claimed, were not witnessed by all; because of the nature of the social space the experimentalists elected to occupy, they were not even available for public witness. Even so, there were immense problems for the very notion of witnessing. Suppose that the experimentalists made their space a truly public one, into which everyone could enter. What would be seen by each man witnessing the experiments? The problems of witnessing experiments, Hobbes suggested, were not different in kind to those involved in evaluating testimony in natural history. It is right, Hobbes agreed, "not to believe in histories blindly." "But are not those phenomena which can be seen daily by each of you suspect, unless all of you see them simultaneously?" Were the experimental displays, in fact, witnessed simultaneously? Were they witnessed together by all members of the experimental collective? If they were not witnessed simultaneously and together, then in what ways was the evaluation of experimental testimony different from the evaluation of testimony generally? Hobbes strongly implied that there was no substantive difference, and, therefore, that the experimental form of life had not discovered a royal road to the making of objective knowledge. Hobbes then briefly treated the *necessity* of the programme dedicated to reiterated series of experimental performances. Why, after all, was it required to produce a great number of these displays instead of just one? Why were the *artificial* phenomena generated by experiment deemed superior to the experience each man has to himself? "[A]re there not enough [experiments], do you not think, shown by the high heavens and the seas and the broad Earth?"[10] Hobbes judged that the experimental programme was otiose. We shall see below that his reasons for this conclusion concerned the validity of inductive inference from effects to causes.

[9] Ibid., pp. 241-242. Interlocutor denied this.
[10] Ibid., p. 241.

But his position on the necessity of repeated artificial productions was clear: "[Experimentalists] fall back on this one thing, that they procure new phenomena, when from the experience of one phenomenon alone the causes are known by reasoning about motion."[11]

Experimental Airs

Hobbes did not rest his criticisms of Boyle and the Greshamites solely on abstract programmatic grounds. The *Dialogus physicus* offered a detailed critical account of how the air-pump worked, or rather how it did not work in the manner claimed for it. The air-pump, Hobbes decided, was not a reliable philosophical instrument. It did not operate in the way that Boyle said it did; the physical integrity of the machine was massively violated, and, therefore, the claim that it produced a vacuum in the receiver (a space devoid of air) was without foundation. In this demonstration it mattered little to Hobbes whether "vacuum" was construed as total or partial (as in Boyle's qualified operational definition). Hobbes attempted to show that all the pump's phenomena were best accounted for by supposing that the receiver was always *full*.

First, Hobbes found it essential to sort out correct ideas about the constitution of the air. According to Hobbes, it was impossible to understand the air-pump experiments "unless the nature of the air is known first."[12] In the *Dialogus* the air's constitution mattered for three reasons: first, because Hobbes's stipulation about the ultimate fluidity of the air ruled out the possibility of an absolutely impermeable seal, and thus the possibility of a secure vacuum; second, because Hobbes's description of the air as a mixture of different fluids enabled him to offer explanations of the phenomena displayed by the pump; finally, because Hobbes claimed that Boyle's unwillingness to offer a certain cause of the spring of the air, and satisfaction with showing that the air had a spring, were marks of Boyle's inadequate conception of natural philosophy. Hobbes assumed a set of basic hypotheses about the structure of the air. Since the air contained a purer and subtler part, no air-pump *could* be impermeable, and, since the air contained a grosser and more earthy part, there was an easily identifiable mechanical

[11] Hobbes, "Mathematicae hodiernae," p. 228. This was published in July 1660, at about the same time that the Society began meeting at Gresham, but it contains material that parallels the later *Dialogus physicus*.

[12] Hobbes, "Dialogus physicus," pp. 243-244.

cause of the spring. From this basis Hobbes proceeded to show why the Greshamites were wrong to claim that their machine produced a vacuum.

In the *Dialogus* both Hobbes and his interlocutor excused themselves for not having a picture of Boyle's air-pump.[13] Nevertheless, the description of the pump and its operation was, on the whole, both highly detailed and accurate. The experimentalist maintained that a vacuum was produced when the sucker was pulled down and the valves appropriately arranged. In Hobbes's view this basic supposition was in error, and, therefore, none of the physical explanations Boyle offered had any validity. Hobbes's demonstration paralleled the more general discussions of the Torricellian experiment and the working of the gardener's pot in *De corpore*, but in this context he was particularly concerned to identify specific and necessary faults in the machine's seals. Put simply, the air-pump leaked. It was no good trying to patch it up, because it was always *bound to* leak. Here is how Hobbes reckoned the air-pump actually worked: when the sucker is pulled down, that much less space is left in the plenum outside, and, by the pulsion of contiguous volumes of air,

> . . . of necessity the air is forced into the place left by the sucker and enters between the convex surface of the sucker and the concave surface of the cylinder. For supposing the parts of the air are infinitely subtle, it is impossible but that they insinuate themselves by this path left by the sucker.

There were several routes by which air might invade the supposedly evacuated machine. The contact between the leather washer (figure 1, "4") and the inner walls of the brass cylinder "cannot be perfect at all points" and there must be space left for the passage of "pure air." Second, since the force with which the sucker is drawn down is very great, this "distends the cavity of the cylinder a little bit" and another path for the passage of air is formed. Finally, "if any hard atoms get in between the edges of the two surfaces, pure air gets in that way." So the retraction of the sucker in an allegedly closed system in fact produces no vacuum at all. Moreover, the "pure air" in the cylinder and receiver is forced, by its manner of entry, to move in a circuit. And, as Hobbes said, "there is nothing . . . which could weaken its [circulatory] motion," since "there can be nothing which can impart motion to itself or diminish it."[14]

[13] Ibid., pp. 235, 242.

[14] Ibid., pp. 245-246.

The imperfection of the pump's seals and the resulting violent passage of the air through these pathways remained a standard component of Hobbes's later tracts against Boyle and experimental practices. In the *Problemata physica* of 1662 Hobbes developed his earlier notions concerning the goodness of the seal between washer and cylinder:

> Truly I think it close enough to keep out straw and feathers, but not to keep out air, nor yet matter. For suppose they were not so exactly close but that there were round about a difference for a small hair to lie between; then will the pulling back of the cylinder of wood [i.e., the sucker] force so much air in, as in retiring it forces back, and that without any sensible difficulty. And the air will so much more swiftly enter as the passage is left more narrow. Or if they touch, and the contact be in some points and not in all, the air will enter as before, in case the force be augmented accordingly. Lastly, though they touch exactly, if either the leather [i.e., the washer] yield, or the brass [i.e., the cylinder], which it may do, to the force of a strong screw, the air will again enter. . . . The effect therefore of their pumping is nothing else but a vehement wind, a very vehement wind.[15]

And in the *Decameron physiologicum* of 1678 Hobbes again allowed "no such exact contiguity [between leather and brass], nor such fastness of the leather: for I never yet had any that in a storm would keep out either air or water." Once air passed through these imperfect seals the violent circulation thus set up could account for "all the alterations that have appeared in the engine."[16] Just as Hobbes showed that Boyle's device was a physically open, not a closed, system, so he showed that its alleged findings were open to reformulation, that they were not necessarily the phenomena they were claimed to be.

Hobbes did not attempt to provide alternative physical accounts of all the phenomena of the *machina Boyleana*: he focused on just a few of Boyle's series, some of which he glossed in several lines and some of which he dealt with in greater detail, evidently regarding them as vitally important. We discuss some of these "crucial" phenomena below. First, however, we must examine the tactics of the *Dialogus* in stipulating proper meanings for the terms "air," "aether," and "vacuum," since these stipulations inform the rest of

15 Hobbes, Seven Philosophical Problems, pp. 20-21.
16 Hobbes, "Decameron physiologicum," pp. 94-95.

Hobbes's arguments against his adversary. The salient features of this analysis are that common air consists of a mixture of earthy and aqueous effluvia with pure air [*aer purus*], this latter pure component sometimes called the "aether"; that such fluids are indefinitely fluid (their fluidity is not due to some minimal nonfluid particle but to the nature of a fluid medium); and, finally, that the term "vacuum" must properly denote places utterly devoid of all matter. Thus, "I suppose the air is a fluid . . . easily divisible into parts which are always still fluid and still air, such that all divisible quantities are there in any quantity. Nor do I suppose as much, but I also believe that we only understand an air purified from all effluvia of earth and water, such as may be considered as aether." Hobbes went on to argue that the Royal Society was wrong in its notion of fluidity: "You make me despair of fruit from your meeting by saying that they think that air, water and other fluids consist of nonfluids. . . . If such is to be said, then there is nothing that is not fluid."[17]

Hobbes laid down the proper use of the word "vacuum." He argued that this word must mean truly empty space, and, therefore, that the pump could not produce a vacuum. His tactic here was analyzed by John Wallis in the *Hobbius heauton-timorumenos* of 1662:

> For Mr. Hobs is very dexterous in confuting others by putting a new sense on their words rehearsed by himself: different from what the words signifie with other Men. And therefore if you [Boyle] shall have occasion to speak of Chalk, He'll tell you that by Chalk he means Cheese: and then if he can prove that what you say of Chalk is not true of Cheese, he reckons himself to have gotten a great victory. And in like manner, when that Heterogeneous Mixture (whatever it be) wherein we breath is commonly known by the name of Air and this Air wherein we live abounds, you say, with parts of such a nature; he tells you that, by air, he understands such an aether as is among the stars, and that in this air there be no such particles.[18]

In the 1640s Hobbes had confronted the same difficulty in his argument with Descartes, who, as we noted in chapter 3, had complained of the identification of his own subtle matter with that of Hobbes.[19] In 1657 Hobbes told Samuel Sorbière that in the context

[17] Hobbes, "Dialogus physicus," pp. 244-245.
[18] Wallis, *Hobbius heauton-timorumenos*, p. 154.
[19] Descartes to Mersenne, 22 February/4 March 1641, in Mersenne, *Correspondance*, vol. x, p. 524.

119 of the experiment of the gardener's pot, the Epicureans "call a vacuum what Descartes calls subtle matter and what I call the purest aethereal substance; of which no part is an Atom, but which can be divided . . . into parts which are always divisible."[20] Now, in 1661, Hobbes repeated his view on what constituted a true void. His interlocutor in the *Dialogus* reported the view of the Royal Society:

> [O]thers, no less authoritative among us, are of the opinion that it would not be very repugnant if by vacuum were understood a place empty of all corporeal substances. For supposing air to be made up of particles which cannot be put together without interstices, they see that it is necessary that these interstices be full of corporeal substance, or (as I will say more openly) of bodies. But they do not believe what the *plenists* understand of such a vacuum, especially recently.[21]

Hobbes responded violently to these views: he denied that any "plenist" limited real ontology to visible substance, and cited Democritus, Epicurus, and Lucretius as authorities (albeit vacuists) for the definition of vacuum as the absence of body, visible and invisible: "None of those whom you call *plenists* understands the vacuum as anything but a place in which there is no corporeal substance at all. If someone speaking negligently were to say, 'in which there is no visible body or air,' then he would be saying that he understands by air all that body which fills all the space left by the Earth and the stars."[22] Having defined "vacuum" this way, Hobbes now set himself the task of showing that some substance, even if invisible, was always present in the receiver of the air-pump.

With this stipulation, Hobbes placed a very firm boundary-condition on the results of the air-pump trials. They *could not* show that a vacuum existed. Very few of Boyle's colleagues ever claimed that the trials had shown such a thing. John Wallis told Boyle in 1662 that "I do not remember that you have therein anywhere declared your Opinion whether there be or be not, a Vacuum, but onely related matter of fact," though Wallis and Boyle had stated that "much of what we call Air is drawn out of the Recipient," which

Wallis claimed "Mr. Hobs doth not Deny."[23] In Boyle's own response to Hobbes, he wrote that "the atmosphere or fluid body, that sur-

[20] Hobbes to Sorbière, 27 January/6 February 1657, in Tönnies, *Studien*, p. 72.

[21] Hobbes, "Dialogus physicus," p. 275.

[22] Ibid., p. 276.

[23] Wallis, *Hobbius heauton-timorumenos*, p. 152; cf. A. R. Hall, *The Scientific Revolution*, p. 212.

rounds the terraqueous globe, may, besides the grosser and more 120
solid corpuscles wherewith it abounds, consist of a thinner matter, which for distinction-sake I also now and then call ethereal."[24] Similarly, Henry Power, one of Boyle's collaborators, said of the Torricellian apparatus that "in the superior part of the tube there is no absolute vacuity," and was careful to distinguish between the views of those like himself who maintained that a subtle fluid was present above the mercury and those disciples of Gassendi, true vacuists, who "will admit of no aether or forrain substances to enter the pores thereof."[25] Finally, as we shall see in chapter 6, the development of the phenomenon of anomalous suspension in the air-pump prompted successive writers to theorize ever more complex mixtures of subtle fluids present in the receiver, and authorities such as Wallis, Huygens and Hooke all wrote of such fluids and their important functions and effects. In September 1672 Wallis explained that what "Wee mean by 'Air' " was a mixture of a purer subtle matter (to be identified with Cartesian "*materia subtilis*" or Hobbesian "*aer purus*") and a grosser group of terrestrial effluvia (identified with Huygens' "air"): "[T]herefore where I speak of 'Vacuity' . . . I do expressly caution . . . not to be understood as affirming absolute Vacuity (which whether or no there be, or can be, in nature, I list not to dispute)."[26] In this context Hobbes's stipulations about the meaning of "air" and "void" were telling: they called for, and obtained, responses from the experimentalists.

How, then, did Hobbes describe the air? In the *Dialogus*, he said that his "hypotheses" about the air's constitution were twofold: "first, that many earthy particles are interspersed in the air, to whose nature simple circular motion is congenital; second, the quantity of these particles is greater in the air near the Earth than in the air further from the Earth."[27] Hobbes mobilized the tripartite typology of visible matter, invisible matter and a fluid space-filling aether which he had outlined in *De corpore*.[28] The invocation of an ultimately fluid body in which grosser substances were mixed was typical of Hobbes's physical armamentarium; he used it, for ex-

[24] Boyle, "Examen of Hobbes," p. 196; we discuss Boyle's aether experiments in chapter 5.

[25] Power, *Experimental Philosophy*, pp. 132-140, 101-103.

[26] Wallis to Oldenburg, 26 September/6 October 1672, in Oldenburg, *Correspondence*, vol. IX, p. 259; cf. Hooke, *Micrographia*, pp. 12-16, 103-105; Huygens to J. Gallois, July 1672, in Huygens, *Oeuvres*, vol. VII, pp. 204-206.

[27] Hobbes, "Dialogus physicus," p. 253.

[28] Hobbes, "Concerning Body," p. 426.

ample, in his miasmal theory of the plague.[29] In the *Problemata* 121
physica of 1662 Hobbes repeated his view on ultimate fluidity and said that "it is that internal motion which distinguisheth all natural bodies one from another."[30] Furthermore, the argument that fluidity and firmness were due to the motion of particles of body and that the air was a mixture of varying gross particles had itself been developed against Cartesian opposition in the 1640s. Where Descartes explained fluidity by the motion of particles, Hobbes explained *firmness* by such motion, as Cavendish told Jungius in 1645 and as Hobbes himself told Mersenne: "Those who wish their matter to be Body and their subtle to be Subtle must necessarily wish

the same as that which is signified by different names."[31] In each of the phenomena Hobbes now examined in the *Dialogus*, he used the contrasting fluidity of pure air and earthy effluvia to explain the observed effects. In so doing, Hobbes showed how it was always possible to generate such explanations from his two hypotheses of fluidity and firmness; he also showed that the invocation of absolute vacuity was both unnecessary and unphilosophical. Once again Hobbes picked out a central problem of the air-pump research programme. Boyle laboured to establish the air's spring as a matter of fact, eschewing any systematic attempt to explain the spring or to prove the vacuum. Hobbes's polemic disproved the vacuum and offered a physical explanation of the apparent spring. Hobbes's interlocutor agreed that "Your hypothesis pleases me more than that of the elastic force of the air. For I see that the truth of the vacuum or of the plenum depends upon the former's truth, whereas from the truth of the latter, nothing follows for either part of the question."[32] It was Boyle's nescience, and his *recommendation* of nescience as an appropriate philosophical stance, that Hobbes found objectionable: hence his effort in the *Dialogus* to show how *easily* his two hypotheses could explain all the phenomena whose cause Boyle said he was unable to find.

In dealing with phenomena which did not obviously involve the air's spring, Hobbes's task was straightforward, given his stipulation of the meaning of the term "vacuum." For example, he agreed with

[29] Hobbes, "Decameron physiologicum," p. 129; cf. p. 136.

[30] Hobbes, "Seven Philosophical Problems," p. 12.

[31] Hobbes to Mersenne, 20/30 March 1641, in Tönnies, *Studien*, p. 115; Sorbière to Mersenne, May 1647, in ibid., pp. 64-65; Hobbes to Mersenne, 28 January/7 February, 1641, in Hobbes, *Latin Works*, vol. v, p. 284; Brockdorff, *Cavendish Bericht für Jungius*, p. 3; Gargani, *Hobbes e la scienza*, p. 217.

[32] Hobbes, "Dialogus physicus," p. 262.

Boyle that animals died in the "evacuated" receiver; they were, however, literally blown to death by a violent circulatory wind, not deprived of vital air. (This was a phenomenon that Hobbes especially commended to the attention of his medical friend, Samuel Sorbière.) Candles went out for the same reason. It was difficult, Hobbes assented, to lift up the cover on the top of the "exhausted" receiver, but this was because of the nature of the plenum and the vehement circulation within the glass.[33] However, in phenomena that were explicitly said to demonstrate the presence of a "spring" in the air, Hobbes took the opportunity to develop a thoroughgoing mechanical account, using his hypothesis of the structure and fluidity of the air and its effluvia. Hobbes used the term *antitupia* for "spring," a word which Henry More had used in 1647 and claimed to have found in Sextus Empiricus.[34] Again and again, the phenomena that Boyle prized as clear examples of the effects of the spring were appropriated as exemplars of the effects of uneven mixtures of earthy and subtle particles moving with a simple circular motion. The cause of the rapid ascent of the sucker when released after an exsuction was not the difference in air pressure but the difference in the number of earthy particles: on pulling back the sucker, pure air could leak into the receiver but earthy particles could not, so a greater proportion of these latter remained outside the sucker and they pressed the sucker up very rapidly.[35] The ascent of water in a hydroscope was explained in the same terms:

> The air, with which the spherical glass was full in the beginning, being moved by those earthy particles in the simple circular motion which we described a little earlier, being compressed

by the force of injection, that of it which is pure leaves by penetrating the injected water for the outer air, leaving a place for the water. So it follows that those earthy particles are left less space in which to exercise their natural motion. Thus impinging on one another, they force the water to leave: and, in leaving, the external air (since the universe is supposed to be full) penetrates it, and successively takes up the place of the

[33] Ibid., pp. 235, 253-254, 257-258, 260, 263-264. In the case of animals' death within the receiver, Hobbes offered a choice of non-Boylean explanations: either the violent wind or some form of suction that interrupted respiration.

[34] Ibid., p. 271; idem, "Decameron physiologicum," p. 108; More, *Philosophicall Poems*, "Interpretation Generall," p. 423.

[35] Hobbes, "Dialogus physicus," p. 253.

air that leaves, until the same quantity of air being replaced, 123
the particles regain the liberty natural to their motion.[36]

Hobbes's account of these phenomena appealed to the space necessary for the earthy particles to "exercise their natural motion": this motion itself produced the rigidity of bodies, as he had indicated in *De corpore*, and this rigidity accounted for resistance to compression and for the force of motion of bodies that contained the earthier air. The consequences of his initial definition of air and vacuum were, therefore, considerable: the subtler part of the air was just that part which *rendered a vacuum impossible*, while the grosser parts were those which performed *the effects Boyle interpreted as "spring."* These two types of matter often combined in their effects. For example, in Boyle's experiment in which a moderately inflated bladder was inserted in the receiver and the glass exhausted, the bladder was observed to inflate and finally to burst. Roberval had developed a similar experiment as evidence for the presence of a *subtle fluid* in the Torricellian space; Boyle used it as evidence for the air's *elasticity*. Hobbes's explanation again adduced the vehement circulation of the *purer part* of the air:

> [E]very skin is made up of small threads, which because of their shapes cannot touch accurately in all points. The bladder, being a skin, must therefore be pervious not only to air but to water, such as sweat. Therefore, there is the same compression of the air compressed inside the bladder by force as there is outside, whose endeavour, its motion following paths that intersect everywhere, tends in every direction towards the concave surface of the bladder. Whence it is necessary that it swells in every direction and, the strength of the endeavour increasing, it is at last torn open.[37]

These emphases on the porosity of the materials in the pump, the ability of the purer air to penetrate all such materials, and the evident power of the simple motion of these particles all characterized the accounts Hobbes offered in the *Dialogus*. As we have seen, these resources were explicitly developed here to contest the vacuum and the spring as Hobbes now defined them.

The experiment to which Hobbes devoted the greatest attention was the thirty-first of Boyle's *New Experiments*, the one in which cohered marbles were placed in the receiver in the (unfulfilled)

[36] Ibid., pp. 274-275.

[37] Ibid., pp. 266-267.

expectation that they would fall apart upon evacuation. Hobbes 124
took up the challenge of explaining this problematic experiment and of reducing its apparent troubles to Hobbesian order. In the *Dialogus*, Hobbes's interlocutor put Boyle's case. He sketched the general form of Boyle's account of cohesion, and claimed that

> . . . if marbles thus cohering were transferred into the receiver and suspended therein, the air being sucked out, were the lower marble to cease sticking to the upper, it would not be possible to doubt that the assigned cause was true. They were moved into the receiver, but without the success expected. For by no further means would they cease to cohere, unless it happened that they were not joined together well enough.

In reply Hobbes suggested that "there was nothing in this [experiment] which should be done by the weight of the atmosphere," and that "No stronger or more evident argument could be devised against those who assert the vacuum than this experiment."[38] Part of the explanation followed Hobbes's treatment of cohesion and of gravity developed in *De corpore*.[39] This was extended and refined in response to Boyle's intervention. Hobbes pointed out that in a plenum the separation of the marbles would either require an instantaneous motion or else moving two bodies simultaneously to the same space, "to say either of which is absurd." He then examined the two possible accounts which Boyle could offer of the phenomenon. One involved the concept of atmospheric weight. Hobbes first defined weight ("fully acknowledged by them, as by everyone else") as "an endeavour along straight lines from all places to the centre of the Earth," which therefore operates in a pyramid whose vertex is in the Earth's centre. The upper marble, Hobbes argued, acted, so to speak, as a "weight shadow" for the lower marble, which received no reflected endeavour from the surface of the Earth: "So nothing arises as a result of atmospheric endeavour sustained by the lower marble to prevent its separation from contact with the upper one."[40]

The other possible explanation that Boyle might offer involved the spring of the air. Hobbes's interlocutor asked: ". . . cannot that elastic force which they say is in the air contribute anything to sustaining the marble?" By no means, Hobbes answered,

[38] Ibid., pp. 267-268.
[39] Hobbes, "Concerning Body," pp. 419, 511-513.
[40] Hobbes, "Dialogus physicus," pp. 268-269.

125

> . . . the endeavour of the air is no greater towards the centre of the Earth than to any other point in the universe. Since all heavy things tend from the edge of the atmosphere to the centre of the Earth, and thence again to the edge of the atmosphere by the same lines of reflection, the endeavour upwards would be equal to the endeavour downwards, and thence by mutually annihilating each other they would endeavour neither way.[41]

Hobbes could easily assert that his plenist account was the correct one because he had already produced the claim that the receiver was full, and he now condemned Boyle's explanations as "dreams": ". . . if I should deny it to be possible by human art to make the surfaces of two hard bodies touch so accurately that not the least creatable particle could be let through, then I do not see how their hypothesis could be rightly sustained, nor how our negation could be rightly argued to be unproven."[42] Hobbes evidently regarded the point as made: he had seized upon an experimentally produced phenomenon that was both central and troublesome to Boyle's programme; he had provided a physical accounting compatible with his own natural philosophy. Moreover, in his confidence, Hobbes had, as it were, "laid a bet" on future trials of this experiment: if Boyle made the experiment into a success (that is, if the marbles separated in the receiver), then, according to his interlocutor, "it would not be possible to doubt" that Boyle's accounting was superior. In the next chapter we shall see how Boyle responded to this challenge.

The Engines of Philosophy

Some historians have dismissed Hobbes's criticisms from consideration on the grounds that he did not perform experiments himself, or at least that he did not repeat those of Boyle's experiments to which he took exception: indeed, as we shall see in the next chapter, this was one of Boyle's own tactics for rejecting Hobbes's views. For this reason we need to give especially careful attention to Hobbes's opinions on the role and value of experimental procedures in natural philosophy. Let us start by confronting claims that Hobbes actually *approved of* experiment and accorded it a cen-

[41] Ibid., p. 269.
[42] Ibid., p. 271.

tral place in properly constituted philosophy. For example, J.W.N. Watkins' excellent book on Hobbes's philosophy attempts to refute the "popular idea that Hobbes despised experiments. . . . He only despised haphazard experimenting."[43] In evidence Watkins cites Hobbes's injunction in the *Decameron physiologicum* that in examining physical hypotheses "you must furnish yourself with as many experiments . . . as you can."[44] Watkins also cites, but does not quote, remarks Hobbes made in reply to one of Wallis's attacks. The passage starts: 126

> Every man that hath spare money, can get furnaces, and buy coals. Every man that hath spare money, can be at the charge of making great moulds, and hiring workmen to grind their glasses; and so may have the best and greatest telescopes. They can get engines made, and apply them to the stars; recipients made, and try conclusions. . . .[45]

(We shall pick up the remainder of this passage shortly.)

In addition, there are intriguing remarks on the subject of experiment in Hobbes's *Six Lessons* of 1656. Here he attempted to exonerate himself from Wallis's charge that he had denigrated the experimental work of Hobbes's friend William Harvey. The story concerned a visit made to Harvey by the Flemish Jesuit Moranus. According to Hobbes, the Jesuit, a man of "but common and childish learning," refused to be instructed by the learned physiologist, but merely vented his own meretricious opinions. In so doing, said Hobbes, "He took occasion, writing against me, to be revenged of Dr. Harvey, by slighting his learning publicly; he tells me that his learning was only experiments; which he says I say have no more certainty than civil histories. Which is false." Hobbes then quoted his remarks on this subject in the "Epistle Dedicatory" of *De corpore*: "Before these [Galileo and Harvey], there was nothing certain in natural philosophy but every man's experiments to himself, and the natural histories, if they may be called certain, that are no certainer than civil histories." Hobbes pointed out that "I except expressly from uncertainty the experiments that every man maketh to himself," and that there was no slur on Harvey.[46]

[43] Watkins, *Hobbes's System*, p. 70n; cf. Laird, *Hobbes*, p. 116: "Hobbes did not despise experimental investigations."
[44] Hobbes, "Decameron physiologicum," p. 88.
[45] Hobbes, "Considerations on the Reputation of Hobbes," p. 436. Watkins does not quote anything from the *Dialogus physicus* in *Hobbes's System*.
[46] Hobbes, "Six Lessons," pp. 338-339. Hobbes gave the Latin version of the

127 Hobbes's sensitivity about the appropriation of Harvey's reputation by the experimentalists of the Royal Society was even more evident in the introduction to the *Dialogus physicus*. Hobbes wanted to show that all sense experience is a consequence of an external motion. His interlocutor reported that he was dazed by the brightness of the sun. Hobbes invited him to sit down "until that excessive motion of the organ of vision settles down." Interlocutor replied:

> You advise well. Truly, I am of the opinion that lassitude of this kind due to solar heat has the habit of increasing mental cloudiness a little. But I do not see enough of the way in which either light or heat produces such effects. Since the time you first demonstrated it to us, I have no longer doubted that not only all feeling but also all change is some motion in the feeling body and in the moving body, and that this motion is generated by some external mover. For previously almost everyone denied it; for whether standing, sitting, or lying down, they nevertheless understood well enough that they were feeling.

Our own feelings appear to be within us, and, therefore, if such appearances were to be the grounds of knowledge, they would lead us to erroneous conclusions. For instance, Hobbes continued, such feelings led people to doubt "whether their own blood moved; for no one feels the motion of their blood unless it pours forth." Interlocutor agreed: "Indeed everyone doubted it before Harvey. Now, however, the same people both confess that Harvey's opinion is true and they are also beginning to accept your beliefs about the motion by which vision is produced. For in our Society there are few who feel otherwise."[47] Hobbes's contention was that it was Harvey, using correct philosophical method, who convinced men of the motion of the blood, not personal experience, and he assimilated the status of his own optical theory to that of Harvey's views of circulation. If, Hobbes argued, Harvey was to be a hero to the Greshamites, then so should Hobbes. Rightly understood, he said, Harvey and Hobbes were methodological allies, both denying the foundational nature of personal experience.

The "experiments that every man maketh to himself" are *experience*. Being, as Hobbes had said, but "sense and memory," they

passage from *De corpore*; we provide the English, from "Concerning Body," pp. viii-ix. For Hobbes's views regarding Galileo, Harvey and the methods of the Padua School, see Watkins, *Hobbes's System*, pp. 55-65.

[47] Hobbes, "Dialogus physicus," pp. 239-240. The same example of the Sun and personal feeling was also used in "Decameron physiologicum," pp. 117-118.

128 generated certainty in him who has the experience; they could not, however, produce the collective certainty which was the prerogative of *philosophy*. Hobbes's views on the role of experimental practices in natural philosophy were, in any case, spelt out clearly elsewhere. In *Decameron physiologicum* Hobbes explicitly devalued the role of formal experimental procedures compared to those experiences of natural phenomena any man can have: "As for mean and common experiments, I think them a great deal better witnesses of nature, than those that are forced by fire, and known but to a very few."[48] And, after enjoining his interlocutor to "furnish yourself with as many experiments (which they call phenomenon) as you can," the agreeable interlocutor assented: "What I want of experiments you may supply out of your own store, or such natural history as you know to be true; though I can be well content with the knowledge of causes of those things which everybody sees commonly produced."[49] Of course, the best evidence of Hobbes's opinion of experimentation in natural philosophy is contained in the *Dialogus physicus*, where it is worked out in the concrete context of his reaction to the Greshamites' programme. However, let us continue

with the passage which starts with Hobbes's account of what "Every man that hath spare money" can do with "furnaces," "telescopes," and "engines." This is how Hobbes concluded:

> They can get engines made, and apply them to the stars; recipients made, and try conclusions; *but they are never the more philosophers for all this.* It is laudable, I confess, to bestow money upon curious or useful delights; but that is none of the praisis of a philosopher. And yet, because the multitude cannot judge, they will pass with the unskilful, for skilful in all parts of natural philosophy . . . So also of all other arts; not every one that brings from beyond seas a new gin, or other jaunty device, *is therefore a philosopher.* For if you reckon that way, not only apothecaries and gardeners, but many other sorts of workmen, will put in for, and get the prize.[50]

And again: "If the sciences were said to be experiments of natural things, then the best of all physicists are quacks [*pharmacopoei*]."[51]

[48] Hobbes, "Decameron physiologicum," p. 117.

[49] Ibid., p. 88; cf. p. 143. Watkins substitutes an ellipsis for the round-bracketed phrase; see *Hobbes's System*, p. 70.

[50] Hobbes, "Considerations on the Reputation of Hobbes," pp. 436-437 (emphases added).

[51] Hobbes, "Mathematicae hodiernae," p. 229. Others have translated *pharmacopoei*

"Ingenuity," Dogmatism, and the Experimental Community

The point to be made is not that Hobbes "despised" experiment, nor that he argued that experiments ought not to be performed, nor even that experiments had no significant place in a properly constituted philosophy of nature. What Hobbes was claiming, however, was that the systematic doing of experiments was not to be equated with philosophy: going on in the way Boyle recommended for experimentalists was not the same thing as philosophical practice. It was not the case that one could ground philosophy in experimentally generated matters of fact. This experimental way and the philosophical way were fundamentally different: they differed in their capacity to secure assent among intellectuals and peace in the polity. The distinction that Hobbes wanted to make involved four considerations that were regarded as intimately related in mid-seventeenth-century schemes: the status of the philosopher's role, his social and moral character, the thought processes involved in doing intellectual work, and the nature of the knowledge that was the outcome of this work. By claiming that adopting an experimental form of life changed proper physicists into "quacks," Hobbes was saying something highly derogatory about the experimentalist's role, character, and practice. Machine-minders were not, in Hobbes's view, to be accounted philosophers. Philosophers should not be identified with mechanical tricksters who produced "various spectacles of an amusing nature."[52]

The modes of thought associated with the philosopher and the mechanic were different. In the *Dialogus physicus* Hobbes insisted upon that contrast: "Ingenuity is one thing and method [*ars*] is another. Here method is needed."[53] The repeated juxtaposition in Hobbes's critiques of method or philosophy, on the one hand, and ingenuity, on the other, is significant. It is plausible that Hobbes was making a substantive point about the experimental mentality by way of etymological punning. The Latin *ingenium* denotes "natural ability, cleverness, inventiveness." In Latin *ingenio* also means a kind of mill, and, from this root, are derived the Old French

as "pharmacists"; in this context "quacks" clearly renders Hobbes's meaning more accurately.

[52] Hobbes, "Dialogus physicus," p. 235. Hobbes specified that it was "a man well-known in breeding and ingenuity" who had produced these trivial spectacles; the juxtaposition was presumably meant to jar.

[53] Ibid., p. 236.

130 *engin* and the Middle English *gin*. Thus Hobbes's identification of ingenuity with, as it were, "engine philosophy" is precisely right for the evaluation he wanted to be placed upon the experimental programme and its products: it relied upon the intellectual processes of artificers and mechanics and, therefore, it yielded an inferior grade of knowledge.[54] That is why Hobbes contrasted "workmen," "apothecaries" and "gardeners" with "philosophers" and why he insisted that not every procurer of "jaunty devices" was a "philosopher." The philosopher was not *banausic*.[55]

Hobbes and Boyle had two things in common in this connection: first, they both gauged the worth of knowledge by taking into consideration the moral constitution and known probity of its producers. This was taken for granted in mid-seventeenth-century calculations, and the problem of assessing testimony made these calculations important, as we have discussed in chapter 2. Second, both Hobbes and Boyle reckoned that the philosopher should be seen as *noble*. Yet their characterizations of the philosopher's role and practice were diametrically opposed. Whose version of the philosopher was, indeed, noble? We have seen that Boyle and his colleagues liked to describe the experimental philosopher as "humble," "modest," an "under-builder," and a "drudge," while specifying that this was a noble character. Boyle and his associates in the Royal Society wanted, for specified purposes, to use the language of the craftsman and to put on the guise of the humble artisan. Hobbes was trying to insinuate that, through their celebration of ingenuity, the Greshamites *really were* making philosophy ignoble. This could have been a seriously damaging imputation in early Restoration society. Hobbes and Boyle agreed that worthy knowledge was produced by worthy men. Yet to Boyle and his friends ingenuity was to be celebrated and the knowledge produced by machines was to be accounted valuable. No stigma was said to be attached to machine minding, no odium to its intellectual products, and no contrast was made between experimental manipulations with machines and philosophy. The Greshamites enjoyed addressing each other as ingenious: the ingenious Mr. Boyle, the ingenious Mr. Wren. But this was an ingenuity made noble by the participation in experimental labour of noble, honest, and trust-

[54] Ibid., p. 278, where the role of mechanic is explicitly contrasted to that of the philosopher; and see Bennett, "Hooke as Mechanic and Natural Philosopher." Sources for the etymology are the *Oxford English Dictionary* and Partridge, *Origins*, under "General."

[55] For the notion of a banausic intellect, see Shapin and Barnes, "Head and Hand."

131 worthy men. That is one reason why, as Robert Greene has perceptively noted, much mid-seventeenth-century usage freely interchanged "ingenuity" and "ingenuousness."[56] Still, despite the public pronouncements, it is not the case that the Fellows of the Royal Society treated mechanics and gardeners *as* philosophers, or that they regarded the testimony of artisans as on a par with that of gentlemen. And it is well to remember that the "ingenious Mr. Boyle" possibly never tended his air-pump himself: the work was done under his supervision by "strong workmen" and skilled instrument-makers.

Now that we understand aspects of Hobbes's condemnation of experimental practice we can parenthetically discuss his relations with the Royal Society as a corporate body. Why was Hobbes not a Fellow? Was he "excluded," and, if so, on what grounds? On the face of it this is not a matter we need to treat in any detail: ours is not a study of the Royal Society itself but of conflicting strategies for generating natural knowledge in mid-seventeenth-century England. Nevertheless, since some recent work has addressed the question of Hobbes's nonmembership in the Royal Society, we ought to point out in what ways our material bears upon that issue. Quentin Skinner has argued against the view that Hobbes was kept out of the Society either because of his religious heterodoxy or because of his opinions on experimentalism and natural philosophy in general. In Skinner's account, within "the broad strategy of mid-seventeenth-century science Hobbes and the Royal Society stand on the same 'side' throughout." He concludes: "The exclusion of Hobbes is then readily explained: no one wants to encourage a club bore."[57] More recently, Hunter has echoed Skinner's judgment, pointing out that Hobbes had friends in the Royal Society, notably Sir John Hoskyns and John Aubrey.[58]

It is, in fact, Aubrey who provides the best apparent support for the claim that there was mutual respect and good will between Hobbes and leading lights in the Royal Society. According to Aubrey, Hobbes "had a high esteeme for the Royall Societie . . . , and

[56] Greene, "Whichcote, Wilkins, 'Ingenuity,' and the Reasonableness of Christianity," esp. pp. 227-229. In the religious context that Greene examines, "ingenuity" often referred to cleverness in exegesis or to the use of reason in theology: "ingenuity" was contrasted with "grace."

[57] Skinner, "Hobbes and the Early Royal Society," pp. 231, 238. Cf. C. Hill, *Some Intellectual Consequences*, pp. 63-64.

[58] Hunter, *Science and Society*, pp. 178-179; idem, "The Debate over Science," pp. 189-190; cf. idem, *The Royal Society and Its Fellows*, p. 6.

the Royall Societie (generally) had the like for him: and he would long since have been ascribed a member there, but for the sake of one or two persons whom he tooke to be his enemies." These "enemies" were, Aubrey noted, "Dr. Wallis (surely their Mercuries are in opposition), and Mr. Boyle. I might add Sir Paul Neile, who disobliges everybody." Furthermore, Aubrey quoted Hobbes's remark in *Behemoth* that " 'Naturall Philosophy was removed from the Universities to Gresham Colledge,' meaning the Royall Societie that meets there." Aubrey pointed out that Hobbes's portrait hung in the Society's meeting-place, and noted that Hobbes fell out with Henry Stubbe "for that he wrote against the lord chancellor Bacon, and the Royall Society."[59]

Aubrey's remarks bear inspection. His *Life of Hobbes*, from which they come, is a partial account, written after Hobbes's death, by a man who was a friend, a Fellow, and a friend of several of Hobbes's bitter enemies.[60] It was an exercise in posthumous reconciliation, and it played down the series of bitter controversies between Hobbes and leading Fellows. Aubrey offered no extended account of the *Dialogus physicus*, in which Hobbes declared all the "Greshamites" to be his "enemies."[61] The remark he quoted from Hobbes's *Behemoth* is taken out of context. It is not praise of the Royal Society, nor, indeed of the professors of Gresham College, but part of another extended indictment of the universities and the clergy for their divisive role in society, and it is not even absolutely clear that "Gresham College" is meant to refer to the Royal Society.[62] Even Aubrey's claim about Hobbes's portrait bears closer examination. The picture in question was painted by J. B. Caspars in 1663, and was commissioned by Aubrey himself, who presented it seven years later to the Royal Society. In notes to the manuscript version of his *Life of Hobbes* Aubrey asked himself whether it would be "improper for me to mention my owne guift?" and ultimately decided not to.[63] Still, there is no reason to doubt that Hobbes did have his friends and admirers among the Fellowship. In addition to Aubrey and Hoskyns, one might include John Evelyn, Sir William Petty, Sir Kenelm Digby, and, of course, his patron William Cavendish, 3rd Earl of Devonshire. Moreover, Hobbes had been the aman-

[59] Aubrey, "Life of Hobbes," pp. 371-372.

[60] On the writing of Aubrey's *Life of Hobbes*, see Hunter, *Aubrey and the Realm of Learning*, pp. 78-80.

[61] Hobbes, "Dialogus physicus," p. 237.

[62] Hobbes, "Behemoth," p. 348.

[63] Aubrey, "Life of Hobbes," p. 354; cf. Powell, *Aubrey and His Friends*, p. 102.

133 uensis of the great Bacon and the friend of William Harvey, two of the Society's heroes.[64]

Of greater interest in this connection is Hobbes's relationship with the new monarch Charles II, the Society's "founder" and patron. After the Restoration Charles continued to receive his former mathematics tutor at Court. While their relationship had something of a "joking" character to it, it appears to have been publicly affectionate: the King liked to refer to the old philosopher as "the beare," and, because, as Aubrey said, the "wittes at Court were wont to bayte him," Charles would greet his approach by crying, " 'Here comes the beare to be bayted!' "[65] The connection was solid enough for the King to grant Hobbes a substantial (albeit irregularly paid) pension, and Hobbes dedicated his *Problemata physica* of 1662 to the King, using the occasion to apologize for any offence that *Leviathan* may have given. The King also possessed a portrait of Hobbes (by Samuel Cooper) which, according to Aubrey, he "conserves as one of his great rarities in his closet at Whitehall."[66] There is also

[64] A list of Hobbes's friends, with comments, is in Aubrey, "Life of Hobbes," pp. 365-371. Aubrey says that Robert Hooke "loved him," but adds that Hooke "was never but once in his company" (p. 371). In the event, there are records that Hooke met Hobbes at least twice, once in July 1663 and once in June 1674 at Aubrey's house; see Hooke to Boyle, 3/13 July 1663, in Boyle, *Works*, vol. VI, pp. 486-487 (cf. Gunther, *Early Science in Oxford*, vol. VI, pp. 139-141, where Hooke's references to Hobbes are unflattering; and, for the 1674 meeting, Hooke, *Diary*, p. 108). Of Hobbes's relationship with Bacon, Aubrey wrote: "The Lord Chancellor Bacon loved to converse with him. . . . His lordship would often say that he better liked Mr. Hobbes's taking his thoughts, then any of the other, because he understood what he wrote": "Life of Hobbes," p. 331. Hobbes's friend Sorbière also remarked upon the relationship with Bacon: Hobbes "is upon the Matter the very Remains of *Bacon*, to whom he was Amanuensis in his Youth": Sorbière, *Voyage to England*, p. 40. Sprat, protecting the Royal Society's hero, took violent exception to the claim that there was any similarity between Hobbes and Bacon, and maintained that Sorbière did not understand Hobbes's philosophy: "Of this I will give an unanswerable Testimony, and that is *the Resemblance that he makes of him to the Lord* Verulam, between whom there is no more likeness than there was between St. *George* and the Waggoner. . . . I scarce know Two Men in the World that have more different Colours of Speech than these Two Great Wits": Sprat, *Observations on Sorbière's Voyage*, p. 163.

[65] Aubrey, "Life of Hobbes," p. 340. Sorbière also reported, in reference to the English clergy and the Oxford mathematicians, that Hobbes "was like a Bear, whom they baited with Dogs to try him": Sorbière, *Voyage to England*, p. 40.

[66] According to Sorbière, the King's pension of £100 per year showed how far Charles was "from laying any stress upon Dr. *Wallis*'s Arguments" that Hobbes's politics were anti-royalist: Sorbière, *Voyage to England*, p. 39. On Hobbes's royal pension and other finances, see Laird, *Hobbes*, pp. 20-21; Hobbes to the King, 1663?, in Hobbes, *English Works*, vol. VII, pp. 471-472; Hobbes to Aubrey, 7/17 September 1663, in Tönnies, *Studien*, p. 108. For Hobbes's remarks to the King concerning

134 the intriguing suggestion that Charles would not have been unhappy to see Hobbes elected to the Society. Sorbière related an interview with the King in which it was "agreed on all Hands, that if Mr. *Hobbs* were not so very Dogmatical, he would be very Useful and Necessary to the Royal-Society; for there are few People that can see farther into things than he, or have applied themselves so long to the Study of Natural Philosophy." Early in the Society's career the King signalled his opinion of Hobbes's worth as a mathematician when he forwarded to the Royal Society one of Hobbes's

geometrical demonstrations.[67] As several historians have suggested, the closeness of the King's association with the great dogmatist must have constituted a considerable threat to the experimentalists of the Royal Society. The King, on whom rested the Society's hopes of material support, was a patron of the new science, but there is little evidence that he discriminated markedly between the rationalist and the experimentalist programmes. Indeed, as Pepys reported, he was known to jest at those very experimental activities which the Society treated as emblematic: "Gresham College he mightily laughed at, for spending time only in weighing of ayre, and doing nothing else since they sat." Nor is it clear what the Royal Society made of the King's sport of placing bets on the outcome of the Society's pneumatic experiments.[68] The stream of fulsome praise directed from the Society to the King was closely connected to its expectations of royal patronage—expectations that were rarely satisfied. Meanwhile, anxious eyes were turned towards "Hobbist" morality at Court. And, as we noted in chapter 3, Hobbes's philosophical standing on the Continent was substantial, just as his political reputation and influence were feared to be in England. "Baiting the bear" was, therefore, an important tactic in policing the boundaries of the new experimental philosophy and in displaying publicly what counted as proper scientific activity and what did not. As de Beer remarks, "It may be said that Hobbes did influence the early policy of the Royal Society, for he set for all time the standard of the sort of man who must not be elected into the Fellowship."[69]

Hobbes continued to engage the Royal Society in controversy through the 1670s. There is no evidence that he assimilated, or responded to, Boyle's post-1660 researches in pneumatics, but the geometrical disputes with John Wallis, begun in the 1650s, flared up repeatedly. In 1671 and 1672 Hobbes attacked Wallis in his *Rosetum geometricum* and *Lux mathematica*, and in a pamphlet addressed *To the Right Honourable and Others, the Members of the Royal Society*.[70] Wallis used the *Philosophical Transactions* to reply, but Hobbes was not allowed access to its pages.[71] Increasingly irritated by this slight, Hobbes wrote to Oldenburg in November 1672 requesting that "if hereafter I shall send you any paper tending to the advancement of Physiques or Mathematiques, and not too long, you will cause it to be printed by him that is Printer to the Society, as you have done often for Dr. Wallis. It will save me some charges."[72] Oldenburg consulted with Wallis about the wisdom of acceding to Hobbes's wish, but the professor, while expressing cool disinterestedness in the matter, judged that Hobbes had nothing to contribute to geometry.[73] Thus armed with advice, Oldenburg wrote to Hobbes in a mollifying tone:

Leviathan, see "Seven Philosophical Problems," pp. 3-6. On Hobbes's portraits, see Sorbière, *Voyage to England*, pp. 39-40; Laird, *Hobbes*, p. 25n; Aubrey, "Life of Hobbes," p. 338. There was also an engraving made in 1664 by William Faithorne (who engraved Boyle's portrait as well) in a series devoted to "distinguished royalists."

67 Sorbière, *Voyage to England*, p. 40; cf. Laird, *Hobbes*, p. 21. For the King's present to the Royal Society, see Birch, *History*, vol. I, p. 42: the gift was made on 4/14 September 1661, just weeks after Hobbes published his *Dialogus physicus*. The only other communication from Hobbes that was placed in the Royal Society's *Letter-Book* was a letter of 10/20 December 1668 concerning a fasting woman, and given to the Society by Daniel Colwall: Birch, *History*, vol. II, pp. 333-334; also in Hobbes, *English Works*, vol. VII, pp. 463-464.

68 Pepys, *Diary*, vol. V, pp. 32-33 (entry for 1/11 February 1664). On 12/22 January 1671 Sir Robert Moray reported to the Royal Society that "the King has laid a wager of fifty pounds to five for the compression of air by water; and that it was acknowledged, that his Majesty had won the wager": Birch, *History*, vol. II, p. 463. The King also referred to the experimental philosophers as "court jesters"; see Middleton, "What did Charles II Call the Fellows of the Royal Society?"

. . . I have no mind to repeat [Wallis's] Answer, being far more inclined, if I were capable, to make you friends, than set you further asunder. Neither is ye R. Society willing to enter into ye decision of the disputes betwixt you, having regard to yr

[69] de Beer, "Some Letters of Hobbes," p. 197; also Bredvold, "Dryden, Hobbes, and the Royal Society," pp. 422-423; Laird, *Hobbes*, pp. 20-21.

[70] The latter appears in *English Works*, vol. VII, pp. 429-448, under the title "Three Papers Presented to the Royal Society against Dr. Wallis."

[71] See, for example, *Philosophical Transactions*, no. 72 (19/29 June 1671), pp. 2185-2186; no. 75 (18/28 September 1671), pp. 2241-2250; no. 87 (14/24 October 1672), pp. 5067-5073.

[72] Hobbes to Oldenburg, 26 November/6 December 1672, in Oldenburg, *Correspondence*, vol. IX, pp. 329-330; also in Hobbes, *English Works*, vol. VII, pp. 465-466, and in Aubrey, "Life of Hobbes," pp. 362-363.

[73] Wallis to Oldenburg, 26 December 1672/5 January 1673, in Oldenburg, *Correspondence*, vol. IX, p. 372.

age, and esteeming yr parts, but doubting you doe mistake in these controversies. However, I am ready to comply with your desires in yt particular, wch concerns ye publication of such papers, you shall send me tending to ye advancement of Physiques and Mathematiques, as are not too long, nor interwoven with personal reflexions; in a word, yt shall be licensed by ye Council of ye R.S.[74]

Apart from another mathematical tract of 1674, Hobbes retired from the fray.[75] He did not reply to Oldenburg, and he never had anything printed by the Royal Society.

The central issue was indeed Hobbes's "dogmatism." But we would miss the point if we separated claims that Hobbes was personally dogmatic from the dogmatism he was seen to recommend in natural philosophical practice. From what we know of his character, there is little doubt that Hobbes could be a difficult person, set in his ways, and not relishing contradiction. In contrast to the "modest" and "humble" experimental philosophers of the Royal Society, Hobbes confidently claimed that he had developed a complete and self-sufficient system of philosophy: they were to come to him to remedy defects in their thought. On the other hand, his friends liked him, and said so. He had a good line in humour, which he delivered in a mild West Country accent. He sang "pricksongs" (badly), got drunk only rarely, played tennis once a week (at the age of 78), gave alms freely, and was regarded as extraordinarily handsome (see figure 5). There was, however, no Mrs. Hobbes, although there was talk of an illegitimate daughter whom he supported. He swore a bit more than was considered strictly proper, but there was no hard evidence of personal libertinism, and he received the sacraments on his deathbed.[76] On these bases it is difficult to recognize in Hobbes the archetypal "club bore." He was, in the event, much more clubbable than one of his major antagonists, John Wallis, F.R.S., who, according to Aubrey, "makes it his Trade to be a comon spye, steals from every ingeniose persons discourse, and prints it. . . . He is a most ill-natured man, an egregious lyar and back-biter, a flatterer and fawner."[77] Boyle himself

[74] Oldenburg to Hobbes, 30 December 1672/9 January 1673, ibid., pp. 374-375.

[75] Hobbes, *Principia et problemata aliquot geometria* . . . (1674).

[76] This is according to Aubrey, "Life of Hobbes," pp. 340, 347-353; see also Sorbière, *Voyage to England*, p. 27.

[77] Aubrey to Hobbes, 24 June/4 July 1675, as quoted in Hunter, *Aubrey and the Realm of Learning*, p. 224; cf. Aubrey, *Brief Lives*, vol. II, pp. 280-283. In fact, Wallis did act as a code breaker for the New Model Army in the 1640s.

was a renowned valetudinarian; he was long-winded but jealous of his privacy: in later years he went so far as to put up visiting hours on his door. At times, his company could be dreary. (For portraits of Boyle, see figure 16.)

When the leading lights of the Royal Society censured Hobbes's dogmatism, they tended to conjoin comments on his personal qualities with judgments upon his philosophical programme. Neither could be tolerated; both personal and programmatic dogmatism were anathema to the practice of experimental philosophy. Sprat's 138
attack on the "Modern Dogmatists," while characteristically not mentioning Hobbes by name, could only have been composed with Hobbes in view. Sprat made his point with a telling analogy between philosophy and politics. Having rejected the dogmatic "tyranny" of the ancients, modern dogmatists directly

> . . . fell to form and impose new Theories on Mens Reason, with an usurpation, as great as that of the others: An action, which we that live in this Age, may resemble to some things that we have seen acted on the Stage of the World: For we also have beheld the Pretenders to publick Liberty, turn the greatest *Tyrants* themselves.

"Methinks," Sprat said, "there is an agreement, between the growth of *Learning*, and of *Civil Government*." Tyranny in each was to be combatted, and there was no reason to prefer the tyranny of any modern philosopher to that of the ancients. No man could rightly claim to have produced a complete and satisfactory system of philosophy. All such dogmatic systems contained the seeds of dissent and, therefore, of their own destruction:

> It is probable, that he, who first discover'd, that all things were order'd in *Nature* by *Motion* [i.e. Hobbes]; went upon a better ground then any before him. But now if he will onely manage this, by nicely disputing about the Nature, and Causes of *Motion* in general; and not prosecute it through all particular Bodies: to what will he at last arrive, but onely to a better sort of *Metaphysicks*? And it may be, his Followers, some Ages hence, will divide his Doctrine into as many distinctions, as the *Schole-men* did that of *Matter*, and *Form*: and so the whole life of it, will also vanish away, into air, and words, as that of theirs has already done.

Sprat discerned a causal connection between philosophical dogmatism and the social relationships this engendered. Dogmatism inclined men to become "imperious," to be unshakable in their convictions, and to be "impatient of contradiction." It produced egotism and individualism, which is a "Temper of mind, of all others the most pernicious." It was pernicious because it disrupted the social relationships which could alone produce and sustain factual natural knowledge. By contrast, the experimental philosophers of the Royal Society were "modest, humble, and friendly"; they were tolerant of differing opinions and worked collectively towards 139
attainable and solid goals.[78]

We now have an answer to the question, "Why was Hobbes excluded from the Royal Society?" It is an answer that does not attempt to distinguish assessments of Hobbes's personality from judgments of his philosophical programme. The connections among personal characteristics, social relationships, and philosophical practices were perceived, as Sprat's polemic shows, to be substantial and vital. The modest and humble Boyle was juxtaposed to the intolerant and confident Hobbes, just as the modest and humble experimental programme was contrasted to Hobbes's overweening rationalism. Each philosophical programme was predicated upon its distinctive social relationships, and each valued a characteristic philosophical persona. The social order implicated in the rationalistic production of knowledge threatened that involved in the Royal Society's experimentalism. Thus our excursion into Hobbes's relations with the Royal Society is not, in fact, peripheral to our major concern with conflicting knowledge-generating strategies. Hobbes's anti-experimentalism, as expressed in the *Dialogus physicus* and elsewhere, gave grounds for his exclusion.[79]

Experiments and Causes

In chapter 2 we discussed Boyle's proposal to erect a procedural boundary between speech of matters of fact (as experimentally produced and manifested) and speech of the physical causes of these facts. In this practice one recognized that God might produce the same effect by a number of different causes, and one professed the appropriately nescient attitude towards the search for real

[78] Sprat, *History*, pp. 28-34. Paul Wood differs from earlier writers on the reliability of Sprat's *History* as an "official" and authoritative account of the Society's activities, while crediting it as sanctioned apologetic: "Methodology and Apologetics." For similar views of dogmatism, see Glanvill, *Vanity of Dogmatizing* (1661); idem, *Scepsis scientifica* (1665).

[79] We therefore find ourselves in some agreement with writers less recent than Skinner and Hunter. However, we do not equate anti-experimentalism with "anti-science," nor need we accept that Hobbes "did not understand" or "did not appreciate" what experimentalism entailed: see Laird, *Hobbes*, p. 24; de Beer, "Some Letters of Hobbes," p. 197; R. F. Jones, *Ancients and Moderns*, p. 128; Bredvold, "Dryden, Hobbes, and the Royal Society," pp. 424-425.

causes.[80] Such real causes *might* ultimately be unveiled by experimental philosophy, but it was wisest and safest to treat causal inquiry with modest caution. Knowledge of causes was at best conjectural and the quest for real causes ought to be carefully segregated from the factual enterprise that laid down the foundations of experimental philosophy. It was this boundary, and the epistemological hierarchy it manifested between knowledge of effects and knowledge of causes, that Hobbes attacked as unphilosophical. In Hobbes's view, in order to be counted as philosophy an intellectual practice could not affect nescience concerning the causes of things. Indeed, philosophy could proceed *from* correct knowledge of causes to knowledge of effects. These programmatic differences between the enterprises proposed by Boyle and Hobbes were concretely expressed in the *Dialogus physicus*.

Hobbes's experimentalist interlocutor had provided an account of the spring of the air and of how this spring might be conceived in terms of wool fleece. Hobbes interrupted: ". . . I ask you, is this not the rule for all hypotheses, that all things which are supposed must be of a possible, that is, conceivable, nature?" The experimentalist concurred and suggested that the elastical hypothesis was supported by the restorative powers "seen in many things" which, therefore, "can very easily be conceived to be in air." Hobbes was not content with this response:

> It is for a philosopher to find the true or at least very probable causes of such things. How could compressed wool or steel plates or atoms of air give your experimental philosophers the cause of restitution? Or do you offer a likely cause why in a crossbow [*ballista*] the steel plate regains its usual straightness so swiftly?

The experimentalist came back with the affectation of causal nescience that Boyle enjoined: "I cannot give a very certain cause for this thing." Hobbes's insistence upon a causal enterprise troubled the experimentalist. Where do causal questions end? "Whatever true cause I told you, you would not then acquiesce to its truth, but would ask me further what was then the cause of this cause, whence it would go on to infinity." This Hobbes flatly denied; the identification of proper causes ends the inquiry: ". . . when you will

[80] Among Boyle's many statements of this type, see "Usefulness of Experimental Natural Philosophy," pp. 45-46; cf. Bechler, "Newton's Optical Controversies," pp. 132-134.

have come to some external cause, there I will leave off asking 141
you."[81] If the profession of causal nescience was acceptable to, even celebrated by, experimentalists, to Hobbes it constituted a damning admission that the experimental programme was not philosophical. Causal inquiry *could* be concluded; it did not breed dissent but could provide the surest remedy for dissent.

Hobbes relentlessly pursued the question of the spring of the air as a causal physical explanation. Either the air's elasticity was offered as a causal account or it was not. If not, then one learned nothing of causes from the experimental programme, and the whole enterprise was truly vacuous. Moreover, even if the air's spring was advanced as a cause, then, as Hobbes endeavoured to show, what resulted was an absurdity. Hobbes argued that this concept, as Boyle used it, was fundamentally anti-mechanical, proceeding from an impossible notion of body. Philosophers "make a legitimate hypothesis from two things: of which the first is, that it be conceivable, that is, not absurd; the other, that by conceding it, the necessity of the phenomenon may be inferred." Yet the hypothesis of the spring of the air *was* absurd, "unless perhaps we concede what is not to be conceded, that something can be moved by itself. For you suppose that the air particle, which certainly stays still when pressed, is moved to its own restitution, assigning no cause for such a motion, except that particle itself."[82]

No argument against Boyle's position could have been, if accepted, more devastating. Boyle advertised his mechanical philosophy as the best way to undermine the "vulgar" and dangerous conception of self-moving matter.[83] Now, Hobbes argued that a true and self-consistent mechanism was obliged to specify the material and mechanical cause of the air's elasticity:

> For if spring were allowed by them [the Greshamites] to be something in the threads of the air, and they were to search for something by which, when somewhat curved yet at rest, the threads would be moved again to straightness: if they wish to be taken for physicists, they would have to assign some possible cause for it.[84]

[81] Hobbes, "Dialogus physicus," pp. 247-249.
[82] Ibid., pp. 254-255.
[83] On this point, see J. Jacob, *Boyle*, chap. 3; idem, "Boyle's Atomism," where the location of vulgar hylozoism among the radical sectaries is discussed; idem, *Stubbe*, chap. 3. Boyle's response to this charge is examined in our next chapter.
[84] Hobbes, "Dialogus physicus," p. 271.

And, if the experimentalists declined to assign such a cause, then 142
how were they different from those Peripatetics and others who invoked "metaphorical terms such as *fuga vacui, horror naturae,* etc.?"[85] As it was, Hobbes was content that the experimentalists should condemn themselves out of their own mouths by admitting their ignorance of causes. He asked: "But what cause . . . do they assign?" Answer: "As yet, none, but they seek for it with this experiment itself."[86] And later: "So you admit there to be nothing yet from your colleagues for the advancement of the science of

natural causes, except that one of them has found a machine" whose operations render the causal hypotheses of Hobbes even "more probable." Experimentalist: "Nor is it shameful to admit it; for it is something to advance so far, if nothing further is allowed." Hobbes attempted to force home the appropriate conclusion:

> Why *so far*? Why such apparatus and the expense of machines of difficult manufacture, just so as you could get as far as Hobbes had already progressed? Why did you not rather begin from where he left off? Why did you not use the principles he established? Since Aristotle had rightly said that *to be ignorant of motion is to be ignorant of nature*, how did you dare to take such a burden upon yourselves, and to arouse in very learned men, not only of our country but also abroad, the expectation of advancing physics, when you have not yet established the doctrine of universal and abstract motion (which was easy and mathematical)?[87]

This is why, Hobbes said, the experimental "philosophy," not being a science grounded in causal knowledge, was no better than the lore acquired by people who were mere mechanics:

> If indeed philosophy were (as it is) the science of causes, in what way did they have more philosophy, who discovered machines useful for experiments, not knowing the causes of the experiments, than this man who, not knowing the causes, designed machines? For there is no difference, except that the one who does not know acknowledges that he does not know, and the others do not so acknowledge.[88]

[85] Ibid., p. 276.
[86] Ibid., p. 261; cf. pp. 277, 287.
[87] Ibid., p. 273; also p. 236.
[88] Ibid., p. 278. Compare Wilkins, *Mathematical Magick* (1648), p. 8, which, twelve years before Boyle's *New Experiments*, identified "philosophy" with "that discipline which discovers the generall causes, effects and properties of things."

143 Much of the *Dialogus physicus* was, in fact, devoted to displaying this equivalence between low artisans and experimental philosophers who were ignorant of causes. Hobbes attacked the celebrated experiment of weighing the air in a bladder suspended on a balance in an exhausted receiver. Boyle could be satisfied that the bladder was indeed depressed on exsuction, but not that it therefore weighed more, since he possessed no knowledge of the efficient cause of gravity. Similarly, Hobbes pointed out that Boyle offered no cause of spring, and compared him to those who ask how many times a bell has rung, "though they have not heard the first stroke."[89] At most, what the experimentalists achieved was the enrichment of "natural history": they "make the phenomena visible."[90] These purposes were not to be despised, but they were not the objectives of the philosopher. Philosophy obliges men to give assent; natural history carries with it no such obligation.

Hobbes's Literary Technology

The scope and character of the assent at which Hobbes's philosophy was aimed is evident in his views on philosophical method, briefly sketched in the preceding chapter. More concretely, it is visible in Hobbes's literary practices. The contrast between the literary forms employed by Boyle and Hobbes is instructive. For example, both used the dialogue form in natural philosophy. However, there is a telling difference between Boyle's usage in the dialogues that constitute *Sceptical Chymist* and Hobbes's literary practices in the natural philosophical dialogues of the *Dialogus physicus, Problemata physica* and the *Decameron physiologicum.* We noted in chapter 2 that the dialogues of *Sceptical Chymist* had the character of a conversation among four participants, so structured that consensus was displayed as emerging through the conversation. Each participant was given something to contribute to the dénouement, which was seen to depend upon the free exchange of factual information. Hobbes's natural philosophical dialogues were in the traditional Socratic mould. There were only two participants: one unambiguously represented Hobbes and the other personated an antagonist (a vacuist, an experimentalist, an inductivist). Truth did not emerge through the exchanges between Hobbes and his interlocutor, for it was

[89] Hobbes, "Dialogus physicus," pp. 261, 271.
[90] Hobbes, "Mathematicae hodiernae," p. 228.

already fully contained in Hobbes's philosophy. Knowledge was portrayed as flowing from Hobbes to interlocutor, who mainly played the role of recipient.

Nevertheless, interlocutor's role was far from negligible. His participation was necessary for the literary exemplification of the conception of knowledge and its social transmission that Hobbes recommended for philosophy.[91] Interlocutor may pose a simple question or express perplexity, to which Hobbes offers satisfying solutions. He may make a statement about physical processes or align himself with certain positions which Hobbes reveals to be fallacious. Interlocutor's statement may be countered with a question from Hobbes, requesting definitions. Interlocutor may then admit that he has no adequate definition for his usage, and Hobbes may then supply the reasons why interlocutor's statement is founded upon absurd speech. Or, interlocutor may give a reply to Hobbes's query, in which Hobbes discerns a flawed logical process: "It is no good argument." Interlocutor can point out possible incompleteness in Hobbes's claims: "These assertions need demonstration," which demonstration Hobbes supplies. Interlocutor then manifests his contentment with Hobbes's proof and gives his assent: "It is very probable"; "It is like enough to be so"; "No doubt"; "It is true." As the dialogue proceeds, interlocutor ceases to represent the adversary and becomes a possible convert: "I am a narrator of other philosophies to you, not a defender." Towards the end of the dialogue, conversion is total: "... I agree with and approve of everything you have said." But one step remains to be taken on this philosophical road to Damascus: having given his assent, interlocutor can now act as "philosopher" himself; he can put Hobbes right and thus display the power of right method to command assent even from a master. Thus, the last line of the *Dialogus physicus*

[91] It would be valuable to have a detailed study of the uses and career of the dialogue form in natural philosophy during the sixteenth and seventeenth centuries. For some interesting remarks, see Multhauf, "Some Nonexistent Chemists"; Beaujot and Mortureux, "Genèse et fonctionnement du discours"; Hannaway, *The Chemists and the Word*; Christie and Golinski, "The Spreading of the Word," esp. pp. 238-246. Literary historians have treated the dialogue form systematically, but have had little to say about its scientific uses: Hirzel, *Der Dialog*, does not mention Boyle's dialogues and dismisses Hobbes's *Dialogus physicus* and *Problemata physica* from consideration (vol. II, p. 399n). Merrill, *The Dialogue in English Literature*, chap. 5, treats "The Philosophical Dialogue" but concentrates on Shaftesbury and Berkeley. For perceptive comments on Galileo's dialogues, see Feyerabend, *Against Method*, chap. 7.

145 belongs to Hobbes, and it is "I judge the same. I have erred: and you have rightly corrected my error."[92]

In these ways the Hobbesian dialogue dramatized the power of philosophical method to secure complete assent. Men may err, but the force of proper method consists in its capacity to put men right, surely and swiftly, when the nature of their error is pointed out to them. Just as philosophical knowledge is produced using the tools of logic, so it is transmitted logically and syllogistically, and this transmission is effective. Thus, in *De corpore* Hobbes described the method by which men *make* knowledge, the "method of invention," and then showed its relationship to the method by which we *demonstrate* to others:

> And seeing teaching is nothing but leading the mind of him we teach, to the knowledge of our inventions, in that track by which we attained the same with our own mind; therefore, the same method that served for our invention, will serve also for demonstration to others. . . . [This method] proceeds by a perpetual composition of propositions into syllogisms, till at last the learner understand the truth of the conclusion sought after.[93]

No man, Hobbes maintained, can continue in error in the face of proper method. The dialogues display the overpowering force of method to compel assent and to correct error. In Hobbes's dialogues, it is method, not matters of fact, that puts men right and that mobilizes consensus. When empirical evidence, whether from observation or from experiment, is given a role in the dialogues, it serves to *illustrate* the conclusions reached by method, and not to determine belief. Thus, in both Boyle's and Hobbes's writings, literary structure and process dramatize the social relations and practices deemed appropriate to the production of knowledge. Differences in theories of knowledge-production and evaluation are displayed in different literary technologies.

Similar considerations inform the character of Hobbes's philo-

[92] In his reply to the dialogues of Hobbes's *Examinatio et emendatio mathematicae hodiernae*, Wallis noted the structural roles of A and B (by which letters Hobbes designated his two participants). They were, Wallis said, "Thomas" and "Hobs," and "when Hobs hath occasion to assume what he cannot prove, Thomas, by a *Manifestum est* saves him the trouble of attempting a demonstration." Wallis, *Hobbius heautontimorumenos*, pp. 15, 103; cf. Laird, *Hobbes*, p. 38. See also the hilariously acute account of the scientific dialogue in Ellis, *So This is Science!*, pp. 45-46.

[93] Hobbes, "Concerning Body," pp. 80-81; cf. p. 87.

146 sophical iconography. The figures supplied to illustrate Hobbes's natural philosophical works were almost entirely geometrical in character, depicting abstract geometrical treatments of physical processes (see, for example, the diagram representing gravitation in our translation of the *Dialogus physicus*). Only very rarely did Hobbes include a pictorial representation of a physical phenomenon or process, and even more rarely did he offer a representation of an experimental system. One example appears in the *Problemata physica*: it is an engraving of the basic Torricellian experiment, but it depicts the apparatus in minimal detail, with no obvious effort at showing the particularities of any specific experimental apparatus.[94] Unlike Boyle, Hobbes never used the engraver's art so as to offer the viewer a virtual sensory experience of an experimental scene. We have already noted that Hobbes endeavoured to describe the air-pump in the *Dialogus physicus* without the aid of a picture. The mind, evidently, was not thought to require the assistance of the eye, much less of the hand. In such ways, Hobbes's philosophical iconography expressed the relative evaluations he placed upon log-

ical and geometrical methods, on the one side, and manipulations of experimental systems, on the other. His iconographic preferences and usages were noticed by his enemies. In 1662 the geometer John Wallis wrote that

> . . . I cannot but observe, in the general, a great Resemblance between this his *Physical Hypothesis* and his *Geometrical Conclusions*. For as in these he draws a Multitude of Lines whereof there is no Use made, as to the Construction or Demonstration of his Problem, . . . so much of his Hypotheses is to no purpose as to the Effects of Nature.[95]

Steven Shapin & Simon Schaffer, *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life*, Princeton University Press, Princeton, NJ, 1985, pp. 110-31, 139-46.

2

DEMARCATION

2.1

Karl R. Popper

SCIENCE: CONJECTURES AND REFUTATIONS

I

WHEN I received the list of participants in this course and realized that I had 33
been asked to speak to philosophical colleagues I thought, after some hesitation and consultation, that you would probably prefer me to speak about those problems which interest me most, and about those developments with which I am most intimately acquainted. I therefore decided to do what I have never done before: to give you a report on my own work in the philosophy of science, since the autumn of 1919 when I first began to grapple with the problem, '*When should a theory be ranked as scientific?*' or '*Is there a criterion for the scientific character or status of a theory?*'

The problem which troubled me at the time was neither, 'When is a theory true?' nor, 'When is a theory acceptable?' My problem was different. I *wished to distinguish between science and pseudo-science;* knowing very well that science often errs, and that pseudo-science may happen to stumble on the truth.

I knew, of course, the most widely accepted answer to my problem: that science is distinguished from pseudo-science—or from 'metaphysics'—by its *empirical method,* which is essentially *inductive,* proceeding from observation or experiment. But this did not satisfy me. On the contrary, I often formulated my problem as one of distinguishing between a genuinely empirical method and a non-empirical or even a pseudo-empirical method—that is to say, a method which, although it appeals to observation and experiment, nevertheless
does not come up to scientific standards. The latter method may be exempli- 34
fied by astrology, with its stupendous mass of empirical evidence based on observation—on horoscopes and on biographies.

But as it was not the example of astrology which led me to my problem I should perhaps briefly describe the atmosphere in which my problem arose and the examples by which it was stimulated. After the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories. Among the theories which interested me Einstein's theory of relativity was no doubt by far the most important. Three others were Marx's theory of history, Freud's psycho-analysis, and Alfred Adler's so-called 'individual psychology'.

There was a lot of popular nonsense talked about these theories, and especially about relativity (as still happens even today), but I was fortunate in those who introduced me to the study of this theory. We all—the small circle of students to which I belonged—were thrilled with the result of Eddington's eclipse observations which in 1919 brought the first important confirmation of Einstein's theory of gravitation. It was a great experience for us, and one which had a lasting influence on my intellectual development.

The three other theories I have mentioned were also widely discussed among students at that time. I myself happened to come into personal contact

A lecture given at Peterhouse, Cambridge, in Summer 1953, as part of a course on developments and trends in contemporary British philosophy, organized by the British Council; originally published under the title 'Philosophy of Science: a Personal Report' in British Philosophy in Mid-Century, *ed. C. A. Mace,* 1957.

with Alfred Adler, and even to co-operate with him in his social work among the children and young people in the working-class districts of Vienna where he had established social guidance clinics.

It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories—the Marxist theory of history, psycho-analysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps first took the simple form, 'What is wrong with Marxism, psycho-analysis, and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?'

To make this contrast clear I should explain that few of us at the time would have said that we believed in the *truth* of Einstein's theory of gravitation. This shows that it was not my doubting the *truth* of those other three theories which bothered me, but something else. Yet neither was it that I merely felt mathematical physics to be more *exact* than the sociological or psychological type of theory. Thus what worried me was neither the problem of truth, at that stage at least, nor the problem of exactness or measurability. It was rather that I felt that these other three theories, though posing as sciences, had in fact more in common with primitive myths than with science; that they resembled astrology rather than astronomy.

I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent *explanatory power*. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an
35 intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still 'un-analysed' and crying aloud for treatment.

The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which 'verified' the theories in question; and this point was constantly emphasized by their adherents. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation—which revealed the class bias of the paper—and especially of course in what the paper did *not* say. The Freudian analysts emphasized that their theories were constantly verified by their 'clinical observations'. As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analysing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. 'Because of my thousandfold experience,' he replied; whereupon I could not help saying: 'And with this new case, I suppose, your experience has become thousand-and-one-fold.'

What I had in mind was that his previous observations may not have been much sounder than this new one; that each in its turn had been interpreted in the light of 'previous experience', and at the same time counted as additional confirmation. What, I asked myself, did it confirm? No more than that a case could be interpreted in the light of the theory. But this meant very little, I reflected, since every conceivable case could be interpreted in the light of Adler's theory, or equally of Freud's. I may illustrate this by two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behaviour which could not be interpreted in terms of either theory. It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness.

With Einstein's theory the situation was strikingly different. Take one typical instance—Einstein's prediction, just then confirmed by the findings of 36
Eddington's expedition. Einstein's gravitational theory had led to the result that light must be attracted by heavy bodies (such as the sun), precisely as material bodies were attracted. As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun, and from one another. This is a thing which cannot normally be observed since such stars are rendered invisible in daytime by the sun's overwhelming brightness; but during an eclipse it is possible to take photographs of them. If the same constellation is photographed at night one can measure the distances on the two photographs, and check the predicted effect.

Now the impressive thing about this case is the *risk* involved in a prediction of this kind. If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation*—in fact with results which everybody before Einstein would have expected.[1] This is quite different from the situation I have previously described, when it turned out that the theories in question were compatible with the most divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories.

These considerations led me in the winter of 1919–20 to conclusions which I may now reformulate as follows.

(1) It is easy to obtain confirmations, or verifications, for nearly every theory—if we look for confirmations.

(2) Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory—an event which would have refuted the theory.

(3) Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.

(4) A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.

(5) Every genuine *test* of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.

[1] This is a slight oversimplification, for about half of the Einstein effect may be derived from the classical theory, provided we assume a ballistic theory of light.

(6) Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of 'corroborating evidence'.)

37 (7) Some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (I later described such a rescuing operation as a '*conventionalist twist*' or a '*conventionalist stratagem*'.)

One can sum up all this by saying that *the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*

II

I may perhaps exemplify this with the help of the various theories so far mentioned. Einstein's theory of gravitation clearly satisfied the criterion of falsifiability. Even if our measuring instruments at the time did not allow us to pronounce on the results of the tests with complete assurance, there was clearly a possibility of refuting the theory.

Astrology did not pass the test. Astrologers were greatly impressed, and misled, by what they believed to be confirming evidence—so much so that they were quite unimpressed by any unfavourable evidence. Moreover, by making their interpretations and prophecies sufficiently vague they were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of their theory. It is a typical soothsayer's trick to predict things so vaguely that the predictions can hardly fail: that they become irrefutable.

The Marxist theory of history, in spite of the serious efforts of some of its founders and followers, ultimately adopted this soothsaying practice. In some of its earlier formulations (for example in Marx's analysis of the character of the 'coming social revolution') their predictions were testable, and in fact falsified.[2] Yet instead of accepting the refutations the followers of Marx re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a 'conventionalist twist' to the theory; and by this stratagem they destroyed its much advertised claim to scientific status.

The two psycho-analytic theories were in a different class. They were simply non-testable, irrefutable. There was no conceivable human behaviour which could contradict them. This does not mean that Freud and Adler were not seeing certain things correctly: I personally do not doubt that much of what they say is of considerable importance, and may well play its part one day in a psychological science which is testable. But it does mean that those 'clinical observations' which analysts naïvely believe confirm their theory cannot do this any more than the daily confirmations which astrologers find
38 in their practice.[3] And as for Freud's epic of the Ego, the Super-ego, and the Id, no substantially stronger claim to scientific status can be made for it than for Homer's collected stories from Olympus. These theories describe some

[2] See, for example, my *Open Society and Its Enemies*, ch. 15, section iii, and notes 13–14.

[3] 'Clinical observations', like all other observations, are *interpretations in the light of theories* (see below, sections iv ff.); and for this reason alone they are apt to seem to support those theories in the light of which they were interpreted. But real support can be obtained only from observations undertaken as tests (by 'attempted refutations'); and for this purpose *criteria of refutation* have to be laid down beforehand: it must be agreed which obser-

facts, but in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form.

At the same time I realized that such myths may be developed, and become testable; that historically speaking all—or very nearly all—scientific theories originate from myths, and that a myth may contain important anticipations of scientific theories. Examples are Empedocles' theory of evolution by trial and error, or Parmenides' myth of the unchanging block universe in which nothing ever happens and which, if we add another dimension, becomes Einstein's block universe (in which, too, nothing ever happens, since everything is, four-dimensionally speaking, determined and laid down from the beginning). I thus felt that if a theory is found to be non-scientific, or 'metaphysical' (as we might say), it is not thereby found to be unimportant, or insignificant, or 'meaningless', or 'nonsensical'.[4] But it cannot claim to be backed by empirical evidence in the scientific sense—although it may easily be, in some genetic sense, the 'result of observation'.

(There were a great many other theories of this pre-scientific or pseudo-scientific character, some of them, unfortunately, as influential as the Marxist interpretation of history; for example, the racialist interpretation of history—another of those impressive and all-explanatory theories which act upon weak minds like revelations.)

Thus the problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences, and all other statements—whether they are of a religious or of a metaphysical character, or simply pseudo-scientific. Years later—it must have been in 1928 or 1929—I called this first problem of mine the '*problem of demarcation*'. The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations.

. . .

vable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular analytic diagnosis but psycho-analysis itself? And have such criteria ever been discussed or agreed upon by analysts? Is there not, on the contrary, a whole family of analytic concepts, such as 'ambivalence' (I do not suggest that there is no such thing as ambivalence), which would make it difficult, if not impossible, to agree upon such criteria? Moreover, how much headway has been made in investigating the question of the extent to which the (conscious or unconscious) expectations and theories held by the analyst influence the 'clinical responses' of the patient? (To say nothing about the conscious attempts to influence the patient by proposing interpretations to him, etc.) Years ago I introduced the term '*Oedipus effect*' to describe the influence of a theory or expectation or prediction *upon the event which it predicts* or describes: it will be remembered that the causal chain leading to Oedipus' parricide was started by the oracle's prediction of this event. This is a characteristic and recurrent theme of such myths, but one which seems to have failed to attract the interest of the analysts, perhaps not accidentally. (The problem of confirmatory dreams suggested by the analyst is discussed by Freud, for example in *Gesammelte Schriften*, III, 1925, where he says on p. 314: 'If anybody asserts that most of the dreams which can be utilized in an analysis . . . owe their origin to [the analyst's] suggestion, then no objection can be made from the point of view of analytic theory. Yet there is nothing in this fact', he surprisingly adds, 'which would detract from the reliability of our results.')

[4] The case of astrology, nowadays a typical pseudo-science, may illustrate this point. It was attacked, by Aristotelians and other rationalists, down to Newton's day, for the wrong reason—for its now accepted assertion that the planets had an 'influence' upon terrestrial ('sublunar') events. In fact Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that 'influenza' epidemics are due to an astral 'influence'. And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology.

46 V

The belief that science proceeds from observation to theory is still so widely and so firmly held that my denial of it is often met with incredulity. I have even been suspected of being insincere—of denying what nobody in his senses can doubt.

But in fact the belief that we can start with pure observations alone, without anything in the nature of a theory, is absurd; as may be illustrated by the story of the man who dedicated his life to natural science, wrote down everything he could observe, and bequeathed his priceless collection of observations to the Royal Society to be used as inductive evidence. This story should show us that though beetles may profitably be collected, observations may not.

Twenty-five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: 'Take pencil and paper; carefully observe, and write down what you have observed!' They asked, of course, *what* I wanted them to observe. Clearly the instruction, 'Observe!' is absurd.[13] (It is not even idiomatic, unless the object of the transitive verb can be taken as understood.) Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem. And its description presupposes a descriptive language, with property words; it presupposes similarity and classification, which in its turn presupposes interests, points of view, and problems. 'A hungry animal',
47 writes Katz,[14] 'divides the environment into edible and inedible things. An animal in flight sees roads to escape and hiding places. . . . Generally speaking, objects change . . . according to the needs of the animal.' We may add that objects can be classified, and can become similar or dissimilar, *only* in this way—by being related to needs and interests. This rule applies not only to animals but also to scientists. For the animal a point of view is provided by its needs, the task of the moment, and its expectations; for the scientist by his theoretical interests, the special problem under investigation, his conjectures and anticipations, and the theories which he accepts as a kind of background: his frame of reference, his 'horizon of expectations'.

The problem 'Which comes first, the hypothesis (H) or the observation (O),' is soluble; as is the problem, 'Which comes first, the hen (H) or the egg (O)'. The reply to the latter is, 'An earlier kind of egg'; to the former, 'An earlier kind of hypothesis'. It is quite true that any particular hypothesis we choose will have been preceded by observations—the observations, for example, which it is designed to explain. But these observations, in their turn, presupposed the adoption of a frame of reference: a frame of expectations: a frame of theories. If they were significant, if they created a need for explanation and thus gave rise to the invention of a hypothesis, it was because they could not be explained within the old theoretical framework, the old horizon of expectations. There is no danger here of an infinite regress. Going back to more and more primitive theories and myths we shall in the end find unconscious, *inborn* expectations.

The theory of inborn *ideas* is absurd, I think; but every organism has inborn *reactions* or *responses*; and among them, responses adapted to impending events. These responses we may describe as 'expectations' without implying that these 'expectations' are conscious. The new-born baby 'expects', in this sense, to be fed (and, one could even argue, to be protected and loved). In view of the close relation between expectation and knowledge we may even speak in quite a reasonable sense of 'inborn knowledge'. This 'knowledge' is not, however, *valid a priori*; an inborn expectation, no matter how strong and specific, may be mistaken. (The newborn child may be abandoned, and starve.)

[13] See section 30 of *L.Sc.D.*

[14] Katz, *loc. cit.*

Thus we are born with expectations; with 'knowledge' which, although not *valid a priori*, is *psychologically or genetically a priori*, i.e. prior to all observational experience. One of the most important of these expectations is the expectation of finding a regularity. It is connected with an inborn propensity to look out for regularities, or with a *need* to *find* regularities, as we may see from the pleasure of the child who satisfies this need.

This 'instinctive' expectation of finding regularities, which is psychologically *a priori*, corresponds very closely to the 'law of causality' which Kant believed to be part of our mental outfit and to be *a priori* valid. One might thus be inclined to say that Kant failed to distinguish between psychologically *a priori* ways of thinking or responding and *a priori* valid beliefs. But I do
not think that his mistake was quite as crude as that. For the expectation of 48
finding regularities is not only psychologically *a priori*, but also logically *a priori*: it is logically prior to all observational experience, for it is prior to any recognition of similarities, as we have seen; and all observation involves the recognition of similarities (or dissimilarities). But in spite of being logically *a priori* in this sense the expectation is not valid *a priori*. For it may fail: we can easily construct an environment (it would be a lethal one) which, compared with our ordinary environment, is so chaotic that we completely fail to find regularities. (All natural laws could remain valid: environments of this kind have been used in the animal experiments mentioned in the next section.)

Thus Kant's reply to Hume came near to being right; for the distinction between an *a priori* valid expectation and one which is both genetically *and* logically prior to observation, but not *a priori* valid, is really somewhat subtle. But Kant proved too much. In trying to show how knowledge is possible, he proposed a theory which had the unavoidable consequence that our quest for knowledge must necessarily succeed, which is clearly mistaken. When Kant said, 'Our intellect does not draw its laws from nature but imposes its laws upon nature', he was right. But in thinking that these laws are necessarily true, or that we necessarily succeed in imposing them upon nature, he was wrong.[15] Nature very often resists quite successfully, forcing us to discard our laws as refuted; but if we live we may try again.

To sum up this logical criticism of Hume's psychology of induction we may consider the idea of building an induction machine. Placed in a simplified 'world' (for example, one of sequences of coloured counters) such a machine may through repetition 'learn', or even 'formulate', laws of succession which hold in its 'world'. If such a machine can be constructed (and I have no doubt that it can) then, it might be argued, my theory must be wrong; for if a machine is capable of performing inductions on the basis of repetition, there can be no logical reasons preventing us from doing the same.

The argument sounds convincing, but it is mistaken. In constructing an induction machine we, the architects of the machine, must decide *a priori* what constitutes its 'world'; what things are to be taken as similar or equal; and what *kind* of 'laws' we wish the machine to be able to 'discover' in its 'world'. In other words we must build into the machine a framework determining what is relevant or interesting in its world: the machine will have its 'inborn' selection principles. The problems of similarity will have been solved for it by its makers who thus have interpreted the 'world' for the machine.

• • •

[15] Kant believed that Newton's dynamics was *a priori* valid. (See his *Metaphysical Foundations of Natural Science*, published between the first and the second editions of the *Critique of Pure Reason*.) But if, as he thought, we can explain the validity of Newton's theory by the fact that our intellect imposes its laws upon nature, it follows, I think, that our intellect *must succeed* in this; which makes it hard to understand why *a priori* knowledge such as Newton's should be so hard to come by. A somewhat fuller statement of this criticism can be found in ch. 2, especially section x, and chs. 7 and 8 of the present volume.

Let us now turn from our logical criticism of the *psychology of experience* to our real problem—the problem of *the logic of science*. Although some of the things I have said may help us here, in so far as they may have eliminated certain psychological prejudices in favour of induction, my treatment of the *logical problem of induction* is completely independent of this criticism, and of all psychological considerations. Provided you do not dogmatically believe in the alleged psychological fact that we make inductions, you may now forget my whole story with the exception of two logical points: my logical remarks on testability or falsifiability as the criterion of demarcation; and Hume's logical criticism of induction.

From what I have said it is obvious that there was a close link between the two problems which interested me at that time: demarcation, and induction or scientific method. It was easy to see that the method of science is criticism, i.e. attempted falsifications. Yet it took me a few years to notice that the two problems—of demarcation and of induction—were in a sense one.

Why, I asked, do so many scientists believe in induction? I found they did so because they believed natural science to be characterized by the inductive method—by a method starting from, and relying upon, long sequences of observations and experiments. They believed that the difference between genuine science and metaphysical or pseudo-scientific speculation depended solely upon whether or not the inductive method was employed. They
53 believed (to put it in my own terminology) that only the inductive method could provide a satisfactory *criterion of demarcation*.

I recently came across an interesting formulation of this belief in a remarkable philosophical book by a great physicist—Max Born's *Natural Philosophy of Cause and Chance*.[18] He writes: 'Induction allows us to generalize a number of observations into a general rule: that night follows day and day follows night . . . But while everyday life has no definite criterion for the validity of an induction, . . . science has worked out a code, or rule of craft, for its application.' Born nowhere reveals the contents of this inductive code (which, as his wording shows, contains a 'definite criterion for the validity of an induction'); but he stresses that 'there is no logical argument' for its acceptance: 'it is a question of faith'; and he is therefore 'willing to call induction a metaphysical principle'. But why does he believe that such a code of valid inductive rules must exist? This becomes clear when he speaks of the 'vast communities of people ignorant of, or rejecting, the rule of science, among them the members of anti-vaccination societies and believers in astrology. It is useless to argue with them; I cannot compel them to accept the same criteria of valid induction in which I believe: the code of scientific rules.' This makes it quite clear that '*valid induction' was here meant to serve as a criterion of demarcation between science and pseudo-science.*

But it is obvious that this rule or craft of 'valid induction' is not even metaphysical: it simply does not exist. No rule can ever guarantee that a generalization inferred from true observations, however often repeated, is true. (Born himself does not believe in the truth of Newtonian physics, in spite of its success, although he believes that it is based on induction.) And the success of science is not based upon rules of induction, but depends upon luck, ingenuity, and the purely deductive rules of critical argument.

I may summarize some of my conclusions as follows:

(1) Induction, i.e. inference based on many observations, is a myth. It is neither a psychological fact, nor a fact of ordinary life, nor one of scientific procedure.

[18] Max Born, *Natural Philosophy of Cause and Chance*, Oxford, 1949, p. 7.

(2) The actual procedure of science is to operate with conjectures: to jump to conclusions—often after one single observation (as noticed for example by Hume and Born).

(3) Repeated observations and experiments function in science as *tests* of our conjectures or hypotheses, i.e. as attempted refutations.

(4) The mistaken belief in induction is fortified by the need for a criterion of demarcation which, it is traditionally but wrongly believed, only the inductive method can provide.

(5) The conception of such an inductive method, like the criterion of verifiability, implies a faulty demarcation.

(6) None of this is altered in the least if we say that induction makes theories only probable rather than certain. (See especially chapter 10, below.)

IX 54

If, as I have suggested, the problem of induction is only an instance or facet of the problem of demarcation, then the solution to the problem of demarcation must provide us with a solution to the problem of induction. This is indeed the case, I believe, although it is perhaps not immediately obvious.

For a brief formulation of the problem of induction we can turn again to Born, who writes: '. . . no observation or experiment, however extended, can give more than a finite number of repetitions'; therefore, 'the statement of a law—B depends on A—always transcends experience. Yet this kind of statement is made everywhere and all the time, and sometimes from scanty material.'[19]

In other words, the logical problem of induction arises from (*a*) Hume's discovery (so well expressed by Born) that it is impossible to justify a law by observation or experiment, since it 'transcends experience'; (*b*) the fact that science proposes and uses laws 'everywhere and all the time'. (Like Hume, Born is struck by the 'scanty material', i.e. the few observed instances upon which the law may be based.) To this we have to add (*c*) *the principle of empiricism* which asserts that in science, only observation and experiment may decide upon the *acceptance or rejection* of scientific statements, including laws and theories.

These three principles, (*a*), (*b*), and (*c*), appear at first sight to clash; and this apparent clash constitutes the *logical problem of induction*.

Faced with this clash, Born gives up (*c*), the principle of empiricism (as Kant and many others, including Bertrand Russell, have done before him), in favour of what he calls a 'metaphysical principle'; a metaphysical principle which he does not even attempt to formulate; which he vaguely describes as a 'code or rule of craft'; and of which I have never seen any formulation which even looked promising and was not clearly untenable.

But in fact the principles (*a*) to (*c*) do not clash. We can see this the moment we realize that the acceptance by science of a law or of a theory is *tentative only*; which is to say that all laws and theories are conjectures, or tentative *hypotheses* (a position which I have sometimes called 'hypotheticism'); and that we may reject a law or theory on the basis of new evidence, without necessarily discarding the old evidence which originally led us to accept it.[20]

The principle of empiricism (*c*) can be fully preserved, since the fate of a theory, its acceptance or rejection, is decided by observation and experiment —by the result of tests. So long as a theory stands up to the severest tests we can design, it is accepted; if it does not, it is rejected. But it is never inferred, in any sense, from the empirical evidence. There is neither a psychological nor

[19] *Natural Philosophy of Cause and Chance*, p. 6.

[20] I do not doubt that Born and many others would agree that theories are accepted only tentatively. But the widespread belief in induction shows that the far-reaching implications of this view are rarely seen.

55 a logical induction. *Only the falsity of the theory can be inferred from empirical evidence, and this inference is a purely deductive one.*

Hume showed that it is not possible to infer a theory from observation statements; but this does not affect the possibility of refuting a theory by observation statements. The full appreciation of this possibility makes the relation between theories and observations perfectly clear.

This solves the problem of the alleged clash between the principles (*a*), (*b*), and (*c*), and with it Hume's problem of induction.

Karl R. Popper, *Conjectures and refutations*, 5th edn, Routledge & Kegan Paul, London, 1974, pp. 33–55 (excerpts).

2.2

Thomas S. Kuhn

LOGIC OF DISCOVERY OR PSYCHOLOGY OF OF RESEARCH?

I

Among the most fundamental issues on which Sir Karl and I agree is our 4
insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practiced. That being so, a few of his recurrent generalizations startle me. One of these provides the opening sentences of the first chapter of the *Logic of Scientific Discovery:* 'A scientist', writes Sir Karl, 'whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.'[1] The statement is virtually a cliché, yet in application it presents three problems. It is ambiguous in its failure to specify which of two sorts of 'statements' or 'theories' are being tested. That ambiguity can, it is true, be eliminated by reference to other passages in Sir Karl's writings, but the generalization that results is historically mistaken. Furthermore, the mistake proves important, for the unambiguous form of the description misses just that characteristic of scientific practice which most nearly distinguishes the sciences from other creative pursuits.

There is one sort of 'statement' or 'hypothesis' that scientists do repeatedly subject to systematic test. I have in mind statements of an individual's best guesses about the proper way to connect his own research problem with the corpus of accepted scientific knowledge. He may, for example, conjecture that a given chemical unknown contains the salt of a rare earth, that the obesity of his experimental rats is due to a specified component in their diet, or that a newly discovered spectral pattern is to be understood as an effect of nuclear spin. In each case, the next steps in his research are intended to try out or test the conjecture or hypothesis. If it passes enough or stringent enough tests, the scientist has made a discovery or has at least resolved the puzzle he had been set. If not, he must either abandon the puzzle entirely or attempt to solve it with the aid of some other hypothesis. Many research problems, though by no means all, take this form. Tests of this sort are a standard component of what I have elsewhere labelled 'normal science' or 'normal research', an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must *premise* current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to

[1] Popper [1959], p. 27.

5 define that puzzle and to guarantee that, given sufficient brilliance, it can be solved.[1] Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested.

This is not, however, the sort of test Sir Karl has in mind. He is above all concerned with the procedures through which science grows, and he is convinced that 'growth' occurs not primarily by accretion but by the revolutionary overthrow of an accepted theory and its replacement by a better one.[2] (The subsumption under 'growth' of 'repeated overthrow' is itself a linguistic oddity whose *raison d'être* may become more visible as we proceed.) Taking this view, the tests which Sir Karl emphasizes are those which were performed to explore the limitations of accepted theory or to subject a current theory to maximum strain. Among his favourite examples, all of them startling and destructive in their outcome, are Lavoisier's experiments on calcination, the eclipse expedition of 1919, and the recent experiments on parity conservation.[3] All, of course, are classic tests, but in using them to characterize scientific activity Sir Karl misses something terribly important about them. Episodes like these are very rare in the development of science. When they occur, they are generally called forth either by a prior crisis in the relevant field (Lavoisier's experiments or Lee and Yang's[4]) or by the existence of a theory which competes with the existing canons of research (Einstein's general relativity). These are, however, aspects of or occasions for what I have elsewhere called 'extraordinary research', an enterprise in which scientists do display
6 very many of the characteristics Sir Karl emphasizes, but one which, at least in the past, has arisen only intermittently and under quite special circumstances in any scientific speciality.[1]

I suggest then that Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz; Sir Karl would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise. Nevertheless, neither science nor

[1] For an extended discussion of normal science, the activity which practitioners are trained to carry on, see my [1962], pp. 23–42, and 135–42. It is important to notice that when I describe the scientist as a puzzle solver and Sir Karl describes him as a problem solver (e.g. in his [1963], pp. 67, 222), the similarity of our terms disguises a fundamental divergence. Sir Karl writes (the italics are his), 'Admittedly, our expectations, and thus our theories, may precede, historically, even our problems. *Yet science starts only with problems.* Problems crop up especially when we are disappointed in our expectations, or when our theories involve us in difficulties, in contradictions'. I use the term 'puzzle' in order to emphasize that the difficulties which *ordinarily* confront even the very best scientists are, like crossword puzzles or chess puzzles, challenges only to his ingenuity. *He* is in difficulty, not current theory. My point is almost the converse of Sir Karl's.

[2] Cf. Popper [1963], pp. 129, 215 and 221, for particularly forceful statements of this position.

[3] For example, Popper [1963], p. 220.

[4] For the work on calcination see, Guerlac [1961]. For the background of the parity experiments see, Hafner and Presswood [1965].

[1] The point is argued at length in my [1962], pp. 52–97.

the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing. Or again, it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal practice depends, that is an oddity which must be explained. Finally, and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores.

In one of his most evocative essays, Sir Karl traces the origin of 'the tradition of critical discussion [which] represents the only practicable way of expanding our knowledge' to the Greek philosophers between Thales and Plato, the men who, as he sees it, encouraged critical discussion both between schools and within individual schools.[2] The accompanying description of Presocratic discourse is most apt, but what is described does not at all resemble science. Rather it is the tradition of claims, counterclaims, and debates over fundamentals which, except perhaps during the Middle Ages, have characterized philosophy and much of social science ever since. Already by the Hellenistic period mathematics, astronomy, statics and the geometric parts of optics had abandoned this mode of discourse in favour of puzzle solving. Other sciences, in increasing numbers, have undergone the same transition since. In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy.[1] Only when they must choose between com- 7
peting theories do scientists behave like philosophers. That, I think, is why Sir Karl's brilliant description of the reasons for the choice between metaphysical systems so closely resembles my description of the reasons for choosing between scientific theories.[2] In neither choice, as I shall shortly try to show, can testing play a quite decisive role.

There is, however, good reason why testing has seemed to do so, and in exploring it Sir Karl's duck may at last become my rabbit. No puzzle-solving enterprise can exist unless its practitioners share criteria which, for that group and for that time, determine when a particular puzzle has been solved. The same criteria necessarily determine failure to achieve a solution, and anyone who chooses may view that failure as the failure of a theory to pass a test. Normally, as I have already insisted, it is not viewed that way. Only the practitioner is blamed, not his tools. But under the special circumstances which induce a crisis in the profession (e.g. gross failure, or repeated failure by the most brilliant professionals) the group's

[2] Popper [1963], chapter 5, especially pp. 148–52.

[1] Though I was not then seeking a demarcation criterion, just these points are argued at length in my [1962], pp. 10–22 and 87–90.

[2] Cf. Popper [1963], pp. 192–200, with my [1962], pp. 143–58. [3] Popper [1963], p. 34.

[4] The index to Popper [1963] has eight entries under the heading 'astrology as a typical pseudo science'.

opinion may change. A failure that had previously been personal may then come to seem the failure of a theory under test. Thereafter, because the test arose from a puzzle and thus carried settled criteria of solution, it proves both more severe and harder to evade than the tests available within a tradition whose normal mode is critical discourse rather than puzzle solving.

In a sense, therefore, severity of test-criteria is simply one side of the coin whose other face is a puzzle-solving tradition. That is why Sir Karl's line of demarcation and my own so frequently coincide. That coincidence is, however, only in their *outcome*; the *process* of applying them is very different, and it isolates distinct aspects of the activity about which the decision—science or non-science—is to be made. Examining the vexing cases, for example, psychoanalysis or Marxist historiography, for which Sir Karl tells us his criterion was initially designed,[3] I concur that they cannot now properly be labelled 'science'. But I reach that conclusion by a route far surer and more direct than his. One brief example may suggest that of the two criteria, testing and puzzle solving, the latter is at once the less equivocal and the more fundamental.

To avoid irrelevant contemporary controversies, I consider astrology rather than, say, psychoanalysis. Astrology is Sir Karl's most frequently cited example of a 'pseudo-science'.[4] He says: 'By making their interpretations and prophecies sufficiently vague they [astrologers] were able to
8 explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of the theory.'[1] Those generalizations catch something of the spirit of the astrological enterprise. But taken at all literally, as they must be if they are to provide a demarcation criterion, they are impossible to support. The history of astrology during the centuries when it was intellectually reputable records many predictions that categorically failed.[2] Not even astrology's most convinced and vehement exponents doubted the recurrence of such failures. Astrology cannot be barred from the sciences because of the form in which its predictions were cast.

Nor can it be barred because of the way its practitioners explained failure. Astrologers pointed out, for example, that, unlike general predictions about, say, an individual's propensities or a natural calamity, the forecast of an individual's future was an immensely complex task, demanding the utmost skill, and extremely sensitive to minor errors in relevant data. The configuration of the stars and eight planets was constantly changing; the astronomical tables used to compute the configuration at an individual's birth were notoriously imperfect; few men knew the instant of their birth with the requisite precision.[3] No wonder, then, that forecasts often failed. Only after astrology itself became implausible did these arguments come to seem question-begging.[4] Similar arguments are regularly used today when explaining, for example, failures in medicine or meteorology. In times of trouble they are also deployed in the exact sciences, fields like physics, chemistry, and astronomy.[5] There was nothing unscientific about the astrologer's explanation of failure.

[1] Popper [1963], p. 37.

[2] For examples see, Thorndike [1923–58], **5**, pp. 225 ff.; **6**, pp. 71, 101, 114.

[3] For reiterated explanations of failure see, *ibid.* **1**, pp. 11 and 514 f.; **4**, 368; **5**, 279.

[4] A perceptive account of some reasons for astrology's loss of plausibility is included in Stahlman [1956]. For an explanation of astrology's previous appeal see, Thorndike [1955].

[5] Cf. my [1962], pp. 66–76.

Nevertheless, astrology was not a science. Instead it was a craft, one of the practical arts, with close resemblances to engineering, meteorology, and medicine as these fields were practised until little more than a century ago. The parallels to an older medicine and to contemporary psychoanalysis are, I think, particularly close. In each of these fields shared theory was adequate only to establish the plausibility of the discipline and to provide a rationale for the various craft-rules which governed practice. These rules had proved their use in the past, but no practitioner supposed they were sufficient to prevent recurrent failure. A more articulated theory and more powerful rules were desired, but it would have been absurd to
abandon a plausible and badly needed discipline with a tradition of limited 9
success simply because these desiderata were not yet at hand. In their absence, however, neither the astrologer nor the doctor could do research. Though they had rules to apply, they had no puzzles to solve and therefore no science to practise.[1]

Compare the situations of the astronomer and the astrologer. If an astronomer's prediction failed and his calculations checked, he could hope to set the situation right. Perhaps the data were at fault: old observations could be re-examined and new measurements made, tasks which posed a host of calculational and instrumental puzzles. Or perhaps theory needed adjustment, either by the manipulation of epicycles, eccentrics, equants, etc., or by more fundamental reforms of astronomical technique. For more than a millennium these were the theoretical and mathematical puzzles around which, together with their instrumental counterparts, the astronomical research tradition was constituted. The astrologer, by contrast, had no such puzzles. The occurrence of failures could be explained, but particular failures did not give rise to research puzzles, for no man, however skilled, could make use of them in a constructive attempt to revise the astrological tradition. There were too many possible sources of difficulty, most of them beyond the astrologer's knowledge, control, or responsibility. Individual failures were correspondingly uninformative, and they did not reflect on the competence of the prognosticator in the eyes of his professional compeers.[2] Though astronomy and astrology were regularly practised by the same people, including Ptolemy, Kepler, and Tycho Brahe, there was never an astrological equivalent of the puzzle-solving astronomical tradition. And without puzzles, able first to challenge and then to attest the ingenuity of the individual practitioner, astrology could

[1] This formulation suggests that Sir Karl's criterion of demarcation might be saved by a minor restatement entirely in keeping with his apparent intent. For a field to be a science its conclusions must be *logically derivable* from *shared premises*. On this view astrology is to be barred not because its forecasts were not testable but because only the most general and least testable ones could be derived from accepted theory. Since any field that did satisfy this condition *might* support a puzzle solving tradition, the suggestion is clearly helpful. It comes close to supplying a sufficient condition for a field's being a science. But in this form, at least, it is not even quite a sufficient condition, and it is surely not a necessary one. It would, for example, admit surveying and navigation as sciences, and it would bar taxonomy, historical geology, and the theory of evolution. The conclusions of a science may be both precise and binding without being fully derivable by logic from accepted premises. Cf. my [1962], pp. 35–51, and also the discussion in Section III, *below*.

[2] This is not to suggest that astrologers did not criticize each other. On the contrary, like practitioners of philosophy and some social sciences, they belonged to a variety of different schools, and the inter-school strife was sometimes bitter. But these debates ordinarily revolved about the *implausibility* of the particular theory employed by one or another school. Failures of individual predictions played very little role. Compare Thorndike [1923–58], 5, p. 233.

not have become a science even if the stars had, in fact, controlled human destiny.

In short, though astrologers made testable predictions and recognized that these predictions sometimes failed, they did not and could not engage in the sorts of activities that normally characterize all recognized sciences. Sir Karl is right to exclude astrology from the sciences, but his over-concentration on science's occasional revolutions prevents his seeing the surest reason for doing so.

That fact, in turn, may explain another oddity of Sir Karl's historiography. Though he repeatedly underlines the role of tests in the replacement of scientific theories, he is also constrained to recognize that many theories, for example the Ptolemaic, were replaced before they had in fact been tested.[1] On some occasions, at least, tests are not requisite to the revolutions through which science advances. But that is not true of puzzles. Though the theories Sir Karl cites had not been put to the test before their displacement, none of these was replaced before it had ceased adequately to support a puzzle-solving tradition. The state of astronomy was a scandal in the early sixteenth century. Most astronomers nevertheless felt that normal adjustments of a basically Ptolemaic model would set the situation right. In this sense the theory had not failed a test. But a few astronomers, Copernicus among them, felt that the difficulties must lie in the Ptolemaic approach itself rather than in the particular versions of Ptolemaic theory so far developed, and the results of that conviction are already recorded. The situation is typical.[2] With or without tests, a puzzle-solving tradition can prepare the way for its own displacement. To rely on testing as the mark of a science is to miss what scientists mostly do and, with it, the most characteristic feature of their enterprise.

[1] Cf. Popper [1963], p. 246.

[2] Cf. my [1962], pp. 77–87.

Imre Lakatos and Alan Musgrave, *Criticism and the growth of knowledge*,
Cambridge University Press, London and New York, 1970, pp. 4–10.

Imre Lakatos

2.3

SCIENCE AND PSEUDO-SCIENCE

Man's respect for knowledge is one of his most peculiar characteristics. 114
Knowledge in Latin is *scientia*, and science came to be the name of the most respectable kind of knowledge. But what distinguishes knowledge from superstition, ideology or pseudoscience? The Catholic Church excommunicated Copernicans, the Communist Party persecuted Mendelians on the ground that their doctrines were pseudoscientific. The demarcation between science and pseudoscience is not merely a
problem of armchair philosophy: it is of vital social and political 115
relevance.

Many philosophers have tried to solve the problem of demarcation in the following terms: a statement constitutes knowledge if sufficiently many people believe it sufficiently strongly. But the history of thought shows us that many people were totally committed to absurd beliefs. If the strength of beliefs were a hallmark of knowledge, we should have to rank some tales about demons, angels, devils, and of heaven and hell as knowledge. Scientists, on the other hand, are very sceptical even of their best theories. Newton's is the most powerful theory science has yet produced, but Newton himself never believed that bodies attract each other at a distance. So no degree of commitment to beliefs makes them knowledge. Indeed, the hallmark of scientific behaviour is a certain scepticism even towards one's most cherished theories. Blind commitment to a theory is not an intellectual virtue: it is an intellectual crime.

Thus a statement may be pseudoscientific even if it is eminently 'plausible' and everybody believes in it, and it may be scientifically valuable even if it is unbelievable and nobody believes in it. A theory may even be of supreme scientific value even if no one understands it, let alone believes it.

The cognitive value of a theory has nothing to do with its psychological influence on people's minds. Belief, commitment, understanding are states of the human mind. But the objective, scientific value of a theory is independent of the human mind which creates it or understands it. Its scientific value depends only on what objective support these conjectures have in facts. As Hume said:

> If we take in our hand any volume; of divinity, or school metaphysics, for instance; let us ask, does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames. For it can contain nothing but sophistry and illusion.

But what is 'experimental' reasoning? If we look at the vast seventeenth-century literature on witchcraft, it is full of reports of careful observations and sworn evidence – even of experiments. Glanvill, the house philosopher of the early Royal Society, regarded witchcraft as the paradigm of experimental reasoning. We have to define experimental reasoning before we start Humean book burning.

In scientific reasoning, theories are confronted with facts; and one of
116 the central conditions of scientific reasoning is that theories must be supported by facts. Now how exactly can facts support theory?

Several different answers have been proposed. Newton himself thought that he proved his laws from facts. He was proud of not uttering mere hypotheses: he only published theories proven from facts. In particular, he claimed that he deduced his laws from the 'phenomena' provided by Kepler. But his boast was nonsense, since according to Kepler, planets move in ellipses, but according to Newton's theory, planets would move in ellipses only if the planets did not disturb each other in their motion. But they do. This is why Newton had to devise a perturbation theory from which it follows that no planet moves in an ellipse.

One can today easily demonstrate that there can be no valid derivation of a law of nature from any finite number of facts; but we still keep reading about scientific theories being proved from facts. Why this stubborn resistance to elementary logic?

There is a very plausible explanation. Scientists want to make their theories respectable, deserving of the title 'science', that is, genuine knowledge. Now the most relevant knowledge in the seventeenth century, when science was born, concerned God, the Devil, Heaven and Hell. If one got one's conjectures about matters of divinity wrong, the consequence of one's mistake was eternal damnation. Theological knowledge cannot be fallible: it must be beyond doubt. Now the Enlightenment thought that we were fallible and ignorant about matters theological. There is no scientific theology and, therefore, no theological knowledge. Knowledge can only be about Nature, but this new type of knowledge had to be judged by the standards they took over straight from theology: it had to be proven beyond doubt. Science had to achieve the very certainty which had escaped theology. A scientist, worthy of the name, was not allowed to guess: he had to prove each sentence he uttered from facts. This was the criterion of scientific honesty. Theories unproven from facts were regarded as sinful pseudoscience, heresy in the scientific community.

It was only the downfall of Newtonian theory in this century which made scientists realize that their standards of honesty had been utopian. Before Einstein most scientists thought that Newton had deciphered God's ultimate laws by proving them from the facts. Ampère, in the early nineteenth century, felt he had to call his book on his speculations concerning electromagnetism: *Mathematical Theory of Electrodynamic Phenomena Unequivocally Deduced from Experiment*. But at the end of the volume he casually confesses that some of the experiments were never performed and even that the necessary instruments had not been constructed!

117 If all scientific theories are equally unprovable, what distinguishes scientific knowledge from ignorance, science from pseudoscience?

One answer to this question was provided in the twentieth century by 'inductive logicians'. Inductive logic set out to define the probabilities of different theories according to the available total evidence. If the mathematical probability of a theory is high, it qualifies as scientific; if it is low or even zero, it is not scientific. Thus the hallmark of scientific honesty would be never to say anything that is not at least highly probable. Probabilism has an attractive feature: instead of simply providing a black-and-white distinction between science and pseudoscience, it provides a continuous scale from poor theories with low probability to good theories with high probability. But, in 1934, Karl Popper, one of the most influential philosophers of our time, argued that the mathematical probability of all theories, scientific or pseudoscientific, given *any* amount of evidence is zero. If Popper is right, scientific theories are not only equally unprovable but also equally improbable. A new demarcation criterion was needed and Popper proposed a rather stunning one. A theory may be scientific even if there is not a shred of evidence in its favour, and it may be pseudoscientific even if all the available evidence is in its favour. That is, the scientific or non-scientific character of a theory can be determined independently of the facts. A theory is 'scientific' if one is prepared to specify in advance a crucial experiment (or observation) which can falsify it, and it is pseudoscientific if one refuses to specify such a 'potential falsifier'. But if so, we do not demarcate scientific theories from pseudoscientific ones, but rather scientific method from non-scientific method. Marxism, for a Popperian, is scientific if the Marxists are prepared to specify facts which, if observed, make them give up Marxism. If they refuse to do so, Marxism becomes a pseudoscience. It is always interesting to ask a Marxist, what conceivable event would make him abandon his Marxism. If he is committed to Marxism, he is bound to find it immoral to specify a state of affairs which can falsify it. Thus a proposition may petrify into pseudoscientific dogma or become genuine knowledge, depending on whether we are prepared to state observable conditions which would refute it.

Is, then, Popper's falsifiability criterion the solution to the problem of demarcating science from pseudoscience? No. For Popper's criterion ignores the remarkable tenacity of scientific theories. Scientists have thick skins. They do not abandon a theory merely because facts contradict it. They normally either invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain the anomaly, they ignore it, and direct their attention to other problems. Note that scientists talk about anomalies, recalcitrant 118
instances, not refutations. History of science, of course, is full of accounts of how crucial experiments allegedly killed theories. But such accounts are fabricated long after the theory had been abandoned. Had Popper ever asked a Newtonian scientist under what experimental conditions he would abandon Newtonian theory, some Newtonian scientists would have been exactly as nonplussed as are some Marxists.

What, then, is the hallmark of science? Do we have to capitulate and agree that a scientific revolution is just an irrational change in commitment, that it is a religious conversion? Tom Kuhn, a distinguished American philosopher of science, arrived at this conclusion after discovering the naïvety of Popper's falsificationism. But if Kuhn is right, then there is no explicit demarcation between science and pseudoscience, no distinction between scientific progress and intellectual decay, there is no objective standard of honesty. But what criteria can he then offer to demarcate scientific progress from intellectual degeneration?

In the last few years I have been advocating a methodology of scientific research programmes, which solves some of the problems which both Popper and Kuhn failed to solve.

First, I claim that the typical descriptive unit of great scientific achievements is not an isolated hypothesis but rather a research programme. Science is not simply trial and error, a series of conjectures and refutations. 'All swans are white' may be falsified by the discovery of one black swan. But such trivial trial and error does not rank as science.

Newtonian science, for instance, is not simply a set of four conjectures – the three laws of mechanics and the law of gravitation. These four laws constitute only the 'hard core' of the Newtonian programme. But this hard core is tenaciously protected from refutation by a vast 'protective belt' of auxiliary hypotheses. And, even more importantly, the research programme has also a 'heuristic', that is, a powerful problem-solving machinery, which, with the help of sophisticated mathematical techniques, digests anomalies and even turns them into positive evidence. For instance, if a planet does not move exactly as it should, the Newtonian scientist checks his conjectures concerning atmospheric refraction, concerning propagation of light in magnetic storms, and hundreds of other conjectures which are all part of the programme. He may even invent a hitherto unknown planet and calculate its position, mass and velocity in order to explain the anomaly.

Now, Newton's theory of gravitation, Einstein's relativity theory, quantum mechanics, Marxism, Freudism, are all research programmes, each with a characteristic hard core stubbornly defended,
119 each with its more flexible protective belt and each with its elaborate problem-solving machinery. Each of them, at any stage of its development, has unsolved problems and undigested anomalies. All theories, in this sense, are born refuted and die refuted. But are they equally good? Until now I have been describing what research programmes are like. But how can one distinguish a scientific or progressive programme from a pseudoscientific or degenerating one?

Contrary to Popper, the difference cannot be that some are still unrefuted, while others are already refuted. When Newton published his *Principia*, it was common knowledge that it could not properly explain even the motion of the moon; in fact, lunar motion refuted Newton. Kaufmann, a distinguished physicist, refuted Einstein's relativity theory in the very year it was published. But all the research programmes I admire have one characteristic in common. They all predict novel facts, facts which had been either undreamt of, or have indeed been contradicted by previous or rival programmes. In 1686, when Newton published his theory of gravitation, there were, for instance, two current theories concerning comets. The more popular one regarded comets as a signal from an angry God warning that He will strike and bring disaster. A little known theory of Kepler's held that comets were celestial bodies moving along straight lines. Now according to Newtonian theory, some of them moved in hyperbolas or parabolas never to return; others moved in ordinary ellipses. Halley, working in Newton's programme, calculated on the basis of observing a brief stretch of a comet's path that it would return in seventy-two years' time; he calculated to the minute when it would be seen again at a well-defined point of the sky. This was incredible. But seventy-two years later, when both Newton and Halley were long dead, Halley's comet returned exactly as Halley predicted. Similarly, Newtonian scientists predicted the existence and exact motion of small planets which had never been observed before. Or let us take Einstein's programme. This programme made the stunning prediction that if one measures the distance between two stars in the night and if one measures the distance between them during the day (when they are visible during an eclipse of the sun), the two measurements will be different. Nobody had thought to make such an observation before Einstein's programme. Thus in a progressive research programme theory leads to the discovery of hitherto unknown novel facts. In degenerating programmes, however,

theories are fabricated only in order to accommodate known facts. Has, for instance, Marxism ever predicted a stunning novel fact successfully? Never! It has some famous unsuccessful predictions. It predicted the absolute impoverishment of the working class. It predicted that the first 120
socialist revolution would take place in the industrially most developed society. It predicted that socialist societies would be free of revolutions. It predicted that there will be no conflict of interests between socialist countries. Thus the early predictions of Marxism were bold and stunning but they failed. Marxists explained all their failures: they explained the rising living standards of the working class by devising a theory of imperialism; they even explained why the first socialist revolution occurred in industrially backward Russia. They 'explained' Berlin 1953, Budapest 1956, Prague 1968. They 'explained' the Russian–Chinese conflict. But their auxiliary hypotheses were all cooked up after the event to protect Marxian theory from the facts. The Newtonian programme led to novel facts; the Marxian lagged behind the facts and has been running fast to catch up with them.

To sum up. The hallmark of empirical progress is not trivial verifications: Popper is right that there are millions of them. It is no success for Newtonian theory that stones, when dropped, fall towards the earth, no matter how often this is repeated. But so-called 'refutations' are not the hallmark of empirical failure, as Popper has preached, since all programmes grow in a permanent ocean of anomalies. What really count are dramatic, unexpected, stunning predictions: a few of them are enough to tilt the balance; where theory lags behind the facts, we are dealing with miserable degenerating research programmes.

Now, how do scientific revolutions come about? If we have two rival research programmes, and one is progressing while the other is degenerating, scientists tend to join the progressive programme. This is the rationale of scientific revolutions. But while it is a matter of intellectual honesty to keep the record public, it is not dishonest to stick to a degenerating programme and try to turn it into a progressive one.

As opposed to Popper the methodology of scientific research programmes does not offer instant rationality. One must treat budding programmes leniently: programmes may take decades before they get off the ground and become empirically progressive. Criticism is not a Popperian quick kill, by refutation. Important criticism is always constructive: there is no refutation without a better theory. Kuhn is wrong in thinking that scientific revolutions are sudden, irrational changes in vision. The history of science refutes both Popper and Kuhn: on close inspection both Popperian crucial experiments and Kuhnian revolutions turn out to be myths: what normally happens is that progressive research programmes replace degenerating ones.

The problem of demarcation between science and pseudoscience has grave implications also for the institutionalization of criticism. Copernicus's theory was banned by the Catholic Church in 1616 because it was 121
said to be pseudoscientific. It was taken off the index in 1820 because by that time the Church deemed that facts had proved it and therefore it became scientific. The Central Committee of the Soviet Communist Party in 1949 declared Mendelian genetics pseudoscientific and had its advocates, like Academician Vavilov, killed in concentration camps; after Vavilov's murder Mendelian genetics was rehabilitated; but the Party's right to decide what is science and publishable and what is pseudoscience and punishable was upheld. The new liberal Establishment of the West also exercises the right to deny freedom of speech to

what it regards as pseudoscience, as we have seen in the case of the debate concerning race and intelligence. All these judgements were inevitably based on some sort of demarcation criterion. This is why the problem of demarcation between science and pseudoscience is not a pseudo-problem of armchair philosophers: it has grave ethical and political implications.

Stuart Brown, John Fauvel and Ruth Finnegan (eds), *Conceptions of inquiry*, Methuen/Open University Press, London, 1981, pp. 114–121.

Paul Feyerabend

HOW TO DEFEND SOCIETY AGAINST SCIENCE

Practitioners of a strange trade, friends, enemies, ladies and gentlemen: 55
Before starting with my talk, let me explain to you, how it came into existence.

About a year ago I was short of funds. So I accepted an invitation to contribute to a book dealing with the relation between science and religion. To make the book sell I thought I should make my contribution a provocative one and the most provocative statement one can make about the relation between science and religion is that science is a religion. Having made the statement the core of my article I discovered that lots of reasons, lots of excellent reasons, could be found for it. I enumerated the reasons, finished my article, and got paid. That was stage one.

Next I was invited to a Conference for the Defence of Culture. I accepted the invitation because it paid for my flight to Europe. I also must admit that I was rather curious. When I arrived in Nice I had no idea what I would say. Then while the conference was taking its course I discovered that everyone thought very highly of science and that everyone was very serious. So I decided to explain how one could defend culture from science. All the reasons collected in my article would apply here as well and there was no need to invent new things. I gave my talk, was rewarded with an outcry about my "dangerous and ill considered ideas," collected by ticket and went on to Vienna. That was stage number two.

Now I am supposed to address you. I have a hunch that in some respect you are very different from my audience in Nice. For one, you look much younger. My audience in Nice was full of professors, businessmen, and television executives, and the average age was about 58½. Then I am quite sure that 56
most of you are considerably to the left of some of the people in Nice. As a matter of fact, speaking somewhat superficially I might say that you are a leftist audience while my audience in Nice was a rightist audience. Yet despite all these differences you have some things in common. Both of you, I assume, respect science and knowledge. Science, of course, must be reformed and must be made less authoritarian. But once the reforms are carried out, it is a valuable source of knowledge that must not be contaminated by ideologies of a different kind. Secondly, both of you are serious people. Knowledge is a serious matter, for the Right as well as for the Left, and it must be pursued in a serious spirit. Frivolity is out, dedication and earnest application to the task at hand is in. These similarities are all I need for repeating my Nice talk to you with hardly any change. So, here it is.

Fairytales

I want to defend society and its inhabitants from all ideologies, science included. All ideologies must be seen in perspective. One must not take them too seriously. One must read them like fairytales which have lots of interesting things to say

---

[This] article is a revised version of a talk given to the Philosophy Society at Sussex University in November 1974.

but which also contain wicked lies, or like ethical prescriptions which may be useful rules of thumb but which are deadly when followed to the letter.

Now, is this not a strange and ridiculous attitude? Science, surely, was always in the forefront of the fight against authoritarianism and superstition. It is to science that we owe our increased intellectual freedom vis-à-vis religious beliefs; it is to science that we owe the liberation of mankind from ancient and rigid forms of thought. Today these forms of thought are nothing but bad dreams—and this we learned from science. Science and enlightenment are one and the same thing—even the most radical critics of society believe this. Kropotkin wants to overthrow all traditional institutions and forms of belief, with the exception of science. Ibsen criticises the most intimate ramifications of nineteenth-century bourgeois ideology, but he leaves science untouched. Levi-Strauss has made us realise that Western Thought is not the lonely peak of human achievement it was once believed to be, but he excludes science from his relativization of ideologies. Marx and Engels were convinced that science would aid the workers in their quest for mental and social liberation. Are all these people deceived? Are they all mistaken about the role of science? Are they all the victims of a chimaera?

To these questions my answer is a firm *Yes and No.*

Now, let me explain my answer.

My explanation consists of two parts, one more general, one more specific.

The general explanation is simple. Any ideology that breaks the hold a comprehensive system of thought has on the minds of men contributes to the liberation of man. Any ideology that makes man question inherited beliefs is an aid to enlightenment. A truth that reigns without checks and balances is a
57 tyrant who must be overthrown, and any falsehood that can aid us in the overthrow of this tyrant is to be welcomed. It follows that seventeenth- and eighteenth-century science indeed *was* an instrument of liberation and enlightenment. It does not follow that science is bound to *remain* such an instrument. There is nothing inherent in science or in any other ideology that makes it *essentially* liberating. Ideologies can deteriorate and become stupid religions. Look at Marxism. And that the science of today is very different from the science of 1650 is evident at the most superficial glance.

For example, consider the role science now plays in education. Scientific "facts" are taught at a very early age and in the very same manner in which religious "facts" were taught only a century ago. There is no attempt to waken the critical abilities of the pupil so that he may be able to see things in perspective. At the universities the situation is even worse, for indoctrination is here carried out in a much more systematic manner. Criticism is not entirely absent. Society, for example, and its institutions, are criticised most severely and often most unfairly and this already at the elementary school level. But science is excepted from the criticism. In society at large the judgement of the scientist is received with the same reverence as the judgement of bishops and cardinals was accepted not too long ago. The move towards "demythologization," for example, is largely motivated by the wish to avoid any clash between Christianity and scientific ideas. If such a clash occurs, then science is certainly right and Christianity wrong. Pursue this investigation further and you will see that science has now become as oppressive as the ideologies it had once to fight. Do not be misled by the fact that today hardly anyone gets killed for joining a scientific heresy. This has nothing to do with science. It has something to do with the general quality of our civilization. Heretics in science are still made to suffer from the *most severe* sanctions this relatively tolerant civilization has to offer.

But—is this description not utterly unfair? Have I not presented the matter in a very distorted light by using tendentious and distorting terminology? Must we not describe the situation in a very different way? I have said that science has become *rigid,* that it has ceased to be an instrument of *change* and *liberation,* without adding that it has found the *truth,* or a large part thereof. Considering this additional fact we realise, so the objection goes, that the rigidity of science is not due to human wilfulness. It lies in the nature of things. For once we have discovered the truth—what else can we do but follow it?

This trite reply is anything but original. It is used whenever an ideology wants to reinforce the faith of its followers. "Truth" is such a nicely neutral word. Nobody would deny that it is commendable to speak the truth and wicked to tell lies. Nobody would deny that—and yet nobody knows what such an attitude amounts to. So it is easy to twist matters and to change allegiance to truth in one's everyday affairs into allegiance to the Truth of an ideology which is nothing but the dogmatic defence of that ideology. And it is of course *not* true that we *have* to follow the truth. Human life is guided by many ideas. Truth is one of them. Freedom and mental independence are others. If Truth, as conceived by some ideologists, conflicts with freedom, then we have a *choice.* We may abandon freedom. But we may also abandon Truth. (Alterna- 58
tively, we may adopt a more sophisticated idea of truth that no longer contradicts freedom; that was Hegel's solution.) My criticism of modern science is that it inhibits freedom of thought. If the reason is that it has found the truth and now follows it, then I would say that there are better things than first finding, and then following such a monster.

This finishes the general part of my explanation.

There exists a more specific argument to defend the exceptional position science has in society today. Put in a nutshell the argument says (1) that science has finally found the correct *method* for achieving results and (2) that there are many *results* to prove the excellence of the method. The argument is mistaken—but most attempts to show this lead into a dead end. Methodology has by now become so crowded with empty sophistication that it is extremely difficult to perceive the simple errors at the basis. It is like fighting the hydra—cut off one ugly head, and eight formalizations take its place. In this situation the only answer is superficiality: when sophistication loses content then the only way of keeping in touch with reality is to be crude and superficial. This is what I intend to be.

Against Method

There is a method, says part (1) of the argument. What is it? How does it work?

One answer which is no longer as popular as it used to be is that science works by collecting facts and inferring theories from them. The answer is unsatisfactory as theories never *follow from* facts in the strict logical sense. To say that they may yet be *supported* from facts assumes a notion of support that (a) does not show this defect and (b) is sufficiently sophisticated to permit us to say to what extent, say, the theory of relativity is supported by the facts. No such notion exists today, nor is it likely that it will ever be found (one of the problems is that we need a notion of support in which grey ravens can be said to support "all ravens are black"). This was realised by conventionalists and transcendental idealists who pointed out that theories *shape* and *order* facts and can therefore be retained come what may. They can be retained because the human mind either consciously or unconsciously carries out its ordering function. The trouble with these views is that they assume for the mind what they want to explain for the world, viz., that it works in a regular fashion. There is only one view which overcomes all these difficulties. It was invented twice in the nineteenth century, by Mill, in his immortal essay *On Liberty,* and by some Darwinists who extended Darwinism to the battle of ideas. This view

takes the bull by the horns: theories cannot be justified and their excellence cannot be shown without reference to other theories. We may explain the *success* of a theory by reference to a more comprehensive theory (we may explain the success of Newton's theory by using the general theory of relativity); and we may explain our *preference* for it by comparing it with other theories.
59 Such a comparison does not establish the intrinsic excellence of the theory we have chosen. As a matter of fact, the theory we have chosen may be pretty lousy. It may contain contradictions, it may conflict with well-known facts, it may be cumbersome, unclear, ad hoc in decisive places, and so on. But it may still be better than any other theory that is available at the time. It may in fact be the best lousy theory there is. Nor are the standards of judgement chosen in an absolute manner. Our sophistication increases with every choice we make, and so do our standards. Standards compete just as theories compete and we choose the standards most appropriate to the historical situation in which the choice occurs. The rejected alternatives (theories; standards; "facts") are not eliminated. They serve as correctives (after all, we may have made the wrong choice) and they also explain the content of the preferred views (we understand relativity better when we understand the structure of its competitors; we know the full meaning of freedom only when we have an idea of life in a totalitarian state, of its advantages—and there are many advantages—as well as of its disadvantages). Knowledge so conceived is an ocean of alternatives channelled and subdivided by an ocean of standards. It forces our mind to make imaginative choices and thus makes it grow. It makes our mind capable of choosing, imagining, criticising.

Today this view is often connected with the name of Karl Popper. But there are some very decisive differences between Popper and Mill. To start with, Popper developed his view to solve a special problem of epistemology—he wanted to solve "Hume's problem." Mill, on the other hand, is interested in conditions favourable to human growth. His epistemology is the result of a certain theory of man, and not the other way around. Also Popper, being influenced by the Vienna Circle, improves on the logical form of a theory before discussing it, while Mill uses every theory in the form in which it occurs in science. Thirdly, Popper's standards of comparison are rigid and fixed, while Mill's standards are permitted to change with the historical situation. Finally, Popper's standards eliminate competitors once and for all: theories that are either not falsifiable or falsifiable and falsified have no place in science. Popper's criteria are clear, unambiguous, precisely formulated; Mill's criteria are not. This would be an advantage if science itself were clear, unambiguous, and precisely formulated. Fortunately, it is not.

To start with, no new and revolutionary scientific theory is ever formulated in a manner that permits us to say under what circumstances we must regard it as endangered: many revolutionary theories are unfalsifiable. Falsifiable versions do exist, but they are hardly ever in agreement with accepted basic statements: every moderately interesting theory is falsified. Moreover, theories have formal flaws, many of them contain contradictions, ad hoc adjustments, and so on and so forth. Applied resolutely, Popperian criteria would eliminate science without replacing it by anything comparable. They are useless as an aid to science. In the past decade this has been realised by various thinkers, Kuhn and Lakatos among them. Kuhn's ideas are interesting but, alas, they are much too vague to give rise to anything but lots of hot air. If you don't believe
60 me, look at the literature. Never before has the literature on the philosophy of science been invaded by so many creeps and incompetents. Kuhn encourages people who have no idea why a stone falls to the ground to talk with assurance about scientific method. Now I have no objection to incompetence but I do object when incompetence is accompanied by boredom and self-righteousness.

And this is exactly what happens. We do not get interesting false ideas, we get boring ideas or words connected with no ideas at all. Secondly, wherever one tries to make Kuhn's ideas more definite one finds that they are *false.* Was there ever a period of normal science in the history of thought? No—and I challenge anyone to prove the contrary.

Lakatos is immeasurably more sophisticated than Kuhn. Instead of theories he considers research programmes which are sequences of theories connected by methods of modification, so-called heuristics. Each theory in the sequence may be full of faults. It may be beset by anomalies, contradictions, ambiguities. What counts is not the shape of the single theories, but the tendency exhibited by the sequence. We judge historical developments and achievements over a period of time, rather than the situation at a particular time. History and methodology are combined into a single enterprise. A research programme is said to progress if the sequence of theories leads to novel predictions. It is said to degenerate if it is reduced to absorbing facts that have been discovered without its help. A decisive feature of Lakatos' methodology is that such evaluations are no longer tied to methodological rules which tell the scientist either to retain or to abandon a research programme. Scientists may stick to a degenerating programme; they may even succeed in making the programme overtake its rivals and they therefore proceed rationally whatever they are doing (provided they continue calling degenerating programmes degenerating and progressive programmes progressive). This means that Lakatos offers *words* which *sound* like the elements of a methodology; he does not offer a methodology. There is no method according to the most advanced and sophisticated methodology in existence today. This finishes my reply to part (1) of the specific argument.

Against Results

According to part (2), science deserves a special position because it has produced *results.* This is an argument only if it can be taken for granted that nothing else has ever produced results. Now it may be admitted that almost everyone who discusses the matter makes such an assumption. It may also be admitted that it is not easy to show that the assumption is false. Forms of life different from science either have disappeared or have degenerated to an extent that makes a fair comparison impossible. Still, the situation is not as hopeless as it was only a decade ago. We have become acquainted with methods of medical diagnosis and therapy which are effective (and perhaps even more effective than the corresponding parts of Western medicine) and which are yet based on an ideology that is radically different from the ideology of Western science. We have learned that there are phenomena such as telepathy and telekinesis which are obliterated by a scientific approach and which could be used to do research in an entirely novel way (earlier thinkers such as Agrippa of Nettesheim, John Dee, and even Bacon were aware of these phenomena). And then—is it not the case that the Church saved souls while science often does the very opposite? Of course, nobody now believes in the ontology that underlies this judgement. Why? Because of ideological pressures identical with those which today make us listen to science to the exclusion of everything else. It is also true that phenomena such as telekinesis and acupuncture may eventually be absorbed into the body of science and may therefore be called "scientific." But note that this happens only after a long period of resistance during which a science *not yet* containing the phenomena wants to get the upper hand over forms of life that contain them. And this leads to a further objection against part (2) of the specific argument. The fact that science has results counts in its favour only if these results were achieved by science alone, and without any outside help. A look at history shows that science hardly ever gets its results in this way. When Copernicus introduced a new view of the universe, he did not consult *scientific* predecessors, he consulted a crazy Pythagorean such as Philolaos. He adopted his ideas and he maintained them in the face of all sound rules of scientific method. Mechanics and optics owe a lot to artisans, medicine to midwives and witches. And in our own day we have seen how the interference of the state can advance science: when the Chinese communists refused to be intimidated by the judgement of experts and ordered traditional medicine back into universities and hospitals there was an outcry all over the world that science would now be ruined in China. The very opposite occurred: Chinese science advanced and Western science learned from it. Wherever we look we see that great scientific advances are due to outside interference which is made to prevail in the face of the most basic and most "rational" methodological rules. The lesson is plain: there does not exist a single argument that could be used to support the exceptional role which science today plays in society. Science has done many things, but so have other ideologies. Science often proceeds systematically, but so do other ideologies (just consult the records of the many doctrinal debates that took place in the Church) and, besides, there are no overriding rules which are adhered to under any circumstances; there is no "scientific methodology" that can be used to separate science from the rest. *Science is just one of the many ideologies that propel society and it should be treated as such* (this statement applies even to the most progressive and most dialectical sections of science). What consequences can we draw from this result?

The most important consequence is that there must be a *formal separation between state and science* just as there is now a formal separation between state and church. Science may influence society but only to the extent to which any political or other pressure group is permitted to influence society. Scientists may be consulted on important projects but the final judgement must be left to the democratically elected consulting bodies. These bodies will consist mainly of laymen. Will the laymen be ablé to come to a correct judgement? Most certainly, for the competence, the complications and the successes of science are vastly exaggerated. One of the most exhilarating experiences is to see how a lawyer, who is a layman, can find holes in the testimony, the technical testimony, of the most advanced expert and thus prepare the jury for its verdict. Science is not a closed book that is understood only after years of training. It is an intellectual discipline that can be examined and criticised by anyone who is interested and that looks difficult and profound only because of a systematic campaign of obfuscation carried out by many scientists (though, I am happy to say, not by all). Organs of the state should never hesitate to reject the judgement of scientists when they have reason for doing so. Such rejection will educate the general public, will make it more confident, and it may even lead to improvement. Considering the sizeable chauvinism of the scientific establishment we can say: the more Lysenko affairs, the better (it is not the *interference* of the state that is objectionable in the case of Lysenko, but the *totalitarian* interference which kills the opponent rather than just neglecting his advice). Three cheers to the fundamentalists in California who succeeded in having a dogmatic formulation of the theory of evolution removed from the text books and an account of Genesis included. (But I know that they would become as chauvinistic and totalitarian as scientists are today when given the chance to run society all by themselves. Ideologies are marvellous when used in the companies of other ideologies. They become boring and doctrinaire as soon as their merits lead to the removal of their opponents.) The most important change, however, will have to occur in the field of *education*.

Education and Myth

The purpose of education, so one would think, is to introduce the young into life, and that means: into the *society* where they are born and into the *physical universe* that surrounds the society. The method of education often consists in the teaching of some *basic myth.* The myth is available in various versions. More advanced versions may be taught by initiation rites which firmly implant them into the mind. Knowing the myth, the grownup can explain almost everything (or else he can turn to experts for more detailed information). He is the master of Nature and of Society. He understands them both and he knows how to interact with them. However, *he is not the master of the myth that guides his understanding.*

Such further mastery was aimed at, and was partly achieved, by the Presocratics. The Presocratics not only tried to understand the *world.* They also tried to understand, and thus to become the masters of, the *means of understanding the world.* Instead of being content with a single myth they developed many and so diminished the power which a well-told story has over the minds of men. The sophists introduced still further methods for reducing the debilitating effect of interesting, coherent, "empirically adequate" etc. etc. tales. The achievement of these thinkers were not appreciated and they certainly are not understood today. When teaching a myth we want to increase the chance that it will be understood (i.e. no puzzlement about any feature of the myth), believed, *and accepted.* This does not do any harm when the myth is counterbalanced by other myths: even the most dedicated (i.e. totalitarian) instructor in a certain version of Christianity cannot prevent his pupils from getting in touch with Buddhists, Jews and other disreputable people. It is very different in the case of science, or of rationalism where the field is almost completely dominated by the believers. In this case it is of paramount importance to strengthen the minds of the young, and "strengthening the minds of the young" means strengthening them *against* any easy acceptance of comprehensive views. What we need here is an education that makes people *contrary, counter-suggestive,* without making them incapable of devoting themselves to the elaboration of any single view. How can this aim be achieved?

It can be achieved by protecting the tremendous imagination which children possess and by developing to the full the spirit of contradiction that exists in them. On the whole children are much more intelligent than their teachers. They succumb, and give up their intelligence because they are bullied, or because their teachers get the better of them by emotional means. Children can learn, understand, and keep separate two to three different languages ("children" and by this I mean three to five year olds, *not* eight year olds who were experimented upon quite recently and did not come out too well; why? because they were already loused up by incompetent teaching at an earlier age). Of course, the languages must be introduced in a more interesting way than is usually done. There are marvellous writers in all languages who have told marvellous stories—let us begin our language teaching with *them* and not with "der Hund hat einen Schwanz" and similar inanities. Using stories we may of course also introduce "scientific" accounts, say, of the origin of the world and thus make the children acquainted with science as well. But science must not be given any special position except for pointing out that there are lots of people who believe in it. Later on the stories which have been told will be supplemented with "reasons," where by reasons I mean further accounts of the kind found in the tradition to which the story belongs. And, of course, there will also be contrary reasons. Both reasons and contrary reasons will be told by the experts in the fields and so the young generation becomes acquainted with all kinds of sermons and all types of wayfarers. It becomes

acquainted with them, it becomes acquainted with their stories, and every individual can make up his mind which way to go. By now everyone knows that you can earn a lot of money and respect and perhaps even a Nobel Prize by becoming a scientist, so many will become scientists. They will *become* scientists *without having been taken in by the ideology of science*, they will *be* scientists *because they have made a free choice*. But has not much time been wasted on unscientific subjects and will this not detract from their competence once they have become scientists? Not at all! The progress of science, of good
64 science depends on novel ideas and on intellectual freedom: science has very often been advanced by outsiders (remember that Bohr and Einstein regarded themselves as outsiders). Will not many people make the wrong choice and end up in a dead end? Well, that depends on what you mean by a "dead end." Most scientists today are devoid of ideas, full of fear, intent on producing some paltry result so that they can add to the flood of inane papers that now constitutes "scientific progress" in many areas. And, besides, what is more important? To lead a life which one has chosen with open eyes, or to spend one's time in the nervous attempt of avoiding what some not so intelligent people call "dead ends"? Will not the number of scientists decrease so that in the end there is nobody to run our precious laboratories? I do not think so. Given a choice many people may choose science, for a science that is run by free agents looks much more attractive than the science of today which is run by slaves, slaves of institutions and slaves of "reason." And if there is a temporary shortage of scientists the situation may always be remedied by various kinds of incentives. Of course, scientists will not play any predominant role in the society I envisage. They will be more than balanced by magicians, or priests, or astrologers. Such a situation is unbearable for many people, old and young, right and left. Almost all of you have the firm belief that at least *some* kind of truth has been found, that it must be preserved, and that the method of teaching I advocate and the form of society I defend will dilute it and make it finally disappear. You have this firm belief; many of you may even have reasons. *But what you have to consider is that the absence of good contrary reasons is due to a historical accident*; it does *not* lie in the nature of things. Build up the kind of society I recommend and the views you now despise (without knowing them, to be sure) will return in such splendour that you will have to work hard to maintain your own position and will perhaps be entirely unable to do so. You do not believe me? Then look at history. Scientific astronomy was firmly founded on Ptolemy and Aristotle, two of the greatest minds in the history of Western Thought. Who upset their well-argued, empirically adequate and precisely formulated system? Philolaos the mad and antediluvian Pythagorean. How was it that Philolaos could stage such a comeback? Because he found an able defender: Copernicus. Of course, you may follow your intuitions as I am following mine. But remember that your intuitions are the result of your "scientific" training where by science I also mean the science of Karl Marx. My training, or, rather, my non-training, is that of a journalist who is interested in strange and bizarre events. Finally, is it not utterly irresponsible, in the present world situation, with millions of people starving, others enslaved, downtrodden, in abject misery of body and mind, to think luxurious thoughts such as these? Is not freedom of choice a luxury under such circumstances? Is not the flippancy and the humour I want to see combined with the freedom of choice a luxury under such circumstances? Must we not give up all self-indulgence and *act*? Join together, and *act*? This is the most important objection which today is raised against an approach such
65 as the one recommended by me. It has tremendous appeal, it has the appeal of unselfish dedication. Unselfish dedication—to what? Let us see!

We are supposed to give up our selfish inclinations and dedicate ourselves to the liberation of the oppressed. And selfish inclinations are what? They are our wish for maximum liberty of thought in the society in which we live *now*, maximum liberty not only of an abstract kind, but expressed in appropriate institutions and methods of teaching. This wish for concrete intellectual and physical liberty in our own surroundings is to be put aside, for the time being. This assumes, first, that we do not need this liberty for our task. It assumes that we can carry out our task with a mind that is firmly closed to some alternatives. It assumes that the correct way of liberating others *has always been found* and that all that is needed is to carry it out. I am sorry, I cannot accept such doctrinaire self-assurance in such extremely important matters. Does this mean that we cannot act at all? It does not. But it means that *while acting we have to try to realise as much of the freedom I have recommended so that our actions may be corrected in the light of the ideas we get while increasing our freedom.* This will slow us down, no doubt, but are we supposed to charge ahead simply because some people tell us that they have found an explanation for all the misery and an excellent way out of it? Also we want to liberate people not to make them succumb to a new kind of slavery, *but to make them realise their own wishes,* however different these wishes may be from our own. Self-righteous and narrow-minded liberators cannot do this. As a rule they soon impose a slavery that is worse, because more systematic, than the very sloppy slavery they have removed. And as regards humour and flippancy the answer should be obvious. Why would anyone want to liberate anyone else? Surely not because of some *abstract* advantage of liberty but because liberty is the best way to free development *and thus to happiness.* We want to liberate people so that *they can smile.* Shall we be able to do this if we ourselves have forgotten how to smile and are frowning on those who still remember? Shall we then not spread another disease, comparable to the one we want to remove, the disease of puritanical self-righteousness? Do not object that dedication and humour do not go together—Socrates is an excellent example to the contrary. *The hardest task needs the lightest hand or else its completion will not lead to freedom but to a tyranny much worse than the one it replaces.*

E. D. Klemke, Robert Hollinger, and A. David Kline, *Introductory readings in the philosophy of science*, Prometheus Books, Buffalo, NY, 1980, pp. 55–65.

J. M. Ziman 2.5

WHAT IS SCIENCE?

To answer the question 'What is Science?' is almost as presumptuous as to try to state the meaning of Life itself. Science has become a major part of the stock of our minds; its products are the furniture of our surroundings. We must accept it, as the good lady of the fable is said to have agreed to accept the Universe. 1

Yet the question is puzzling rather than mysterious. Science is very clearly a conscious artefact of mankind, with well-documented historical origins, with a definable scope and content, and with recognizable professional practitioners and exponents. The task of defining Poetry, say, whose subject matter is by common consent ineffable, must be self-defeating. Poetry has no rules, no method, no graduate schools, no logic: the bards are self-anointed and their spirit bloweth where it listeth. Science, by contrast, is rigorous, methodical, academic, logical and practical. The very facility that it gives us, of clear understanding, of seeing things sharply in focus, makes us feel that the instrument itself is very real and hard and definite. Surely we can state, in a few words, its essential nature.

It is not difficult to state the order of being to which Science belongs. It is one of the categories of the intellectual commentary that Man makes on his World. Amongst its kith and kin we would put Religion, Art, Poetry, Law, Philosophy, Technology, etc.—the familiar divisions or 'Faculties' of the Academy or the Multiversity.

At this stage I do not mean to analyse the precise relationship that exists between Science and each of these cognate modes of thought; I am merely asserting that they are on all fours with one another. It makes some sort of sense (though it may not always be stating a truth) to substitute these words for one another, in 2 phrases like '*Science* teaches us...' or 'The Spirit of *Law* is...' or '*Technology* benefits mankind by...' or 'He is a student of *Philosophy*'. The famous 'conflict between Science and Religion' was truly a battle between combatants of the same species—between David and Goliath if you will—and not, say, between the Philistine army and a Dryad, or between a point of order and a postage stamp.

Science is obviously like Religion, Law, Philosophy, etc. in being a more or less coherent set of ideas. In its own technical language, Science is information; it does not act directly on the body; it speaks to the mind. Religion and Poetry, we may concede, speak also to the emotions, and the statements of Art can seldom be written or expressed verbally—but they all belong in the non-material realm.

But in what ways are these forms of knowledge ***unlike*** one another? What are the special attributes of Science? What is the criterion for drawing lines of demarcation about it, to distinguish it from Philosophy, or from Technology, or from Poetry?

This question has long been debated. Famous books have been devoted to it. It has been the theme of whole schools of philosophy. To give an account of all the answers, with all their variations, would require a history of Western thought. It is a daunting subject. Nevertheless, the types of definition with which we are familiar can be stated crudely.

Science is the Mastery of Man's Environment. This is, I think, the vulgar conception. It identifies Science with its products. It points to penicillin or to an artificial satellite and tells us of all the wonderful further powers that man will soon acquire by the same agency.

This definition enshrines two separate errors. In the first place it confounds Science with Technology. It puts all its emphasis on the applications of scientific knowledge and gives no hint as to the intellectual procedures by which that knowledge may be successfully obtained. It does not really discriminate between Science and
3 Magic, and gives us no reason for studies such as Cosmology and Pure Mathematics, which seem entirely remote from practical use.

It also confuses ideas with things. Penicillin is not Science, any more than a cathedral is Religion or a witness box is Law. The material manifestations and powers of Science, however beneficial, awe-inspiring, monstrous, or beautiful, are not even symbolic; they belong in a different logical realm, just as a building is not equivalent to or symbolic of the architect's blueprints. A meal is not the same thing as a recipe.

Science is the Study of the Material World. This sort of definition is also very familiar in popular thought. It derives, I guess, from the great debate between Science and Religion, whose outcome was a treaty of partition in which Religion was left with the realm of the Spirit whilst Science was allowed full sway in the territory of Matter.

Now it is true that one of the aims of Science is to provide us with a Philosophy of Nature, and it is also true that many questions of a moral or spiritual kind cannot be answered at all within a scientific framework. But the dichotomy between Matter and Spirit is an obsolete philosophical notion which does not stand up very well to careful critical analysis. If we stick to this definition we may end up in a circular argument in which Matter is only recognizable as the subject matter of Science. Even then, we shall have stretched the meaning of words a long way in order to accommodate Psychology, or Sociology, within the Scientific stable.

This definition would also exclude Pure Mathematics. Surely this is wrong. Mathematical thinking is so deeply entangled with the physical sciences that one cannot draw a line between them. Modern mathematicians think of themselves as exploring the logi-

cal consequences (the 'theorems') of different sets of hypotheses or 'axioms', and do not claim absolute truth, in a material sense, for their results. Theoretical physicists and applied mathematicians try to confine their explorations to systems of hypotheses that they believe to reflect properties of the 'real' world, but they often have no licence for this belief. It would be absurd to have to 4
say that Newton's *Principia*, and all the work that was built upon it, was not now Science, just because we now suppose that the inverse square law of gravitation is not perfectly true in an Einsteinian universe. I suspect that the exclusion of the 'Queen of the Sciences' from her throne is a relic of some ancient academic arrangement, such as the combination of classical literary studies with mathematics in the Cambridge Tripos, and has no better justification than that Euclid and Archimedes wrote in Greek.

Science is the Experimental Method. The recognition of the importance of experiment was the key event in the history of Science. The Baconian thesis was sound; we can often do no better today than to follow it.

Yet this definition is incomplete in several respects. It arbitrarily excludes pure mathematics, and needs to be supplemented to take cognisance of those perfectly respectable sciences such as Astronomy or Geology where we can only observe the consequences of events and circumstances over which we have no control. It also fails to give due credit to the strong theoretical and logical sinews that are needed to hold the results of experiments and observations together and give them force. Scientists do not in fact work in the way that operationalists suggest; they tend to look for, and find, in Nature little more than they believe to be there, and yet they construct airier theoretical systems than their actual observations warrant. Experiment distinguishes Science from the older, more speculative ways to knowledge but it does not fully characterize the scientific method.

Science arrives at Truth by logical inferences from empirical observations. This is the standard type of definition favoured by most serious philosophers. It is usually based upon the principle of induction—that what has been seen to happen a great many times is almost sure to happen invariably and may be treated as a basic fact or Law upon which a firm structure of theory can be erected.

There is no doubt that this is the official philosophy by which most practical scientists work. From it one can deduce a number 5
of practical procedures, such as the testing of theory by 'predictions' of the results of future observations, and their subsequent confirmation. The importance of speculative thinking is recognized, provided that it is curbed by conformity to facts. There is no restriction of a metaphysical kind upon the subject matter of Science, except that it must be amenable to observations and inference.

But the attempt to make these principles logically watertight does not seem to have succeeded. What may be called the posithe foundations of knowledge he is content to leave the highly technical discussion of the nature of Science to those self-appointed authorities the Philosophers of Science. A rough and ready conventional wisdom will see him through.

Yet in a way this neglect of—even scorn for—the Philosophy of Science by professional scientists is strange. They are, after all, engaged in a very difficult, rather abstract, highly intellectual activity and need all the guidance they can from general theory. We may agree that the general principles may not in practice be very helpful, but we might have thought that at least they would
be taught to young scientists in training, just as medical students 7
are taught Physiology and budding administrators were once encouraged to acquaint themselves with Plato's *Republic.* When the student graduates and goes into a laboratory, how will he know what to do to make scientific discoveries if he has not been taught the distinction between a scientific theory and a non-scientific one? Making all allowances for the initial prejudice of scientists against speculative philosophy, and for the outmoded assumption that certain general ideas would communicate themselves to the educated and cultured man without specific instruction, I find this an odd and significant phenomenon.

The fact is that scientific investigation, as distinct from the theoretical *content* of any given branch of science, is a practical art. It is not learnt out of books, but by imitation and experience. Research workers are trained by apprenticeship, by working for their Ph.Ds under the supervision of more experienced scholars, not by attending courses in the metaphysics of physics. The graduate student is given his 'problem': 'You might have a look at the effect of pressure on the band structure of the III–V compounds; I don't think it has been done yet, and it would be interesting to see whether it fits into the pseudopotential theory'. Then, with considerable help, encouragement and criticism, he sets up his apparatus, makes his measurements, performs his calculations, etc. and in due course writes a thesis and is accounted a qualified professional. But notice that he will not at any time have been made to study formal logic, nor will he be expected to defend his thesis in a step by step deductive procedure. His examiners may ask him why he had made some particular assertion in the course of his argument, or they may enquire as to the reliability of some particular measurement. They may even ask him to assess the value of the 'contribution' he has made to the subject as a whole. But they will not ask him to give any opinion as to whether Physics is ultimately *true*, or whether he is justified now in believing in an external world, or in what sense a theory is verified by the observation of favourable instances. The examiners will assume that the candidate shares with them the common language and
principles of their discipline. No scientist really doubts that theories 8
are verified by observation, any more than a Common Law judge hesitates to rule that hearsay evidence is inadmissible.

What one finds in practice is that scientific argument, written or spoken, is not very complex or logically precise. The terms and concepts that are used may be extremely subtle and technical, but scientist sees through his own eyes—and also through the eyes of his predecessors and colleagues. It is never one individual that goes through all the steps in the logico-inductive chain; it is a group of individuals, dividing their labour but continuously and jealously checking each other's contributions. The cliché of scientific prose betrays itself 'Hence *we* arrive at the conclusion that...' The audience to which scientific publications are addressed is not passive; by its cheering or booing, its bouquets or brickbats, it actively controls the substance of the communications that it receives.

In other words, scientific research is a social activity. Tech- 10
nology, Art and Religion are perhaps possible for Robinson Crusoe, but Law and Science are not. To understand the nature of Science, we must look at the way in which scientists behave towards one another, how they are organized and how information passes between them. The young scientist does not study formal logic, but he learns by imitation and experience a number of conventions that embody strong social relationships. In the language of Sociology, he learns to play his *role* in a system by which knowledge is acquired, sifted and eventually made public property.

It has, of course, long been recognized that Science is peculiar in its origins to the civilization of Western Europe. The question of the social basis of Science, and its relations to other organizations and institutions of our way of life, is much debated. Is it a consequence of the 'Bourgeois Revolution', or of Protestantism—or what? Does it exist despite the Church and the Universities, or because of them? Why did China, with its immense technological and intellectual resources, not develop the same system? What should be the status of the scientific worker in an advanced society; should he be a paid employee, with a prescribed field of study, or an aristocratic dilettante? How should decisions be taken about expenditure on research? And so on.

These problems, profoundly sociological, historical and political though they may be, are not quite what I have in mind. Only too often the element in the argument that gets the least analysis is the actual institution about which the whole discussion hinges—scientific activity itself. To give a contemporary example, there is much talk nowadays about the importance of creating more effective systems for storing and indexing scientific literature, so that every scientist can very quickly become aware of the relevant work of every other scientist in his field. This recognizes that publication is important, but the discussion usually betrays an absence of careful thought about the part that conventional systems of scientific communication play in sifting and sorting the material that they handle. Or again, the problem of why Greek Science never finally took off from its brilliant taxying runs is discussed in
terms of, say, the aristocratic citizen despising the servile labour 11
of practical experiment, when it might have been due to the absence of just such a communications system between scholars as was provided in the Renaissance by alphabetic printing. The internal sociological analysis of Science itself is a necessary preliminary to the study of the Sociology of Knowledge in the secular

The subject is indeed endless. As pointed out in the Preface, the present brief essay is meant only as an exposition of a general theory, which will be applied to a variety of more specific instances in a larger work. The topics discussed here are chosen, therefore, solely to exemplify the main argument, and are not meant to comprehend the whole field. In many cases, also, the discussion has been kept abstract and schematic, to avoid great marshlands of detail. The reader is begged, once more, to forgive the inaccuracies and imprecisions inevitable in such an account, and to concentrate his critical attention upon the validity of the general principle and its power of explaining how things really are.

* 'Hence a true philosophy of science must be a philosophy of scientists and laboratories as well as one of waves, particles and symbols.' Patrick Meredith in *Instruments of Communication*, p. 40.

J. M. Ziman, *Public knowledge*, Cambridge University Press, Cambridge, 1968, pp. 1–12.

2.6

Thomas F. Gieryn

MAKING THE DEMARCATION OF SCIENCE A SOCIOLOGICAL PROBLEM

The history of failed philosophical attempts, from Aristotle to Popper, to 60
generate criteria that will fence in all good science but keep out everything else, has led Larry Laudan to conclude that demarcation is a pseudo-problem. The terms "pseudo-science" and "unscientific," he says, should be removed from the rational discourse of empirical research. As a sociologist of scientific knowledge, I cannot share Laudan's philosophical luxury of talking about scientists by ignoring what they say and do. So long as scientists refer to pseudo-science and non-science-- and they do, of course-- the sociologist will face a demarcation problem that is anything but "pseudo."

This paper introduces a sociological model which views demarcation as *practical activity* of scientists, carried on by them in order to distinguish scientists from others who might claim the authority and resources sought (and often acquired) by the institution of science. Neither philosophers nor sociologists will ever arrive at *definitive* criteria for what makes science unique, because these are perpetually under negotiation as scientists battle non-scientists for "cognitive authority" amidst changing historical circumstances. Scientists themselves have most at stake in successfully convincing others that their style of producing knowledge is different, and their attempts at "boundary-work"-- a combination of rhetorical and social organizational devices designed to exclude some people and their knowledge-claims from science-- constitute the object of this sociological analysis. The familiar features sometimes attributed to science alone-- its objective, cumulative, empirical, theoretical and reliable nature-- are examined as "boundary-setting devices" used by scientists to demarcate themselves from non-scientists. These boundary-setting devices vary contextually depending upon the arguments put forth by one or another competitor from non-science.

The demarcation model offered here is not another attempt to adjudicate 61
the boundary between science and non-science. It has instead the more ambitious aim of explaining *why* scientists claim for science certain attributes, by showing how these effectively serve the goal of demarcation. Through the investigation of how scientists have excluded the varieties of non-science, we learn much about why science looks, sociologically, the way it does.

For this orientation, it is axiomatic that non-science become something failed on all counts: their description of a supernatural God creating something from nothing, and their literal interpretation of Genesis as infallible wisdom, exposed them as religionists in scientific dress.[3]

(iii) The decision by the American government to build an atomic bomb 63
in the mid-1940s forced a number of physicists to consider the boundaries of science. I. I. Rabi explained his refusal to take a full-time position at Los Alamos with a bit of boundary-work: "If we take the stand that our object is merely to see that the next war is bigger and better, we will ultimately lose the respect of the public. In popular demagogy we [will] become the unpaid servants of the 'munitions makers' and mere technicians..."[4] For Rabi, if not for those in the Manhattan Project, science must retain autonomy from political ambitions by refusing to become a handmaiden of government functionaries.

(iv) Near the turn of this century, Emile Durkheim tried to show what was scientific about his sociology by demarcating it from the ideology of Saint-Simonian socialism. Sociology sought descriptions of what is and what has been; the non-science of socialism sought descriptions of what ought to be. Durkheim contrasted his painstaking empirical research with the ideological socialists' reliance upon "rare and meagre" data to support their wishes for a better world.[5]

(v) Aristotle and Plato tried mightily to demarcate their science from its practical applications in engineering and craft. If shipbuilders could build a vessel that floats, they could not possess the scientists' understanding of the causal principles underlying their success. Aristotle denies the need for a utilitarian rationalization of the pursuit of knowledge: "Just as we call a man free who exists for his own ends and not those of another, so it is with this, which is the only free man's science: it alone of the sciences exists for its own sake."[6] Of course, the boundary between science and technology (or
engineering, mechanics, crafts) has repeatedly been erased, and drawn anew, 64
ever since.

This set of cases is a mixed bag only on the surface. Each case shows scientists doing boundary-work, defining science for purposes of excluding others as non-science. Velikovsky, creation scientists, government technocrats, socialist ideologues and Greek artisans are put outside science for reasons distinctive to each case. Figure 1 might be a useful way to visualize the diffraction of non-science. It makes apparent that science and non-science have not one boundary, but many boundaries. Each boundary may require its own definitions of science to be successfully defended by scientists.

nothing about boundaries between the non-sciences, although they are of 66
equal sociological interest, as Howard Becker's analysis of boundary-negotiations between art and craft demonstrates.[7] Finally, "internal" boundary disputes among scientists-- over, say, whether one is an astronomer or astrophysicist-- are of no interest here unless they involve charges that either party is beyond the scientific pale.

Boundary-Work of Scientists: Analytic Issues

This demarcation model of science suggests five distinctively sociological questions, although I can only hint at the kinds of empirical analysis each issue invites.

(1) What is at stake in scientists' efforts at demarcation? Why are scientists and non-scientists pictured as adversaries and competitors? Briefly, scientists compete with non-scientists for "cognitive authority" and its derivative material and symbolic resources. Cognitive authority is legitimate power to promulgate and disseminate knowledge that is accepted as truthful and reliable. A variety of resources typically accompany cognitive authority: money and time to create truth, and access to institutionalized means for spreading it (e.g., systems of education). Societies vary in how they choose to confer cognitive authority: it may be centralized in the hands of one individual, role or social institution; or it may be widely diffused among a number of them. Delegation of cognitive authority to, say, a witchdoctor, does not prevent other members of the tribe from voicing opinions about the truth. But only the witchdoctor can serve as the court of
last resort in settling disputes over *legitimate* knowledge in designated fields. 67
Individuals can disregard the knowledge-claims of cognitive authorities, just as lawbreakers flout the political authority of the state.

In modern, Western societies, the institution of science possesses cognitive authority over a growing domain of knowledge-claims. While we rarely see the need to phone a professional meteorologist when speculating on how cold it is outside, it is not surprising that TV weather reporters have started to broadcast quasi-scientific credentials before they hazard a guess on tomorrow's chance of rain. If the growing cognitive authority of science explains (in part) why TV weather reporters of the future will mention their Ph.D.'s, it also explains the more consequential $1 billion or so given annually to basic scientific research by the Federal Government, and it explains why science education is an integral part of public school curricula, why scientists are called before courtrooms and hearing rooms to provide a "truthful and reliable" context for decision-making, and in a different way, the cumulative scientific record of facts, because it is too-loosely empirical and because it takes the Bible as reliable evidence. Boundary-work consists of arguments put forth by scientists to show how they are different from non-scientists, and of organizational mechanisms through which those criteria are used to exclude some from science.

(3) While boundary-work *by scientists* is at the focus of this analysis, they are not the only ones who do it. At least three other parties make the demarcation between science and non-science. The various non-scientific competitors have much at stake in drawing their own boundaries between themselves and science, although for some, at certain moments, the best strategy might be to erase the boundary by declaring to *be* science (tried unsuccessfully by creation scientists at Little Rock). Boundary-work is a negotiation because the line between science and non-science is never drawn unilaterally, although at times rivals might agree on what does, or does not, separate them. Also involved in demarcation are those in society who confer cognitive authority and distribute its attendant resources. The citizen who chooses to believe scientists but not politicians, or the politician who chooses to fund scientists but not witchdoctors, is drawing lines of demarcation, if often implicitly. The boundaries drawn by these "audiences" may or may not coincide with those drawn by scientists or their adversaries. Perhaps a measure of the cognitive authority of science today is its ability to get others to believe in its own boundary-work as a matter of faith. But even this achievement does not eliminate the need for sustained efforts at demarcation: scientists must forever remind themselves and others of how they are different from the endless stream of impostors and competitors hoping to share the wealth of science. The last, perhaps least consequential boundary-work is done by philosophers who try to decide once and for all what makes science unique.

(4) Scientists choose from among several strategies of boundary-work. At times, scientists will draw boundaries with great precision: a sharp, impermeable boundary will more effectively *exclude* pseudo-scientists than a fuzzy one. At other times, scientists might enhance their cognitive authority by making the boundary ambiguous, perhaps by emphasizing common ground shared with a rival. The long negotiation between scientists and theologians illustrates this well. Seventeenth-century scientists gained authority by avoiding a precise demarcation from religion: natural philosophers fuzzed up the boundary by claiming to reveal the wonders God has wrought in the

scientific paper, on the behavior of crystalline bodies between the poles of a magnet, in an 1851 issue of *Philosophical Magazine*. After his return to England, Tyndall found scarce employment for scientists, and supported himself by lecturing to the public, writing and examining. His fortunes picked up after election to the Royal Society in 1852, and after he benefitted from the patronage of Michael Faraday, who became Tyndall's role model in everything scientific. At age 33, Tyndall was appointed (by Faraday) professor of natural philosophy at the Royal Institution of Great Britain (London). After Faraday's death in 1867, Tyndall succeeded him as Superintendent of the Royal Institution, a position held until retirement in 1886 at age 66.

Tyndall contributed much to scientific knowledge, but perhaps more to its popularization. His research from 1853 to 1874 focussed on questions of
physics: how does compression affect crystalline structures, what are the 73
effects of solar and heat radiation on atmospheric gases, and why is the sky blue? An interest in airborne dust particles led Tyndall, after the mid-seventies, to examine organic matter in the atmosphere, and led him to defend Pasteur in the spontaneous generation controversy. Tyndall's contributions have been doubly-honored with eponyms: the Tyndall Effect describes scattering of light particles in the atmosphere, and Tyndallization describes the use of discontinuous heating for sterilization. Roy MacLeod writes in his entry on Tyndall in the *Dictionary of Scientific Biography:* "this formidable capacity to move from electromagnetism through thermodynamics and into bacteriology was the hallmark of Tyndall's genius." Still, these contributions do not overshadow his role as statesman for science. He delivered hundreds of popular lectures at the Royal Institution and at provincial mechanics' institutes, while regularly contributing articles on science to the popular press. Tyndall's flaw might have been his obstinancy which, when combined with his highly visible place in the scientific establishment, forced him into many bitter disputes with those inside and out of science. His 1874 Belfast Address as President of the British Association (B.A.A.S.) argued such a hard-line materialism that scientists and religionists alike were offended. Late in life, Tyndall married Louisa Charlotte Hamilton, a thoroughly devoted wife who had the misfortune, one morning in 1893, of confusing magnesia with chloral, and administering a fatal dose of the latter to her husband. She survived him for 47 years.[10]

Tyndall's popular writings on science make up the empirical materials examined here as boundary-work. Although demarcation also goes on in
Tyndall's scientific papers, and elsewhere in the gatekeeping activities of the 74
British Association and contemporary scientific journals, this analysis centers

the telegraph, the electrolyte and the photograph, the medical applications of physics, and the various other inlets by which scientific thought filters into practical life."[14] The contributions of religion lie elsewhere, as Tyndall indicated in the Belfast Address: religion is "capable of adding, in the region of *poetry* and *emotion*, inward completeness and dignity to man."[15]

Third: science is objective knowledge free from emotions, private interests, bias or prejudice; religion is a subjective, idiosyncratic, emotional poem about the world. Tyndall asserts that Genesis should be read as "a poem, not a scientific treatise. In the former aspect it is forever beautiful;
in the latter aspect it has been, and will continue to be, purely obstructive 76
and harmful. To *knowledge* its value has been negative..."[16] Knowledge is the domain of science, for it alone can eliminate subjective emotions from claims to truth: "Testimony as to natural facts is worthless when wrapped in this atmosphere of the affections; the most earnest subjective truth being thus rendered perfectly compatible with the most astounding objective error."[17] A military metaphor suggests that boundary-work for Tyndall was not idle speculation but a matter of life and death for scientists' acquisition of cognitive authority: "It is against this objective rendering of the emotions--this thrusting into the region of fact and positive knowledge of conceptions essentially ideal and poetic-- that science, consciously or unconsciously, wages war."[18]

Fourth: science successfully apprehends the truth because it respects no authority other than the facts of nature; religion fails to find truth because it abides by the authority of old concepts and their creators. Huxley, more succinctly than in anything I have read from Tyndall, describes how the skepticism of science separates it from the faith of religion: "The improver of natural knowledge absolutely refuses to acknowledge authority as such. For him, scepticism is the highest of duties; blind faith the one unpardonable sin. The man of science has learned to believe in justification, not by faith, but by verification."[19] Tyndall echoes this ten years later in 1866: "The first condition of [scientific] success is patient industry, an honest receptivity, and a willingness to abandon all preconceived notions, however cherished, if they be found to contradict the truth."[20] Theologians fail to meet this criterion of science: "The most fatal error that could be committed by the
leaders of religious thought is the attempt to force into their own age 77
conceptions which have lived their life, and come to their natural end in preceeding ages."[21] "Foolishness is far too weak a word to apply to any attempt to force upon a scientific age the edicts of a Jewish lawgiver."[22]

Fifth: scientific knowledge is fallible and finite; religious knowledge is eternally true and, it would seem, limitless. After the Belfast Address, Science as Not-Mechanics

At first glance, it would seem silly for Victorian scientists to draw a precise boundary betwen science and mechanics. After all, Tyndall effectively demarcates science from religion by asserting that Britain's technological progress was dependent upon scientific knowledge. But much 79
was at stake in scientists' successfully arguing that they were something more than mechanicians or artisans. If mechanical ingenuity was perceived as essentially the same as scientific knowledge, then the case for a larger role for science in universities was weakened: why create a curriculum for science if its knowledge could be learned as effectively "on the job"? Also, if the goals of mechanics and science were perceived to be the same, then opponents of government support of scientific research could argue that the practical consequences of science would endow private industry with enough profits to support "pure" scientific research: if science was measured by its practicality, then its inevitable profitability would lessen the need for government financial support. Thus, another side of Tyndall's boundary-work sought to show that scientists were not, or not *just*, mechanicians.

The first argument: mechanicians are good observers, but since they do not perform scientific experiments, they cannot explain their successes or avoid their failures. In an 1876 discourse before the Glasgow Science Lectures Association, Tyndall considers the science of fermentation and the mechanical art of brewing beer: "Indeed, it might be said that until the present year no thorough and scientific account was ever given of the agencies which come into play in the manufacture of beer...Hitherto the art and practice of the brewer have resembled those of the physician, both being founded upon empirical observation. By this is meant the observation of facts apart from the principles which explain them...The brewer learned from long experience the conditions, not the reasons, of success."[26] In a later talk, mainly about rainbows, Tyndall suggests that "from observation we learn what Nature is willing to reveal. In experimenting we place her in the 80
witness-box, cross-examine her, and extract from her the knowledge in excess of that which would, or could, be spontaneously given."[27]

Second: mechanicians are not scientists because they never go beyond observed facts to discover the unseen causal principles that rule them. Tyndall writes: "The outward and visible phenomena are the counters of the intellect, and our science would not be worthy of its name and fame if it halted at the facts, however practically useful, and neglected laws which accompany and rule the phenomena."[28] "One of the most important functions

Homer's *Iliad* is good as a means of culture. There's the rub. The people who demand of science practical uses forget, or do not know, that it is also great as a means of culture,-- that the knowledge of this wonderful universe is a thing profitable in itself, and requiring no practical application to justify 82
its pursuit."[33] Few mechanicians would claim their work is great as a means of culture, just as few theologians could claim that religion was materially useful in solving practical problems. Science is practically useful, and that makes it not-religion; science has the nobler purpose of extending our cultural heritage, and that makes it not-mechanics. The unrelenting strain between basic science and applied science, between pure science and technology, is perhaps understandable sociologically as the result of an institutional need for science to claim both virtues if it is to effectively demarcate itself from these two rivals.

I must close with an ironic twist on Tyndall's boundary-work. For all his efforts to demarcate science from religion and mechanics, Tyndall himself was more than once *excluded* from science and placed-- not with theologians or mechanicians-- but with "popularizers." P. G. Tait (who had little love for Tyndall) wrote to *Nature* in 1877: very few can do the popularization of science "without thereby losing their claim to scientific authority. Dr. Tyndall has, in fact, martyred his scientific authority by deservedly winning distinction in the popular field. One learns too late that he cannot make the best of both worlds..."[34] Tyndall's manifest boundary-work before the public, so important for the institutional success of science in Victorian Britain, may have cost him credibility among his scientific peers.

Conclusion

To some, these materials are scarcely cause for headlines. After all, 83
what else would an informed historian expect from a 19th-century defender of science? Is there really surprise in hearing Tyndall describe science as empirical and practical, theoretical and selfless? Tyndall's assertions are indeed uninteresting if one takes for granted that they are an inherent part of an inevitable evolution of science. They do become interesting when one asks *why* science comes to be described and perceived as empirical and practical, theoretical and selfless. My paper offers one answer: these descriptions effectively demarcate science from various non-sciences. If science forever competes with non-science for cognitive authority and attached resources, an essential first-step is scientists' demonstration that they are different from their rivals. The boundary-work of Tyndall, and that of his

14. Tyndall, *Fragments I*, pp. 364-365.

15. Tyndall, *Fragments II*, p. 209.

16. Tyndall, *Fragments II*, p. 224.

17. Tyndall, *Fragments II*, pp. 19-20.

18. Tyndall, *Fragments II*, p. 393.

19. Thomas Henry Huxley, "On the Advisableness of Improving Natural Knowledge" (1866) in his *Method and Results*, pp. 40-41; quoted in Walter E. Houghton, *The Victorian Frame of Mind*. (New Haven: Yale University Press, 1957), p. 95.

20. Tyndall, *Fragments I*, p. 307.

21. John Tyndall, *New Fragments* (New York: Appleton, 1898), p. 33. Hereafter, *New Fragments*.

22. Tyndall, *New Fragments*, p. 36.

23. Tyndall, *Fragments II*, p. 247.

24. Tyndall, *Fragments II*, p. 210.

25. Tyndall, *Fragments II*, pp. 219-220.

26. Tyndall, *Fragments II*, p. 267.

27. Tyndall, *New Fragments*, p. 213.

28. Tyndall, *Fragments I*, pp. 95-96.

29. Tyndall, *Fragments I*, p. 80.

30. John Tyndall, *Faraday as a Discoverer*, Fifth edition. (London: Longmans, Green, 1894), pp. 141-142.

31. Tyndall, *Fragments II*, pp. 472-473.

32. Tyndall, *Fragments II*, p. 403.

33. Tyndall, *Fragments I*, p. 101.

34. Quoted in J. G. Crowther, *Scientific Types* (London: Barrie and Rockliff, 1968), p. 184.

Rachel Laudan (ed.), *Working papers in science and technology*, vol. 2, no. 1, April 1983, pp. 60-86.

3

SCIENTIFIC REVOLUTIONS

Hugh Kearney
SCIENCE AND CHANGE 1500-1700

3.1

Introduction

The Scientific Revolution of the sixteenth and seventeenth centuries is now generally recognised as a decisive turning point in world history. It has taken its place in the judgment of most historians beside such movements as the Renaissance and Reformation, from which indeed it cannot be entirely dissociated. The innovations which it introduced are seen as a major cause of the transition from traditional modes of thinking, in which authority was accepted as natural and desirable, to 'modernity', in which critical assessment of all assumptions is encouraged as an essential part of maturity. The Scientific Revolution initiated scrutiny of nature and human nature by methods of hypothesis and experiment which were expected to lead to novelty and change.

This is of course an over-simplification. There had been 'Scientific Revolutions' before in the history of the world. By any reckoning, the Neolithic Revolution of 4000 BC represented a qualitative change in man's approach to his natural environment. Between the third and thirteenth centuries AD the Chinese made extraordinary headway in their empirical understanding of the universe. But it was the Greeks of *c.* 500-200 BC, more than any other group, who pressed beyond the accepted frontiers of knowledge to reach revolutionary interpretations of nature.

Chronologically speaking, the Scientific Revolution of Classical and Hellenistic Greece does not fall within the scope of this book, but because of its later influence in Europe it cannot be ignored. The mathematical achievement of Pythagoras (582-500 BC), the speculations of Plato (427-347 BC), the empiricism of Aristotle (384-322 BC), the geometry of Euclid (*c.* 300 BC), the engineering insights of Archimedes (287-212 BC), the astronomical observations of Ptolemy (*fl.* AD 139-161), the anatomical and medical work of Galen (*c.* AD 130-201) – all this was to be rediscovered in the West from the twelfth century onwards, after being lost sight of during the 'Dark Age' which followed the fall of the Roman Empire in the fifth century. 8

The rediscovery of Greek science was a complex process which stretched over five centuries, from the twelfth to the sixteenth. It began with the revival of Aristotelian logic in the twelfth century and the incorporation of further sections of Aristotelian science into Christian philosophy. Of those who took part in the work of 'baptising' Aristotle, the best known was the thirteenth-century theologian Thomas Aquinas (1226-74), but there were many others. These attempts to reconcile Aristotelian science with Christian doctrine were given the general name of 'scholasticism' by later generations of philosophers.

that the celestial and terrestrial worlds were different in nature and hence in the natural laws which applied to them. Newton in his *Optics* showed that white light was composed of coloured rays, and demonstrated dramatically the value of experimental method in reaching revolutionary conclusions.

Looked at in the context of world history, the Scientific Revolution was an extraordinary intellectual leap which had repercussions ultimately on every aspect of western thought and life. A new tradition had been created which was to bear fruit a thousandfold in the eighteenth and nineteenth centuries, though by 1900 the assumptions of the original Scientific Revoltuion had been modified almost out of recognition.

This chronological survey of the Scientific Revolution will serve our purposes as a simple account of what happened in the history of science between 500 BC and AD 1500. But it is open to the criticism of being too simple. It brings out the point that the Scientific Revolution was at once a recovery of Greek thought and a repudiation of it. What it does not emphasise is the complexity of the whole process and its links with religion and philosophy.

Greek thought, after all, was not a unified system, any more than western thought is today. Its various schools of thought held sharply conflicting opinions, which changed over a thousand years of development. After AD 500 western Europe lapsed into semi-barbarism (a state of society not without its compensations in art) and intellectual revival began seriously only *c.* AD 1000. Thus the task of recovering Greek thought was not a relatively simple problem, akin to a man putting together the displaced pages of an encyclopedia. It was the painful effort of a largely barbarian society coming to terms with the intellectual sophistication of a superior civilisation. This was in any event a task of immense difficulty. It was intensified by the fact that Greek thought became available from the twelfth century onwards only in defective Latin translations.

Another problem was the reconciliation of Greek thought with Christian tradition. In broad terms this meant bringing together in a single whole, views based upon the amalgam of Jewish history and poetry called the Bible (in the Greek translation known as the Septuagint, which dated from *c.* 200-100 BC) with the philosophy and science of the advanced urban civilisation of Greece – a formidable task. Medieval scholars had one great advantage in all this: they had no sense of history. They did not distinguish chronologically between the Greek thought of one period and that of another, still less between Greek and Hebraic modes of thought. Instead, they attempted to create a logical construction of views which were historically distinct. Often they accepted as genuine what later were seen to be forgeries.

The whole range of Greek thought did not become available to the West immediately. During the Middle Ages the science and philosophy of Aristotle was studied, beginning with his logic and followed by his physics, metaphysics and biological treatises. The work of Plato was almost unknown. Had Plato's critical open-ended dialogues been known first, it is

. . . I would first like to offer some criticisms of a historical 17
outlook which tends to dominate and distort general accounts of the Scientific Revolution and which I will call 'the Whig interpretation of history'.

The Whig interpretation of history

This interpretation implies a view of the past which divides men essentially into two simple categories, progressive or reactionaries, forward-looking or backward-looking, Protestants or Catholics. As Sir Herbert Butterfield pointed out in a brilliant essay, this way of looking at history leads to gross distortions because it imposes the standards of the present upon the past. One danger lies in the assumption that the purpose of the past has been to prepare the way for the present. Another is to trace a simple line of continuity from past to present. Perhaps the basic error is to substitute explanation based on logical progression for a less rational and more complex interpretation of the past.

The Whig interpretation takes its name from the account of English constitutional history given by nineteenth-century Whig historians, who saw English liberty 'slowly broadening down from precedent to precedent'. The foundations of freedom were laid with Magna Carta in 1215, rescued in the Civil War of the seventeenth century, and confirmed by the Glorious Revolution of 1688, the implications of which were worked out in the Reform Acts of the nineteenth century. The basic assumptions which lay behind this interpretation of English
history was a simple one. Englishmen were divided into two 18
simple categories, those who loved liberty, the Whigs, and those who did not, the Tories. By this test, English history assumed an intelligible pattern, which ultimately was enshrined as a myth.

But it would be misleading to restrict our attention to England and the Whig historians. Most nationalist interpretations of history make similar judgments about the past. History appears as a story of those who supported the rise of the nation and those who did not. Crucial differences between the patriots of one generation and another are lost sight of and essential distinctions between men of the same generation are slurred over. Nationalist history of this kind seems to be the response of an overpowering emotional need within newly established political societies, which feel the need to create a 'past' for themselves.

Perhaps the most influential example of the Whig interpretation of history today is provided by Marxists. This fact is glossed over by the Marxist claim to be writing 'scientific' history and by the undoubted erudition and imagination of many Marxist historians. But behind much Marxist history lies the assumption that history is a record of progress, with progressives on one side and reactionaries on the other. Great play is made with such terms as 'forward-looking' and 'backward-looking' as if it were possible to apply such concepts to the complexities of the past, as if indeed these concepts had meaning of an ascertainable kind. We must group such Marxists with the Whigs, as historians who are committed work has appeared in print since the Second World War than ever before. But even apart from this, there is a general dissatisfaction with the artificial confines of Whig interpretations. Hence, many historians following the example of the French school of Marc Bloch and Lucien Febvre have moved towards 'histoire intégrale'. History written on these lines aims at total history, in order to make intelligible all aspects of life in particular societies, not merely a single line of apparent progress.

To some extent, the Marxists had already pointed the way in this direction. Indeed, it may be said that one of the beneficial effects of Marxist interpretations of history has been to force historians to take the broad view, and to see interconnexions between apparently disparate topics. Unfortunately, however, Marxism in practice is often too rigid and too predictable, providing only one form of explanation and one kind of analogy. A more fruitful example has been set by sociologists
and social anthropologists. We are now less tempted to make 22
simple distinctions between rational and irrational behaviour. We look instead for the social function of certain modes of activity. We see that what is called 'magic' in a primitive society may correspond to what is called 'science' in a more sophisticated one. We are also introduced to a wider range of concepts than is supplied in a Marxist 'class' analysis, such as 'roles', 'function', 'status' and other concepts. And what applies to the political and intellectual historian applies with equal force to the historian of science. Thus the history of science no longer appears as a self-contained activity, an enclosed scientific tradition, with truth 'slowly broadening down from precedent to precedent'. Historians of science are now engaged in seeking the influence of allegedly non-scientific and non-rational factors upon scientists.

To some extent, this approach has been practised for a long time. Darwinism is an obvious case in point. It seems certain that Darwin's scientific imagination was kindled by the Malthusian law of population. Malthus, in stressing the continual pressure of population upon food supply, provided the key for Darwin's theory of natural selection. In other words, to explain the origins of *The Origin of Species*, the historian of science must come to terms with the general history of the early nineteenth century.

The three scientific traditions

In attempting to escape from the bonds of a Whig interpretation of the Scientific Revolution I will argue that there were, during this period, at least three approaches to nature which may be broadly termed 'scientific' in the sense that they all produced discoveries which have been incorporated within
the modern scientific tradition. But 'modernity' is a dangerous 23
criterion. All three of them were bound up with religious assumptions about the universe, whereas modern science by definition is a secular activity. No exponent of a particular tradition had a concept of science in its modern sense, indeed, the term 'scientist' was first invented in the nineteenth century.

animal kingdom and even the processes of artistic creation. From this point of view, the Christian God took on some of the characteristics of an engineer. Mechanists concentrated upon those aspects of the world which were most easily explained in mechanical terms. Questions which were thought marginal within the organic and aesthetic traditions, such as problems of acceleration, took on a new significance within the mechanistic framework. The concept of unchanging scientific laws, expressible in mathematical terms, was of particular importance in this tradition and a mathematical approach came to be its dominant characteristic.

In historical terms we may think of the magical tradition as being a reaction against the organic tradition, and the mechanical as a reaction against magic. But it must be said that within each tradition there were sub-groups and distinctive schools of thought. What we have done in effect is to construct three models or paradigms which explain many aspects of the course of the Scientific Revolution, but, as we will see, each tradition was related to some aspect of Greek thought, the organic tradition to Aristotle, the magical tradition to neo-Platonism and the mechanist tradition to the atomists and Archimedes.

Hugh Kearney, *Science and change 1500–1700*, World University Library, Weidenfeld & Nicolson, London, 1971, pp. 7–25 (excerpts).

A. Rupert Hall

INTRODUCTION TO 'THE SCIENTIFIC REVOLUTION 1500–1800'

In many of these respects modern science differs markedly from xi
that of a not very remote past. It demands rigorous standards in observing and experimenting. By insisting that it deals only with material entities in nature, it excludes spirits and occult powers from its province. It distinguishes firmly between theories confirmed by multiple evidence, tentative hypotheses and unsupported speculations. It presents, not a possible or even a plausible picture of nature, but one in which all available facts are given their logical, orderly places. These are the most important characteristics of modern science, which it acquired during the period of transition conveniently known as the scientific
revolution, and has since retained. . . xii

Science began soon after the birth of civilization. Man's attempt to win an Empire over Nature (in Francis Bacon's phrase) was much older still; he had already learnt to domesticate animals and plants, to shape inorganic materials like clay and metals to his purposes, and even to mitigate his bodily ailments. We do not know how or why he did these things, for his magic and his reasoning are equally concealed. Only with the second millennium B.C. is it possible to discern, dimly, the beginnings of science in the coalescence of these three elements in man's attitude to Nature—empirical practice, magic and rational thinking.

The same three elements continued to exist in science for many thousand years, until the scientific revolution took place in the sixteenth and seventeenth centuries. Reason, in conjunction with observation and experiment, slowly robbed magic of its power, and was gradually better able to anticipate and absorb the chance discoveries of inventive craftsmen, but complete reliance upon a rational scientific method in man's reaction to his natural environment is very recent. Magic and esoteric mystery—the elements of the irrational—were not firmly disassociated from serious science before the seventeenth century, at which time even greater stress than before was being laid on the usefulness to scientists of the craftsman's practical skills. This in turn was not outgrown until the nineteenth century, when it became clear that in the future sheer empiricism and chance would add little to man's natural knowledge, or to his natural power.

Rational science, then, by whose methods alone the phenomena of nature may be rightly understood, and by whose application alone they may be controlled, is the creation of the seventeenth and eighteenth centuries. Since then dramatic achievements in understanding and power have followed successively. In this sense the period 1500–1800 was one of preparation, that since 1800 one of accomplishment. And it is convenient to conclude this history of the scientific revolution with the early years of the nineteenth

3.3

John Krige

REVOLUTION AND DISCONTINUITY

In what sense, if at all, can the achievements of men like Copernicus, Galileo, Descartes and Newton be said to constitute a scientific *revolution*? The hesitancy which informs this question is a symptom of the caution which many historians and philosophers now feel when discussing the birth of modern science. They no longer have the confidence of a Kant who, in his Preface to the second edition of the *Critique of Pure Reason*, enthusiastically proclaimed that a 'new light flashed upon all students of nature' in the seventeenth century. And they are far less convinced than he was that physics underwent a 'rapid intellectual revolution' at the hands of Galileo and his contemporaries. Studies of innovations made in the fourteenth century, particularly at Oxford and at Paris, suggest that some of the most fundamental concepts and methods of classical science were anticipated by the medieval schoolmen. It appears that they were not 'groping in the dark', unaided by the light of reason, as Kant supposed — for example, some of them believed, like Galileo, that nature could be comprehended by mathematical laws. The view that the (so-called) Scientific Revolution involved a clean break with the past is wearing thin. Many would agree with Crombie that 'the most striking result of recent scholarship (is) the essential continuity of the Western scientific tradition from Greek times to the seventeenth century and, therefore, to our own day'.[1] 15

If Crombie appears to favour a continuist theory of history here, there are hints elsewhere in the same text that suggest that he has not really thought out his position on this matter too carefully. Thus, he also says that during the sixteenth and seventeenth centuries the successful application of mathematics to mechanics 'changed men's whole conception of Nature and brought about the destruction of the whole Aristotelian system of cosmology'.[2] He is not alone in insisting that a profound and far-reaching change occurred at this time. Hall, for example, takes care to distinguish the emergence of classical mechanics from the transition from Newtonian to Einsteinian physics at the turn of this century, when he says 16 that

> Only the broader extrapolations of nineteenth-century science would now be described blankly as 'wrong', though a larger part of its picture of Nature might be described as 'inadequate' or as 'true within certain limits' . . . . The same could not be said of science before 1500, or even, without restriction, of the science of the seventeenth and eighteenth centuries. Its progress in these earlier times was not by accretion, for it was now and again necessary to jettison encumbering endowments from the past. Such science *was* on occasion simply wrong, both in fact and in interpretation. Its

what they had to do

> was not to criticize and to combat certain faulty theories, and to correct or to replace them by better ones. They had to do something quite different. They had to destroy one world and to replace it by another. They had to reshape the framework of our intellect itself, to restate and to reform its concepts, to evolve a new approach to Being, a new concept of knowledge, a new concept of science — and even to replace a pretty natural approach, that of common sense, by another which is not natural at all.[7]

The choice between Duhem's position and Koyré's seems to be quite straightforward. Yet as we have seen eminent historians like Crombie and Butterfield oscillate between the continuist and discontinuist poles without apparently realizing that they are doing so. Some scholars simply adopt a cosy compromise. Drake, for example, suggests that 'Whether we wish to look upon the history of science as continuous or discontinuous throughout the centuries
depends more on philosophical prepossessions than it does on 18
the record of ideas and events. Either approach is potentially fruitful of increased understanding, but not at the expense of the other'.[8]

Wiener and Noland, on the other hand, reassure their readers that 'the intellectual lines of continuity between ancient and modern science are there despite the revolutionary changes that have occurred within the history of science'.[9] This quest for continuity is, it would seem, compulsive for some historians. Dijksterhuis concludes his summary of the 'evolution' of mechanics from Greek to medieval times with the remark that his analysis 'greatly satisfies our desire for the combination of continuity and renewal, a desire which as a rule spontaneously directs our historical inquiries'.[10]

My aim in this chapter is to explore, albeit somewhat tentatively, some of the factors which have produced this rather confused state of affairs. Following Koyré, I shall assume (here, and throughout the book) that there *was* a revolution in science in the seventeenth century and that it involved a profound 'transformation of the human understanding', as Foucault puts it. . .

the conceptual structures of Aristotelian physics and cosmology, on the one hand, and of classical mechanics on the other, are *so* different from one another that there is no effective overlap between them. They are incommensurable systems of thought, one of which was rejected and replaced by the other.

John Krige, *Science, revolution and discontinuity*, Harvester Press, Sussex, 1980, pp. 15-18 (excerpts).

Thomas S. Kuhn

THE STRUCTURE OF SCIENTIFIC REVOLUTIONS

To discover why this issue of paradigm choice can never be 94
unequivocally settled by logic and experiment alone, we must shortly examine the nature of the differences that separate the proponents of a traditional paradigm from their revolutionary successors. . .

Let us, therefore, now take it for granted that the differences 103
between successive paradigms are both necessary and irreconcilable. Can we then say more explicitly what sorts of differences these are? The most apparent type has already been illustrated repeatedly. Successive paradigms tell us different things about the population of the universe and about that population's behavior. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat or of energy. These are the substantive differences between successive paradigms, and they require no further illustration. But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.

The impact of Newton's work upon the normal seventeenth-
century tradition of scientific practice provides a striking exam- 104
ple of these subtler effects of paradigm shift. Before Newton was born the "new science" of the century had at last succeeded in rejecting Aristotelian and scholastic explanations expressed in terms of the essences of material bodies. To say that a stone fell because its "nature" drove it toward the center of the universe had been made to look a mere tautological word-play, something it had not previously been. Henceforth the entire flux of sensory appearances, including color, taste, and even weight, was to be explained in terms of the size, shape, position, and

are other reasons, too, for the incompleteness of logical contact that consistently characterizes paradigm debates. For example, since no paradigm ever solves all the problems it defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: Which problems is it more significant to have solved? Like the issue of competing standards, that question of values can be answered only in terms of criteria that lie outside of normal science altogether, and it is that recourse to external criteria that most obviously makes paradigm debates revolutionary. Something even more fundamental than standards and values is, however, also at stake. I have so far argued only that paradigms are constitutive of science. Now I wish to display a sense in which they are constitutive of nature as well.

X. Revolutions as Changes of World View

Examining the record of past research from the vantage of contemporary historiography, the historian of science may be tempted to exclaim that when paradigms change, the world itself changes with them. Led by a new paradigm, scientists adopt new instruments and look in new places. Even more important, during revolutions scientists see new and different things when looking with familiar instruments in places they have looked before. It is rather as if the professional community had been suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well. Of course, nothing of quite that sort does occur: there is no geographical transplantation; outside the laboratory everyday affairs usually continue as before. Nevertheless, paradigm changes do cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world.

It is as elementary prototypes for these transformations of the scientist's world that the familiar demonstrations of a switch in visual gestalt prove so suggestive. What were ducks in the scientist's world before the revolution are rabbits afterwards. The man who first saw the exterior of the box from above later sees its interior from below. Transformations like these, though usually more gradual and almost always irreversible, are common concomitants of scientific training. Looking at a contour map, the student sees lines on paper, the cartographer a picture of a terrain. Looking at a bubble-chamber photograph, the student sees confused and broken lines, the physicist a record of familiar subnuclear events. Only after a number of such transformations of vision does the student become an inhabitant of the scientist's world, seeing what the scientist sees and responding as the scientist does. The world that the student then enters is not, however, fixed once and for all by the nature of the environment, on the one hand, and of science, on the other. Rather, it is determined jointly by the environment and the par-

disk-size that was at least unusual for stars. Something was awry, and he therefore postponed identification pending further scrutiny. That scrutiny disclosed Uranus' motion among the stars, and Herschel therefore announced that he had seen a new comet! Only several months later, after fruitless attempts to fit the observed motion to a cometary orbit, did Lexell suggest that the orbit was probably planetary. When that suggestion was accepted, there were several fewer stars and one more planet in the world of the professional astronomer. A celestial body that
had been observed off and on for almost a century was seen dif- 116
ferently after 1781 because, like an anomalous playing card, it could no longer be fitted to the perceptual categories (star or comet) provided by the paradigm that had previously prevailed.

• • •

5. *Exemplars, Incommensurability, and Revolutions* 198

What has just been said provides a basis for clarifying one more aspect of the book: my remarks on incommensurability and its consequences for scientists debating the choice between successive theories. In Sections X and XII I have argued that the parties to such debates inevitably see differently certain of the experimental or observational situations to which both have recourse. Since the vocabularies in which they discuss such situations consist, however, predominantly of the same terms, they must be attaching some of those terms to nature differently, and their communication is inevitably only partial. As a result, the superiority of one theory to another is something that cannot be proved in the debate. Instead, I have insisted, each party must try, by persuasion, to convert the other. Only philosophers have seriously misconstrued the intent of these parts of my argument. A number of them, however, have reported that I believe the following: the proponents of incommensurable theories
cannot communicate with each other at all; as a result, in a 199
debate over theory-choice there can be no recourse to *good* reasons; instead theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached. More than any other parts of the book, the passages on which these misconstructions rest have been responsible for charges of irrationality.

Consider first my remarks on proof. The point I have been trying to make is a simple one, long familiar in philosophy of science. Debates over theory-choice cannot be cast in a form that fully resembles logical or mathematical proof. In the latter, premises and rules of inference are stipulated from the start. If there is disagreement about conclusions, the parties to the ensuing debate can retrace their steps one by one, checking each against prior stipulation. At the end of that process one or the other must concede that he has made a mistake, violated a previously accepted rule. After that concession he has no recourse, and his opponent's proof is then compelling. Only if the two dis-

jects within even the altered sets continue to be grouped together, the names of the sets are usually preserved. Nevertheless, the transfer of a subset is ordinarily part of a critical change in the network of interrelations among them. Transferring the
metals from the set of compounds to the set of elements played 201
an essential role in the emergence of a new theory of combustion, of acidity, and of physical and chemical combination. In short order those changes had spread through all of chemistry. Not surprisingly, therefore, when such redistributions occur, two men whose discourse had previously proceeded with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions and generalizations. Those difficulties will not be felt in all areas of even their scientific discourse, but they will arise and will then cluster most densely about the phenomena upon which the choice of theory most centrally depends.

Such problems, though they first become evident in communication, are not merely linguistic, and they cannot be resolved simply by stipulating the definitions of troublesome terms. Because the words about which difficulties cluster have been learned in part from direct application to exemplars, the participants in a communication breakdown cannot say, "I use the word 'element' (or 'mixture,' or 'planet,' or 'unconstrained motion') in ways determined by the following criteria." They cannot, that is, resort to a neutral language which both use in the same way and which is adequate to the statement of both their theories or even of both those theories' empirical consequences. Part of the difference is prior to the application of the languages in which it is nevertheless reflected.

The men who experience such communication breakdowns must, however, have some recourse. The stimuli that impinge upon them are the same. So is their general neural apparatus, however differently programmed. Furthermore, except in a small, if all-important, area of experience even their neural programming must be very nearly the same, for they share a history, except the immediate past. As a result, both their everyday and most of their scientific world and language are shared. Given that much in common, they should be able to find out a great deal about how they differ. The techniques required are not, however, either straightforward, or comfortable, or parts of the scientist's normal arsenal. Scientists rarely recognize them
for quite what they are, and they seldom use them for longer 202
than is required to induce conversion or convince themselves that it will not be obtained.

Briefly put, what the participants in a communication breakdown can do is recognize each other as members of different language communities and then become translators. Taking the differences between their own intra- and inter-group discourse as itself a subject for study, they can first attempt to discover the terms and locutions that, used unproblematically within each community, are nevertheless foci of trouble for

proceeds, furthermore, some members of each community may also begin vicariously to understand how a statement previously opaque could seem an explanation to members of the opposing group. The availability of techniques like these does not, of course, guarantee persuasion. For most people translation is a threatening process, and it is entirely foreign to normal science.
Counter-arguments are, in any case, always available, and no 204
rules prescribe how the balance must be struck. Nevertheless, as argument piles on argument and as challenge after challenge is successfully met, only blind stubbornness can at the end account for continued resistance.

That being the case, a second aspect of translation, long familiar to both historians and linguists, becomes crucially important. To translate a theory or worldview into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign. That transition is not, however, one that an individual may make or refrain from making by deliberation and choice, however good his reasons for wishing to do so. Instead, at some point in the process of learning to translate, he finds that the transition has occurred, that he has slipped into the new language without a decision having been made. Or else, like many of those who first encountered, say, relativity or quantum mechanics in their middle years, he finds himself fully persuaded of the new view but nevertheless unable to internalize it and be at home in the world it helps to shape. Intellectually such a man has made his choice, but the conversion required if it is to be effective eludes him. He may use the new theory nonetheless, but he will do so as a foreigner in a foreign environment, an alternative available to him only because there are natives already there. His work is parasitic on theirs, for he lacks the constellation of mental sets which future members of the community will acquire through education.

The conversion experience that I have likened to a gestalt switch remains, therefore, at the heart of the revolutionary process. Good reasons for choice provide motives for conversion and a climate in which it is more likely to occur. Translation may, in addition, provide points of entry for the neural reprogramming that, however inscrutable at this time, must underlie conversion. But neither good reasons nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientific change.

6. *Revolutions and Relativism* 205

One consequence of the position just outlined has particularly bothered a number of my critics. They find my viewpoint relativistic, particularly as it is developed in the last section of this book. My remarks about translation highlight the reasons for the charge. The proponents of different theories are like the members of different language-culture communities. Recogwith which the theory populates nature and what is "really there."

Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausability of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. Though the temptation to describe that position as relativistic is understandable, the description seems to me wrong. Conversely, if the position be relativism, I cannot see that the relativist loses anything needed to account for the nature and development of the sciences.

Thomas S. Kuhn, *The structure of scientific revolutions*, 2nd edn, University of Chicago Press, Chicago, 1970, pp. 94–116, 198–207 (excerpts).

Stephen Toulmin

THE PROBLEM OF CONCEPTUAL CHANGE

3.5

On this literal-minded interpretation, Kuhn's account has the same relativist implications as Collingwood's theory. The merits of intellectual 'revolutions' cannot be discussed or justified in rational terms—since no common set of procedures for judging this rationality are acceptable, or even intelligible, to both sides in the dispute. So the considerations operative within a revolutionary change must apparently be interpreted as causes or motives, rather than as reasons or justifications. Only after the victorious new paradigm is securely enthroned in acknowledged power can the rule of rationality be restored. As for Collingwood earlier, the 'rational procedures' of a science are secondary authorities, whose writ carries weight thanks to the sovereignty of the primary intellectual powers—whether paradigms or absolute presuppositions. Transfers of sovereign power and authority between one paradigm and its successor thus take place on the very frontiers of rationality. Rather than conforming to established canons or procedures, they establish novel canons of scientific rationality. New frameworks of fundamental theory cannot themselves be arrived at in a 'rational', or 'rule-following' manner. Paradigms are sovereign; they make their own laws. 102

These conclusions will apply to an actual 'paradigm-switch' or conceptual change—we have said—in those cases, and only those cases, which answer to the fully-fledged definition of a 103 'scientific revolution', as involving the adoption of an entirely new world-view. And the first question we must raise is whether any theoretical change within a given scientific discipline has ever in fact produced so radical a discontinuity; or whether this fully-fledged definition does not exaggerate the depth of the conceptual changes actually occurring within the natural sciences. If we press this question, we shall at once be led to the same reservations about the doctrine of intellectual revolutions, regarded as a theory of conceptual change, as we entered earlier against Collingwood's doctrine of absolute presuppositions, regarded as an account of the structure of conceptual systems.

What examples might we pick on as possible illustrations of such total changes in scientific world-view? Two promising candidates are the changeover from pre-Copernican astronomy to the new science of Galileo and Newton, which was the topic of Kuhn's first, historical book, and the more recent changeover from the classical physics of Newton and Maxwell to the relativistic and quantum physics of Einstein, Heisenberg and their successors, on which Kuhn has also done extensive historical research. Yet in neither case does the full revolutionary would, at first, make it hard for them to understand each other's questions—to say nothing of their respective answers. Yet does this imply that the gulf between their theoretical positions was rationally unbridgeable? Were Copernicus and Galileo, Kepler and Newton the authors of a totally new and all-embracing paradigm, whose novel world-picture snapped all intellectual connections with the physics of earlier times? Once again, this is a caricature of the actual facts.

Over this point, the writings of T. S. Kuhn the historian are the best commentary on the theories of T. S. Kuhn the philosophical sociologist. As his historical analysis makes clear, the so-called 'Copernican Revolution' took a century and a half to complete, and was argued out every step of the way. The world-view that emerged at the end of this debate had—it is true—little in common with earlier pre-Copernican conceptions. Yet, however radical the resulting change in physical and astronomical *ideas and theories*, it was the outcome of a continuing rational discussion and it implied no comparable break in the intellectual *methods* of physics and astronomy. If the men of the sixteenth and seventeenth centuries changed their minds about the structure of the planetary system, they were not forced, motivated, or cajoled into doing so; they were given reasons to do so. In a word, they did not have to be converted to Copernican astronomy; the arguments were there to convince them.

Taken at its face value, then, the fully-fledged definition of an 'intellectual revolution' in science is plagued by paradoxes of the same kind as afflicted Collingwood's theory of absolute presuppositions. If we are to make the theory of paradigms and revolutions fit the actual historical evidence, accordingly, we can do so only on one condition. We must face the fact that paradigm-switches are never as complete as the fully-fledged definition implies; that rival paradigms never really amount to entire alternative world-views; and that intellectual discontinuities on the theoretical level of science conceal underlying continuities at a deeper, methodological level. 106

Stephen Toulmin, *Human understanding*, vol. 1, Clarendon Press, Oxford, 1972, pp. 102–106 (footnotes deleted).

John C. Greene

THE KUHNIAN PARADIGM AND THE DARWINIAN REVOLUTION IN NATURAL HISTORY

We come now to Darwin and the revolution in natural history as- 19
sociated with his name and achievements. From Kuhn's hypothesis
we should expect this revolution to be non-cumulative in character
and to involve the substitution of a new paradigm of natural history
incommensurable with the static paradigm that had reigned before
the revolution. The first question, then, is: Was the Darwinian revo-
lution non-cumulative in character? That is, did it break sharply with
the concepts, methods, and modes of thought that had prevailed 20
before 1859?

From what has already been said it should be apparent that this question does not admit of a simple Yes or No answer. If we compare Darwin's ideas and methods with those that had prevailed in the main tradition of systematic natural history, namely, the tradition of Linnaeus, Jussieu, Candolle, Cuvier, Owen, and Agassiz, we discover a profound break with the past, though *not* one generated in response to internal difficulties in the tradition that was overthrown.

If, on the other hand, we compare Darwin's concepts and methods with those of Buffon, Erasmus Darwin, Lamarck, Étienne Geoffroy St. Hilaire, and Charles Lyell, we begin to have doubts about the non-cumulative character of the Darwinian revolution. We discover that some aspects of Darwin's thought and practice were more original than others.

Darwin himself made no claim to have invented the idea of organic evolution. He was too well acquainted with the writings of Lamarck, Geoffroy St. Hilaire, and his own grandfather, Erasmus Darwin, all of whom he had read or re-read on his return from the voyage of the *Beagle*, to make any such claim. He claimed only to have discovered "the means of modification and co-adaptation" in nature and thereby to have transformed a speculative idea of descent with modification into a workable theory of the origin of species. To this he might have added that he had done more than merely hit upon the idea of natural selection as the means of modification and co-adaptation. More important, he had deduced the consequences of his hypothesis and endeavored to show by observation and experiment that they actually obtained in nature. This combination of inductive and deductive methods had long prevailed in the physical sciences, but Darwin was the first to apply it systematically in natural history. His methods were as revolutionary as his theory.

Thus, one's judgment as to the cumulative or non-cumulative character of the Darwinian revolution depends largely on whether one stresses the general concept of descent with modification or the particular theory of natural selection as the means of organic modification in nature. The general concept had a history reaching back at least to Buffon. Indeed, Thomas Henry Huxley was inclined to credit Descartes with insight into what Huxley deemed "the fundahairsplitting racial taxonomy. As for Darwin's ideas on the subject of heredity and variation, they did give rise to a school of English geneticists led by Galton, Pearson, and Weldon. But, far from being generally accepted, these ideas were attacked strenuously by Weismann and others and were eventually overthrown by the rediscovery of Mendel's laws and the development of cytology. In fact, it could be argued that nothing approaching a "Darwinian" paradigm became established until the 1930's, and even that paradigm was Darwinian only in a very loose sense.

We conclude as follows:

1. Through the work of Ray, Tournefort, and Linnaeus natural history acquired a conceptual framework that dominated the study of natural history until Darwin published his *Origin of Species*, after which systematics was gradually reshaped and relocated within the broader framework of evolutionary biology.

2. Challenges to the dominance of the Linnaean framework in natural history arose both within and outside of that framework.

3. The challenges arising within the static view of nature and natural history failed to precipitate a search for new premises; they were either ignored or evaded by compromises such as the theory of successive creations.

4. Rival concepts, such as those propounded by Buffon, Lamarck, and the transcendental anatomists, arose from time to time, but not in response to anomalies or crises within the dominant view. These counter-concepts exerted a significant influence both on the static view of nature and on the developments that were to eventuate in its overthrow.

5. The evolutionary alternative to the static outlook developed chiefly outside the Linnaean framework in the form of a search for a science of nature that would derive the phenomena of nature from the operations of a law-bound system of matter in motion. The earliest and most powerful challenge to the static view of nature was the challenge implied in geological uniformitarianism. It was this postulate, rather than particular scientific discoveries, that drove Lamarck to an evolutionary position and led Lyell to envisage the piecemeal extinction of species through the struggle for existence.

6. The eventual emergence of the theory of natural selection in Britain seems to have owed a great deal to the influence of the competitive ethos that pervaded British political economy and British *mores* generally.

7. The Darwinian revolution displayed elements both of continuity and discontinuity with the past. It overthrew the static view of nature and natural history, but failed to establish a clear-cut paradigm in its place.

8. The Kuhnian paradigm of paradigms can be made to fit certain aspects of the development of natural history from Ray to Darwin, but its adequacy as a conceptual model for that development seems doubtful. The use of Kuhnian terminology in this essay should not be interpreted as implying belief in its general utility for the historiography of science. At the same time, it should be remembered that an inadequate hypothesis is better than none at all. Those who question the validity of Kuhn's model should feel themselves challenged to provide alternative interpretations of the genesis of revolutions in science. The present essay is intended less as a critique of

3.7

Barry Barnes

T. S. KUHN AND SOCIAL SCIENCE

The response to . . . crisis. . . typically involves a change in 54
the character of research. Speculation becomes more acceptable. Novel and radically deviant procedures and interpretations are tried. Paradigms, and the activities and judgements based upon them, are called into question. They are not, however, discarded: no scientific community ever simply throws aside its tools and abandons research. Only when a new paradigm is agreed upon, as an adequate response to current difficulties and an acceptable foundation for future work, only then can the existing basis for research be set on one side. At this point a large scale re-ordering of practice and perception occurs, reflecting the requirements exemplified in the new paradigm; and the conceptual fabric undergoes an analogous reconstruction. The scene is now set for a new sequence of normal science to develop: a *scientific revolution* has occurred.

Among Kuhn's examples of scientific revolutions are those which occurred, in chemistry, with the acceptance of the oxygen theory of combustion and, later, of Daltonian atomism, in physics, at the transition from Aristotelian to classical mechanics, and then again with the advent of quantum mechanics and relativistic mechanics, and in astronomy, with the acceptance of the Copernican system. He also cites 'lesser' revolutions such as those associated with the discoveries of *X*-radiation and the planet Uranus (cf. Kuhn, 1970, chs 8–10).

In general, Kuhn expects to find normal research interrupted by revolutionary episodes. At times of revolution scientists make the drastic responses necessary to cope with accumulating anomaly, but quite impracticable in the context of the normal science which produced it. Revolutions are to be expected because they perform an essential function; they are vital to the evolution of scientific culture, just as they are vital to the evolution of political institutions (cf. Kuhn, 1970, ch. 9). Here, as everywhere, Kuhn writes as a functionalist; and his account will only be acceptable exactly as it stands to those who see no difficulties in the functionalist point of view.

Kuhn goes out of his way to emphasise that revolutions constitute
discontinuities in research and the growth of knowledge. By recon- 55
structing it around a new achievement scientists effect a truly radical transformation of their culture, at the verbal and symbolic level as well as at the level of procedure and perception. Learned similarity relations are modified, with concrete instances being grouped into new clusters: before Dalton the alloys of the metals were compounds; after his work they were mixtures; in mechanics before Newton, terrestrial and celestial objects were fundamentally different; after his work they were mechanically identical. The conceptual fabric is also reconstructed so that concepts figure in different laws and generalisations; this happened with the term 'compound' as a result of Dalton's work, and to 'mass' as a result of

recalcitrant anomalies. They are carried through by argument and appeal to nature — even if the power of the arguments and appeals is more social-psychological than logical in the accepted sense. The question here is whether Kuhn's account is accurate and sufficient, something which only the collective wisdom of historians will be able to decide. Fortunately, they are well placed to do so over the long term, since Kuhn cites many instances of revolutions which can be considered empirically. For what it is worth, my own reading of current historical research is that it is revealing what are at least significant insufficiencies in Kuhn's account. . . though no doubt his characterisation of revolutionary episodes will continue to retain some utility in descriptive historical writing.

Finally, there is the matter of the necessity of scientific revolu- 57
tions. Kuhn, as we have seen, imbues the alternation of normal and revolutionary science with functional necessity (1970, ch. 9). Commentators accordingly describe his work as a 'theory of scientific development'; and they accept the implication that every episode in the history of an established science must be either 'normal' or 'revolutionary'. This, however, even if it is a legitimate interpretation, is one which demands too much of Kuhn's account. One of the merits of his own properly detailed historical treatment of the Copernican revolution is its demonstration that the changes involved were not necessitated by accumulating anomalies and a sense of crisis: this does not have to be inferred, since it is spelled out in Kuhn's own commentary (Kuhn, 1957, cf. especially ch. 4). That scientific revolutions are functionally necessary is a claim difficult to reconcile with Kuhn's own work, let alone with the full richness of the historical record. It is a claim, moreover, which is offensive to the imagination: it is easy to visualise different kinds of transition from one paradigm and conceptual fabric to another, or even processes which lead to alternative paradigms coexisting alongside each other; nor have we any arguments to suggest that such transitions and processes could not actually occur

It is because of its comparative lack of theoretical interest that Kuhn's account of revolutions is less valuable than his discussion of normal science. The latter is of *fundamental* theoretical importance because it describes many general characteristics of cognition and culture which it is difficult to imagine could be otherwise. The former does not do this, and accordingly can be at best no more than an empirical description of some selected episodes in the history of science.

Those parts of *Structure* which describe scientific revolutions are, however, of interest for another reason. They serve as the occasion for an extended discussion of problems of evaluation in science. The difficulties encountered in the comparative appraisal of alternative paradigms and conceptual fabrics are carefully analysed and illustrated, so that the grounds for a relativistic conception of knowledge emerge with striking clarity. . .

Evaluation and normal science

Kuhn's account of normal science is a fascinating and insightful 83
description of a conventional activity. Yet he himself occasionally encourages us to forget this. His discussion of the insufficiency of logic and experience at a time of paradigm change can create the impression that at other times they *are* sufficient. He even occasion-

Consider an example identified as normal science by Kuhn himself, and already briefly mentioned in the previous chapter. In section 3.2 the problem-solution of the point-mass pendulum was described, as was the way it was extended to compute the speed of a swinging or rolling solid mass at the bottom of its descent. Later, Bernoulli's calculation of the speed of efflux of a liquid from an orifice in a vessel became a further accepted achievement of normal science, by extension of these earlier problem-solutions. This evaluation of Bernoulli's work, however, depended upon it being accepted that liquids could manifest speed in 'the same' way as solids. However this acceptance developed, whether from familiar cases of the speeds of solids to problems involving liquids, or vice versa, it involved no scientific revolution. Yet such a development cannot at all be understood as predetermined by the very meaning of the term 'speed', or as a deductive inference from a definition of 'speed'. Assuming that consideration of liquids was historically the later, conceptions of speed in respect of liquids must have been a development by analogy with understood instances and conceptions of speed in the realm of solids. And at the point when instances of the speed of liquids were first used to teach the term 'speed', that term became systematically linked to nature in a recognisably new way; its 'meaning' was transformed. 85

Note that in this case, when the 'speed' of liquids was first talked of, it would have been open to the community involved to reject the implied analogy, to insist that it involved incorrect usage, and to maintain that 'speed' was a term with reference only to solids. Alternatively, what in a liquid was to count as 'the same' as the speed of a solid could have been decided differently. It is a contingent sequence of such developments of usage which has constructed the current concept of 'speed', which applies in far from self-evident ways to the motion of rigid objects, of liquids, of waves 'travelling' through liquids, of springs vibrating back and forth, of wheels, and so on. 'Speed' is a similarity relation, an agreed cluster of non-identical instances or applications. ./ ; and the applications are built up in an unpredictable historical development involving continuing modification of meaning.

Meaning change does not occur simply at times of revolution. It occurs all the time, producing formal problems of evaluation all the time. If there are such episodes as scientific revolutions, they are not periods when a special kind of cultural change occurs which generates unusual formal problems of evaluation. Any particular 86 change which occurs in a revolutionary episode can occur equally in a period of normal science, whether it be meaning change, technical change, the invention of new problem-solutions, or the emergence of new standards of judgement. If there is anything worth calling a revolutionary episode, it is a period when a large number of cultural changes all occur at once, or when scientists find themselves obliged to opt for one or other alternative cluster of practices and beliefs. The concept of scientific revolution is suited to historical narrative. For the narrow sociological end of identifying basic processes of cultural change it is irrelevant.

Once more we are led to the conclusion that Kuhn's insistence upon the 'necessity' of scientific revolutions is misplaced (cf. Kuhn, 1970, ch. 9). There is nothing in normal science to prohibit any particular kind of development, in technique, in problem-solutions or in verbal culture. Hence there is nothing to compel a leap out of

3.8

Paul Feyerabend

AGAINST METHOD

I shall conclude this chapter by repeating its results in the form of 271 theses. The theses may be regarded as summaries of anthropological material relevant for the elucidation . . .

. . of meaning-terms and of the notion of incommensurability.

The *first thesis* is that *there are* frameworks of thought (action, perception) which are incommensurable. . .

It is, of course, always possible to replace a framework that looks strange and incomprehensible when approached from the point of view of western science by another framework that looks like some piece of western common sense (with, or without, science), or like a clumsy anticipation of such common sense, or else like a fantastic fairy-tale. Most early anthropologists distorted the object of their study in this way and so could easily assume that the English language (or the German, Latin or Greek language) was rich enough to present and comprehend the most outlandish myth. Early dictionaries express this belief in a very direct manner, for we have here simple definitions of all 'primitive' terms and simple explanations of all 'primitive' notions. In the meantime it has become clear that dictionaries and translations are 272 lousy ways of introducing the concepts of a language that is not closely related to our own, or of ideas which do not fit into western ways of thinking. Such languages have to be learned *from scratch*, as a child learns words, concepts, appearances ('appearances' because things and faces are not just 'given', they are 'read' in certain ways – different ways being prominent in different ideologies). We must not demand that the process of learning be structured in accordance with the categories, laws and perceptions we are already familiar with. It is precisely such an 'unprejudiced' way of learning that a *field study* is supposed to achieve. Returning from the field study to his own conceptions and his own language, such as English, an anthropologist often 273 realizes that a direct translation has become impossible and that his views, and the views of the culture to which he belongs, are incommensurable with the 'primitive' ideas he has just begun to understand (or there may be overlap in some parts and incommensurability in others). Naturally, he wants to give an account of these ideas in English, but he will be able to do so only if he is prepared to use familiar terms in strange and novel ways. He may even have to build an entirely new language-game out of English words, and he will be able to start his explanations only when this language-game has become fairly complex. Now we know that almost every language contains within itself the means of restructuring large parts of its conceptual apparatus. Without this, popular science, science fiction, fairy-tales, tales of the supernatural, and science itself would be impossible. There is, therefore, a good sense physiology, as well as by the mechanistic cosmology of Descartes.

Changes of ontology such as those just described are often accompanied by *conceptual changes.*

The discovery that certain entities do not exist may prompt the scientist to re-describe the events, processes, observations which were thought to be manifestations of them and which were therefore described in terms assuming their existence. (Or rather it may prompt him to introduce new *concepts*, since the older *words* will remain in use for a considerable time.) This applies especially to those 'discoveries' which suspend universal principles. The 'discovery' of an 'underlying substance' and of a 'spontaneous I' is of this kind, as we have seen.

An interesting development occurs when the faulty ontology is *comprehensive*, that is, when its elements are thought to be present in every process in a certain domain. In *this* case, *every* description inside the domain must be changed and must be replaced by a different statement (or by no statement at all). Classical physics is a case in point. It has developed a comprehensive terminology for describing some very fundamental properties of physical objects, such as shapes, masses, volumes, time intervals and so on. The conceptual system connected with this terminology assumes, at least in one of its numerous interpretations, that the properties *inhere* in objects and change only as the result of a direct physical interference. This is one of the 'universal principles' of classical physics. The theory of relativity implies, at least in the interpretation that was accepted by Einstein and Bohr, that inherent properties of the kind enumerated do not exist, that shapes, masses, time intervals are relations between physical objects and co-ordinate systems which may change, *without any physical interference*, when we replace one co-ordinate system by another. The theory of relativity also provides new principles for constituting mechanical facts. The new conceptual system that arises in this way does not just *deny* the existence of classical states of affairs, it does not even permit us to *formulate statements* expressing such states of affairs. It does not, and cannot, share a single statement with its predecessor – assuming all the time that we do not use the theories as classificatory schemes for the ordering of neutral facts. If we interpret both theories in a realistic manner, then the 'formal conditions for a suitable successor of a refuted theory', which were stated in Chapter 15 (it has to repeat the successful consequences of the older theory, deny its false consequences, and make additional predictions), cannot be satisfied and the positivistic scheme of progress with its 'Popperian spectacles', breaks down. Even Lakatos' liberalized version cannot survive this result; for it, too, assumes that content-classes of different theories can be compared, i.e. that a relation of inclusion, exclusion or overlap can be established between them. It is no use trying to connect classical statements with relativistic statements by an *empirical hypothesis*. A hypothesis of this kind would be as laughable as the statement 'whenever there is possession by a demon there is also discharge in the brain', which establishes a connection between terms of a possession theory of epilepsy and more recent 'scientific' terms. For we clearly do not want to perpetuate the older devilish terminology, and take it seriously, just in order to guarantee comparability of content-classes. But in the case of relativity vs. classical mechanics, a hypothesis of this kind *cannot even* theory depend on its postulates (and associated grammatical rules) *and also* on initial conditions, while the meaning of the 'primitive' notions depends on the postulates (and associated grammatical rules) *only*. In those rare cases, however, where a theory entails assertions about possible initial conditions, we can refute it with the help of *self-inconsistent observation reports* such as: 'object *A* does not move on a geodesic', which, if analysed in accordance with the Einstein–Infeld–Hoffmann account, reads 'singularity α which moves on a geodesic does not move on a geodesic'.

The *second objection* criticizes an interpretation of science that seems to be necessary for incommensurability to come about. I have already pointed out that the question 'are two particular comprehensive theories, such as classical mechanics and the special theory of relativity, incommensurable?' is not a complete question. Theories can be interpreted in 279
different ways. They will be commensurable in some interpretations, incommensurable in others. Instrumentalism, for example, makes commensurable all those theories which are related to the same observation language and are interpreted on its basis. A realist, on the other hand, wants to give a unified account, both of observable and of unobservable matters, and he will use the most abstract terms of whatever theory he is contemplating for that purpose. He will use such terms in order either to *give* meaning to observation sentences or else to *replace* their customary interpretation. (For example, he will use the ideas of the special theory of relativity in order to replace the customary classical interpretation of everyday statements about shapes, temporal sequences, and so on.) Against this, it is pointed out by almost all empiricists that theoretical terms receive their interpretation from being connected with a pre-existing observation language, or with another theory that has already been connected with such a language. Thus Carnap asserts, in a passage I have already quoted,[140] that there is 'no independent interpretation for L_T [the language in terms of which a certain theory, or a certain world view, is formulated]. The system *T* [the axioms of the theory and the rules of derivation] is in itself an uninterpreted postulate system. [Its] terms . . . obtain only an indirect and incomplete interpretation by the fact that some of them are connected by the [correspondence] rules *C* with observation terms. . . .' Now, if theoretical terms have no 'independent interpretation', then they cannot be used for correcting the interpretation of observation statements which is the one and only source of their meaning. It follows that realism, as described here, is an impossible doctrine and that incommensurability cannot arise as long as we keep within the confines of 'sound' (i.e. empiristic) scientific method.

The guiding idea behind this very popular objection is that new and 280
abstract languages cannot be introduced in a direct way, but must first be connected with an already existing, and presumably stable, observation idiom.

This guiding idea is refuted at once by noting the way in which children learn to speak – they certainly do not start from an innate observation language – and the way in which anthropologists and linguists learn the unknown language of a newly-discovered tribe.

mass of elementary particles, the transverse Doppler effect, are said to refute classical mechanics and to confirm relativity. The answer to this problem is not difficult either. Adopting the point of view of relativity we find that the experiments, *which of course will now be described in relativistic terms*, using the relativistic notions of length, duration, mass, speed, and so on, are *relevant* to the theory, and we also find that they *support* the theory. Adopting classical mechanics (with, or without, an ether) we again find that the experiments *which are now described in the very different terms of classical physics* (i.e. roughly in the manner in which Lorentz described them) are relevant, but we also find that they *undermine* (the conjunction of electrodynamics and) classical mechanics. Why should it be necessary to possess terminology that allows us to say that it is the same experiment which confirms one theory and refutes the other? But did we not ourselves use such terminology? Well, for one thing it should be easy, though somewhat laborious to express what was just said *without* assuming identity. Secondly, the identification is, of course, not contrary to my thesis, for we are now not *using* the terms of either relativity or of classical physics, as is done in a test, but are *referring* 283
to them and their relation to the physical world. The language in which *this* discourse is carried out can be classical, or relativistic, or Voodoo. It is no good insisting that scientists act as if the situation were much less complicated. If they act that way, then they are either instrumentalists (see above) or mistaken: many scientists are nowadays interested in *formulae*, while I am discussing *interpretations*. It is also possible that being well acquainted with both theories they change back and forth between them with such speed that they seem to remain within a single domain of discourse.

(This last remark, incidentally, takes care also of the objection that 'the transition from Newton's theory of gravity to Einstein's cannot be an irrational leap' because Newton's theory 'follows from Einstein's theory' as an excellent approximation. Good thinkers can leap rather quickly and continuity of formal relations does not entail continuity of interpretations, as anyone familiar with the notorious 'derivation' of the law of gravitation from Kepler's laws is by now bound to know.)

It is also said that by admitting incommensurability into science we can no longer decide whether a new view *explains* what it is supposed to explain, or whether it does not wander off into different fields. For example, we would not know whether a newly invented physical theory is still dealing with problems of space and time, or whether its author has not by mistake made a biological assertion. But there is no need to possess such knowledge. For once the fact of incommensurability has been admitted, the question which underlies the objection does not arise (conceptual progress often makes it impossible to ask certain questions and to explain certain things; thus, we can no longer ask for the absolute velocity of an object, at least as long as we take relativity seriously). Is this a serious loss for science? Not at all! Progress was made by the very same 'wandering off into different fields' whose undecidability now so greatly exercises the critic: Aristotle saw the world as a super-*organism*, as a *biological* entity, while one essential element of the new science of

4

GALILEO AND THE CATHOLIC CHURCH

4.1

John William Draper

HISTORY OF THE CONFLICT BETWEEN RELIGION AND SCIENCE

The antagonism we thus witness between Religion vi
and Science is the continuation of a struggle that commenced when Christianity began to attain political power. A divine revelation must necessarily be intolerant of contradiction; it must repudiate all improvement in itself, and view with disdain that arising from the progressive intellectual development of man. But our opinions on every subject are continually liable to modification, from the irresistible advance of human knowledge.

Can we exaggerate the importance of a contention in which every thoughtful person must take part whether he will or not? In a matter so solemn as that of religion, all men, whose temporal interests are not involved in existing institutions, earnestly desire to find the truth. They seek information as to the subjects in dispute, and as to the conduct of the disputants.

The history of Science is not a mere record of isolated discoveries; it is a narrative of the conflict of two contending powers, the expansive force of the human intellect on one side, and the compression arising from traditionary faith and human interests on the other.

No one has hitherto treated the subject from this
point of view. Yet from this point it presents itself vii
to us as a living issue—in fact, as the most important of all living issues.

A few years ago, it was the politic and therefore the proper course to abstain from all allusion to this controversy, and to keep it as far as possible in the background. The tranquillity of society depends so much on the stability of its religious convictions, that no one can be justified in wantonly disturbing them.

. . .

In thus treating the subject it has not been necessary x
to pay much regard to more moderate or intermediate opinions, for, though they may be intrinsically of great value, in conflicts of this kind it is not with the moderates but with the extremists that the impartial reader is mainly concerned. Their movements determine the issue.

stars, but a legend related that one of them had mysteriously disappeared. On turning his telescope toward them, Galileo found that he could easily count not fewer than forty. In whatever direction he looked, he discovered stars that were totally invisible to the naked eye.

On the night of January 7, 1610, he perceived three small stars in a straight line, adjacent to the planet Jupiter, and, a few evenings later, a fourth. He found that these were revolving in orbits round the body of the planet, and, with transport, recognized that they
presented a miniature representation of the Copernican 170
system.

The announcement of these wonders at once attracted universal attention. The spiritual authorities were not slow to detect their tendency, as endangering the doctrine that the universe was made for man. In the creation of myriads of stars, hitherto invisible, there must surely have been some other motive than that of illuminating the nights for him.

It had been objected to the Copernican theory that, if the planets Mercury and Venus move round the sun in orbits interior to that of the earth, they ought to show phases like those of the moon; and that in the case of Venus, which is so brilliant and conspicuous, these phases should be very obvious. Copernicus himself had admitted the force of the objection, and had vainly tried to find an explanation. Galileo, on turning his telescope to the planet, discovered that the expected phases actually exist; now she was a crescent, then half-moon, then gibbous, then full. Previously to Copernicus, it was supposed that the planets shine by their own light, but the phases of Venus and Mars proved that their light is reflected. The Aristotelian notion, that celestial differ from terrestrial bodies in being incorruptible, received a rude shock from the discoveries of Galileo, that there are mountains and valleys in the moon like those of the earth, that the sun is not perfect, but has spots on his face, and that he turns on his axis instead of being in a state of majestic rest. The apparition of new stars had already thrown serious doubts on this theory of incorruptibility.

These and many other beautiful telescopic discoveries tended to the establishment of the truth of the Copernican theory, and gave unbounded alarm to the
Church. By the low and ignorant ecclesiastics they 171
were denounced as deceptions or frauds. Some affirmed that the telescope might be relied on well enough for terrestrial objects, but with the heavenly bodies it was altogether a different affair. Others declared that its invention was a mere application of Aristotle's remark that stars could be seen in the daytime from the bottom of a deep well. Galileo was accused of imposture, heresy, blasphemy, atheism. With a view of defending himself, he addressed a letter to the Abbe Castelli,

Giorgio de Santillana

THE CRIME OF GALILEO

THIS work is not the result of a plan aforethought. As I tried to clear up the astonishingly complex background of Galileo's *Dialogue on the Great World Systems,*[1] I was drawn to the drama which played a decisive part in that fateful event of modern history, the secularization of thought. It seemed strange to me that, after so much research and controversy, the story of events as I found it should make so little sense. As I worked, it became clear that a large area of the puzzle had remained oddly disassembled to the present day, by what looks like an inexplicable tacit agreement between the warring factions.

Galileo did not come to grief as "the scientist" facing a religious credo. He was far from standing in the role of a technician of science; had he done so, he would have escaped all trouble. Everyone knows that his discoveries went unchallenged. Those of Descartes did too. And so did Descartes himself. But then, as he admitted, he "went forth under a mask," whereas Galileo is the man without a mask. Both his friends and his enemies saw in him a unique type of creative personality, whose essential achievement might very well be conceived to stand or fall with him. He was a classic type of humanist, trying to bring his culture to the awareness of the new scientific ideas, and among the forces that he found aligned against him religious fundamentalism was by no means the strongest.

It is difficult to see the actual shape of the conflict in these matters so long as we remain under the spell of a misunderstanding tacitly accepted by both sides: the idea of the scientist as a bold "freethinker" and "progressive" facing the static resistance of conservatism. This may well be the aspect on the level of personalities, for [viii]
it is usually the scientist who shows the freer and more speculative mind in contrast with prejudiced opponents. But the core of the thing is different: there the scientist appears more often than not as the conservative overtaken by fast-moving social forces. He usually has the Law and the Prophets on his side.

This ought to become immediately clear if we think of contemporary events. The tragedy of the geneticists in Russia, with its lamentable apologies and recantations, is a faithful rehearsal of the Galileo story; yet we could not accuse the Soviet government of clinging to ancient superstitions or of underestimating the pressing need for science and technology. And lest our straining for beams make us

1. Galileo Galilei, *Dialogue on the Great World Systems.* The Salusbury translation. Revised, annotated, and with an Introduction by Giorgio de Santillana. Chicago: University of Chicago Press, 1953.

more turn racked up on the old screw. By subjecting the scientist as a cultural being to the administrative suspicion that usually attaches to questionable adventurers in international traffics, we have simply brought one step further the process of secularization of thought.

So much is sure, that in the seventeenth-century episode the seeming paradox comes out with full force: within the specific frame of Western Christendom the actual conflict reveals Galileo, like all free men, seeking a support in established custom, credit, and tradition, while Urban VIII, like all organizers of power, becomes the unwitting tool of the streamlined, the "efficient," and the new.

• • •

The whole drama turns out to be in the nature of a surprise encounter [xi] for both sides. Both the authorities and the scientist had the mutual impression of being ambushed, and in neither case was it true. The ambush, in so far as there was one, had been carefully laid by third parties, who carefully exploited the critical situation of the times. But Galileo never felt himself in the figure of an innovator and a rebel. As the central figure of accepted science, as the acknowledged leader of his culture in thought and expression, not last as a perfectly orthodox representative of metaphysical Christianity, he could not but stand his ground, in growing bewilderment, until administrative violence established a quietus, leaving everybody, including the authorities themselves, in a state of utter confusion.

The confusion goes on unremittingly even today, for the Galileo affair is far from dead, and every decade brings a new "line" and new suggestions meant to explain it away, just as it brings a repetition of the ancient rationalist war whoops. The side that stands for the authorities neither is, nor by any means has been, all Catholic. One of the most irresponsible and widely used accounts came from a Protestant publicist of the eighteenth century, Mallet du Pan, and a popular prejudiced version from the pen of another Protestant, Sir David Brewster. Some of the silliest accusations against Galileo have been accredited by the antireligious French [xii] Encyclopedists. On the other hand, some of the more honest efforts to restore the facts are due to professedly Catholic historians like L'Epinois and Reusch.

Since names have been mentioned, I should add, to honor them, those of scholars who, belonging to neither camp, labored to attain an impartial view of the situation: mainly Emil Wohlwill, Th. H. Martin, Karl von Gebler, and Antonio Favaro. Most of the literature through which one has to wade deserves no mention at all. It ranges from average casual incompetence to prevarication and plain filth. Let it go back to whence it came. There is no common measure between the policy problems of long ago—the motives, the hesitation, the eventual refusal, of men who felt intrusted with the fate of millions of people who pray—and the gratuitous distortions that later and self-appointed apologists scattered abroad in their name. The long-drawn-out polemic is not strictly, I hope to have made it clear, one between the confessional and the anticonfessional faction. It has been made to look like that; in reality it is a confused free-for-all in which prejudice, inveterate rancor, and all sorts of special and corporate interests have been the prime movers. Those who dragged and keep dragging the Church into it are no candid souls.

why the only material submitted is a third-hand denunciation by an informer. The incipient scandal had to be dealt with on its own level.

As it happened, the scandal had been created by the informer himself. Deprived of guidance, the police apparatus had become [142]
the tool of its own *agents provocateurs.*

This fatal short circuit between the judiciary and the executive seems to be a constant feature of the streamlined state—or of the state which feels itself compelled to go pragmatic under the stress of emergency. The case is not unknown in our time, and even in free countries, of a politician who serves at one and the same time as disturber of the peace, prosecuting attorney, judge, jury, and detective agency. The serious trouble begins when the higher authorities find themselves in his tow.

In the Rome of 1616 the legislative decisions had come from the Council of Trent: "Petulant minds must be restrained from interpreting Scripture against the authority of tradition in matters that pertain to faith and morals." This was aimed essentially at the fundamentalist reformers who were forever protesting the word of Scripture against the Roman authorities. The book of Copernicus had come out by then, and yet no specific clause is directed against that kind of interpretation. (It must be admitted that the prelates of the Council, lawyers, literati, preachers, and executives all, who had found it difficult even to follow the arguments of their own Scholastic consultants, had never thought that "mathematics" might give them trouble.)

Thus the judiciary might well have felt some doubts. It was a "conciliar affair," as Descartes was to write with cold Gallican sense, to reconsider the whole issue. In his anomalous position as chief theologian as well as chief executive, Bellarmine made up his mind that it was a mere matter of "petulance." He had the question framed, on behalf of the General Congregation, leaving out the main issue. This question he submitted on February 19 to the Qualifiers, and he received in answer, on February 24, the echo of his own words. On the twenty-fifth, as chief executive, he drew up the decree of the General Congregation, had it passed on to the Congregation of the Index, and intrusted its execution to the Commissary of the Inquisition.

The historic responsibility, then, falls on Bellarmine alone. He was a great Jesuit, tempered in thought, dedicated mind and soul to the welfare of the Church. If his intellect had been able to grasp the issue, there is no doubt that he would have put it on the agenda
of a future Council, and the new science would have had a chance [143]
to enter the circle of orthodoxy. All that was really needed in the meantime was a damper on theological excursions, and that would have been the part of discretion. But fear and suspicion were much closer to the heart of the matter, and, besides, Bellarmine did not even like to think of the next Council. He was of the anticonciliar persuasion. He felt that all had been settled and that what was needed henceforth was administrative resource. We are led back, then, to the incredible passiveness of Bellarmine's brothers in the Order, the mathematicians of the Collegio Romano, who apparently took their vow of obedience *perinde ac si cadaver* to dispense them

he was allowed to change his mind. Well, of course, this is a caricature. Of course, the Church accepted perfectly well that he should have been a "Galileist"; in fact, she was herself persuaded by his reasons. She indorsed, in other words—as she had always indorsed —the idea that a man is entitled to entertain opinions that have been declared false and even to prove them right, so long as it is with the proper external submission. But that was exactly Galileo's [232]
position in his letters to Castelli and to Dini, in his *Letter to the Grand Duchess*, and in his behavior in 1632. He himself had inherited it from the doctors of the thirteenth century who had opposed the decision denying the existence of the antipodes. The legal situation never had changed by one jot.

• • •

IV [233]

Thus, when Bellarmine told Galileo that he must give up the opinion, he was not expecting an "absolute assent" but only obedience. "Galileo acquiesced and promised to obey." He pledged, in other words, not to commit his personal affirmation. He was not forbidden to entertain it in his mind as "mathematical" or "probable" or to discuss it quietly with his peers. With time that brings all things, respectful pressure of educated minds might cause a change which would eventually register in official decisions. (It actually did in 1757.) The Jesuit mathematicians themselves, if left far in the background, were willing to be parties to the invisible conspiracy. It was Father Grienberger himself who stated that, if Galileo had not incurred the displeasure of the Company, he could have gone on writing freely about the motion of the Earth to the end of his days. Grienberger's judgment can hardly be suspected of heresy.

Galileo's crime lay in having perceived that change in the "new things" of science could not be so slow as expected. Catholicity did not have world enough and time to make up its mind at leisure, as was the case concerning papal infallibility. He saw "prematurely" (a term which in our fast-shifting times has forced its way even into police language) what ordinary minds like the Vatican astronomers could realize and communicate only a century too late. But his formal position was as correct as theirs. He had with great care established his intention as strictly pious and submissive and [234]
had surrounded himself with the legal guaranties required. It was his bad luck and nothing else to run into a coalition of forces which decided that he must be liquidated.

Giorgio de Santillana, *The crime of Galileo*, University of Chicago Press, Chicago, 1955, pp vii-xiii, 141-144, 228-234 (excerpts).

Ludovico Geymonat

GALILEO GALILEI

2. Of great importance were the attitudes of the two most influential religious orders of that period—the Jesuits and the Dominicans—toward this delicate problem. 74

At the beginning of the seventeenth century, the Jesuit order was the most influential depository of the loftiest culture within the Church, and had initiated into its ranks some eminent scholars in mathematics, physics, and astronomy. This deep interest in the scientific disciplines, however, did not imply any genuine open-mindedness toward modernity. Rather, it constituted an intelligent attempt by the Jesuits to place all new researches on the rails of orthodoxy, and to derive from them some advantage that would give luster to the great authority of the 75
Church.

This preoccupation with reconciling the new sciences to orthodoxy at any cost often revealed itself in attempts to give them a philosophical interpretation in agreement with the teachings of Aristotle. We have had a particularly instructive example of this in Father Clavius' subtle device for reconciling the sphericity of the moon with the discovery of its mountainous character. At that time, Aristotelianism was unanimously judged by the greatest theologians to be the metaphysics best suited to provide a firm base for Catholic dogma; hence agreement with Aristotle implied secure orthodoxy.

It is easy, therefore, to understand the vacillation of the Jesuits up to 1616 with regard to the Copernican controversy. They did not hide their open-mindedness toward this very important astronomical theory; at the same time, however, they hesitated to declare themselves definitely favorable to it because of its obvious and absolute irreconcilability with any form of Aristotelianism. This hesitation is clearly visible in the reply sent by Cardinal Bellarmine (the greatest theologian of the Jesuit order) to the Carmelite Foscarini, who had requested the cardinal's opinion of his famous little tract (the *Letter* of which we have spoken above). Bellarmine wrote:

"1. I say that it appears to me that your Reverence and Signor Galileo did prudently to content yourselves with speaking hypothetically and not positively, as I have always believed Copernicus did. For to say that assuming the earth moves and the sun stands still saves all the appearances better than eccentrics and epicycles is to speak well. This has no danger in it, and it suffices for mathematicians. But to wish to affirm that the sun is really fixed in the center of the heavens and merely turns upon itself without traveling from east to west, and that the earth is situated in the third sphere and revolves very swiftly around the sun, is a very dangerous thing, not only because it irritates all the the-

the Jesuits. In the second, the accusation was inspired by the Jesuits and included charges against some Dominicans who had authorized publication of Galileo's *Dialogue Concerning the Two Chief World Systems.*

Ludovico Geymonat, *Galileo Galilei*, tr. Stillman Drake, McGraw-Hill Book Co., New York, 1965, pp. 74–77 (footnotes deleted).

Jerome J. Langford

GALILEO, SCIENCE AND THE CHURCH (1)

4.4

Galileo read Bellarmine's letter to Foscarini and jotted down 69
some pertinent observations on its contents. He presented his position in 1615 in his *Letter to the Grand Duchess Christina,* 70
which was circulated and copied, but not published until 1636. He began the *Letter* with a review of the violent opposition which his discoveries had brought down upon him:

> ... I discovered many things in the heavens which had not been seen before our own age. Both the novelty of these things as well as some consequences which naturally followed from them and which contradicted some physical notions commonly held among academic professors, stirred up no small number of professors against me.

Next he attributes the scriptural objections to the same professors:

> ... They threw various charges and published many writings filled with vain arguments, and they made the serious mistake of sprinkling these with citations taken from places in the Bible which they had failed to understand properly, and which were far from suitable for their purposes.
>
> ... First they have endeavored to spread the opinion that such propositions in general are contrary to the Bible and are consequently damnable and heretical. They know that it is part of human nature to support causes which tend to oppress one's neighbor, no matter how unjustly, rather than those by which a man might receive some due encouragement. So they have had no trouble in finding men who would preach, with great confidence, the damnability of the new doctrine. And they even preach this from their pulpits thus doing impious injury not only to that doctrine and its adherents, but to all mathematics and mathematicians in general. Next, becoming bolder and hoping (though vainly) that this seed which first took root in their hypocritical minds would branch out and upward, they began spreading rumors among the people that before long this doctrine would be condemned by the supreme authority.

After reminding his readers that Copernicus's book was accepted by the Church long before this dispute arose, he asks why it should be condemned now, especially after "manifest experiences and necessary proofs have shown the Copernican doctrine
to be well grounded." And, he adds pointedly, Copernicus's 71
book should certainly not be condemned by those who have never even read it:

> I hope to show that I proceed with much greater piety than they do when I argue not against condemning this book, but against condemning it in the way they suggest—that is, without understanding it, weighing it, or so much as reading it. For Copernicus never discusses matters of religion or faith, nor does he use arguments that depend in any way upon the authority of

4.5

Pope John Paul II
THE RESPONSIBILITY OF SCIENCE

245 LADIES and Gentlemen: In addressing you who honorably represent the rich horizons of modern science, I wish in the first place to thank you cordially for your visit, and to tell you that your presence here this morning seems to me to have a deeply symbolic value, for you attest to the fact that between the Church and science a fruitful dialogue is being developed.

Nor am I alone in welcoming you. My brothers, the cardinals of the Holy Church present in Rome, and other leading figures of the Holy See — whom I am pleased to greet with you and whom I likewise thank for being here — testify to the importance which the Church attributes to this dialogue.

We cast our minds back to an age when there had developed between science and faith grave incomprehension, the result of misunderstandings or errors, which only humble and patient reexamination succeeded in gradually dispelling. So we should rejoice together that the world of science and the Catholic Church have learned to go beyond those moments of conflict, understandable no doubt, but nonetheless regrettable. This was the result of a more accurate appreciation of the methods proper to the different orders of knowledge and the fruit of bringing to research a more rigorous attitude of mind.

The Church and science itself have reaped great profit from this and have discovered through reflection and sometimes painful experience the
246 paths that lead to truth and objective knowledge.

To you who are preparing to mark the 350th aniversary of the publication of Galileo Galilei's great work, *Dialoghi sui due massimi sistemi del mondo*, I would like to say that the Church's experience, during the Galileo affair and after it, has led to a more mature attitude and to a more accurate grasp of the authority proper to it. I repeat before you what I stated before the Pontifical Academy of Sciences on Nov. 17, 1979: "I hope that theologians, scholars and historians, animated by a spirit of sincere collaboration, will study the Galileo case more deeply and, in frank recognition of wrongs, from whichever side they come, will dispel the mistrust which still forms an obstacle, in the minds of many, to a fruitful concord between science and faith, between the Church and the world. I give all my support to this task, which will be able to honor the truth of faith and of science and open the door to future collaboration."[1]

As you know, I have asked for the formation of an interdisciplinary research team for the careful study of the whole question. Its work is progressing very encouragingly, and there are good grounds for hoping that it will make an important contribution to the examination of the whole matter.

The Church itself learns by experience and reflection, and it now understands better the meaning that must be given to freedom of research, as I said to the representatives of the Spanish universities on Nov. 3, 1982:

"The Church upholds freedom of research, which is one of the most noble attributes of man. It is through research that man attains to Truth —

Jerome J. Langford

GALILEO, SCIENCE AND THE CHURCH (2)

4.6

So far so good. He has spoken like a superior theologian. But 72
now he falters:

> Yet even in those propositions which are not matters of faith, the authority of Scripture ought to be preferred over that of all human writings which are supported only by bare assertions or probable arguments, and not set forth in a demonstrative way. This I hold to be necessary and proper to the same extent that Divine Wisdom surpasses all human judgment and conjecture.

And again:

> I take this to be an orthodox and indisputable doctrine, and I find it explicitly in St. Augustine when he speaks of the shape of heaven and what we may believe concerning it. Astronomers
> seem to declare what is contrary to Scripture, for they held the 73
> heavens to be spherical while the Scripture calls it "stretched out like a curtain" [Ps 103:2]. St. Augustine is of the opinion that we are not to be concerned lest the Bible contradict astronomers; we are to believe its authority if what they say is founded only on the conjectures of frail humanity. But if what they say is proved by *unquestionable arguments*, this holy Father does not say that the astronomers are to be ordered to dissolve their proofs and declare their own conclusions to be false. Rather, he says, it must be demonstrated that what is meant in the Bible by "curtain" is not contrary to their proofs.

He goes on to say:

> From the above words I conceive that I may deduce this doctrine: that in the books of the sages of this world there are contained some physical truths which are soundly demonstrated, and others that are merely stated. As to the former, it is the job of wise theologians to show that they do not contradict the Holy Scriptures. As to the propositions which are stated but not rigorously demonstrated, anything contrary to the Bible involved in them must be held to be undoubtedly false and should be proved so by every possible means.

Thus after his praiseworthy defense of the fact that Scripture does not teach science, now he yields. Scripture, he says, is a superior authority even in science unless a contrary physical argument is *demonstrative*. Galileo, in this series of passages at least, makes the same mistake as Bellarmine did in conceding the highest authority to Scripture even in matters which are not of faith or morals. He gives Scripture strict scientific authority over physical arguments which are only probable. And scientifically he loses his case. He had no demonstrative proof of the Copernican system. At the most, all he could show was that the Copernican hypothesis was superior to the Ptolemaic and it was not easy even to do this. Moreover, Tycho's system accounted for all of Galileo's passages:

> ... but when nearly everything the philosophers and astronomers say on the other side is proved to be quite false, and all of it inconsequential, then this side should not be deprecated or called paradoxical simply because it cannot be completely proved.
>
> ... As to rendering the Bible false, that is not and never will be the intention of Catholic astronomers such as I am; rather, our opinion is that the Scriptures accord perfectly with demonstrated physical truth. But let those theologians who are not astronomers guard against rendering the Scriptures false by trying to interpret it against propositions which may be true and might be proved so.
>
> Hence I should think it would be the part of prudence not to permit anyone to usurp scriptural texts and force them in some way to maintain any physical conclusion to be true, when at some future time the senses and demonstrative or necessary reasons may show the contrary. Who indeed will set bounds to human ingenuity? Who will assert that everything in the universe capable of being perceived is already discovered and known?

But, reading the *Letter to the Grand Duchess*, one could easily get the impression that its author felt he had already demonstrated
the new astronomy. It was not Galileo's intention to deceive his 76
readers, but some of his remarks are openly exaggerated or at least ambiguous. For example:

> They also know that I support this [the Copernican] position not only by refuting the arguments of Ptolemy and Aristotle, but by producing many counter arguments, in particular, some which pertain to physical effects whose causes can perhaps be accounted for in no other way.

Again and again in the *Letter* he talks about the "manifest sense experience and necessary demonstrations" which cannot be condemned on, and need not be subject to, the authority of Scripture. He seems to imply that the Copernican theory is backed by such experience and demonstrations. Perhaps he thought there was no point in giving his arguments in a logical sequence backed up by his experimental observations because he felt that no one would listen even if he did. After all, he had written to Dini:

> How can I do this [give proofs which demonstrate the Copernican position] and not merely be wasting my time, when those Peripatetics who must be convinced show themselves incapable of following even the simplest and easiest of arguments, while on the other hand they are seen to set great store in worthless propositions.

Now that is not an entirely fair accusation. If he had had the proofs, he would have found a great deal of support, especially from the Jesuits. Rather than give a thorough treatment of his proofs in his *Letter to the Grand Duchess*, he claimed that:

> If truly demonstrated physical conclusions need not be subordinated to biblical passages, but the latter must rather be shown not to interfere with the former, then before a physical proposition is condemned, it must be shown to be not rigorously demonstrated—and this is to be done not by those who hold the proposition to be true, but by those who judge it to be false. This seems to be very reasonable and natural, for those who believe an argument to be false may much more easily find the fallacies in it than can those who consider it to be true and conclusive.

Arthur Koestler 4.7

A SHIFTING OF THE BURDEN

3. *The Shifting of the Burden*

Faithful Father Castelli, now Professor of Mathematics at 439
Pisa, the post from which Galileo had started his career, had been invited for dinner at Court. An illustrious company was present, including the Duke's mother, the Dowager Duchess Christina of Lorraine, his wife Madeleine of Austria, and several other guests, among them Dr Boscaglia, a professor of philosophy. The conversation was led by Madame Christina, who appears to have conformed to the idea of a bossy, talkative, and scatterbrained Dowager. During dinner she felt the sudden urge 'to learn all about' those Medicean planets. First she wanted to know their positions, then whether they were real or just illusions. Both Castelli and Boscaglia solemnly confirmed that they were real. Soon after that, dinner was over, and Father Castelli left.

> But I had hardly come out of the place when Madame Christina's porter overtook me and told me that she wished me to return [Castelli's report to Galileo continued]. Now before I tell you what ensued, you must first know that while we were at table, Dr Boscaglia had had the ear of Madame for a while; and conceding as true all the new things you have discovered in the sky, he said that only the motion of the earth had something incredible in it, and could not take place, in particular because Holy Scripture was obviously contrary to this view.

When Castelli returned to the drawing-room, 'Madame began, after some questions about myself, to argue Holy Scripture against me. Thereupon, after having made suitable disclaimers, I commenced to play the theologian and ... carried things off like a paladine.' Everybody took the side of Castelli and Galileo, 'only Madame Christina remained against me, but from her manner I judged that she did this only to hear my answers. Professor Boscaglia never said a word.' [12]

In subsequent letters, Castelli reported that Boscaglia had 440
once more been defeated in debate, that even the irascible Dowager had been won over, and that the subject had been dropped.

This, then, is the incident which touched off the drama.

As on that previous occasion, when Lorini had remarked on 'Ipernicus – or whatever his name is', Galileo was at once up in arms. His counter-blast to the dinner-table chirpings of the obscure Dr Boscaglia (who is never heard of again), was a kind of theological atom bomb, whose radioactive fall-out is still being felt. It took the form of a *Letter to Castelli*, enlarged a year later into a *Letter to the Grand Duchess Christina*. It was intended to be widely circulated, which indeed it was. Its purpose was to silence all theological objections to Copernicus. Its result was the precise opposite: it became the principal cause of the prohibition of Copernicus, and of Galileo's downfall.

As a work of polemical literature the *Letter* is a master-

> Let us grant then that theology is conversant with the loftiest divine contemplation, and occupies the regal throne among the sciences by this dignity. But acquiring the highest authority in this way, if she does not descend to the lower and humbler speculations of the subordinate sciences and has no regard for them because they are not concerned with blessedness, then her professors should not arrogate to themselves the authority to decide on controversies in professions which they have neither studied nor practised. Why, this would be as if an absolute despot, being neither a physician nor an architect, but knowing himself free to command, should undertake to administer medicines and erect buildings according to his whim – at grave peril of his poor patients' lives, and the speedy collapse of his edifices. . . .[15] 442

While reading this superb manifesto of the freedom of thought, one tends to forgive Galileo his human failings. These, however, become only too apparent in the piece of special pleading which follows the passage I have quoted, and which was to have disastrous consequences.

After invoking Augustine's authority once more, Galileo draws a distinction between scientific propositions which are 'soundly demonstrated' (i.e. proven) and others which are 'merely stated'. If propositions of the first kind contradict the apparent meaning of passages in the Bible, then, according to theological practice, the meaning of these passages must be reinterpreted – as was done, for instance, with regard to the spherical shape of the earth. So far he has stated the attitude of the Church correctly; but he continues: 'And as to the propositions which are stated but not rigorously demonstrated, anything contrary to the Bible involved by them must be held undoubtedly false and should be proved so by every possible means.'[16]

Now this was demonstrably not the attitude of the Church. 'Propositions which are stated but not rigorously demonstrated', *such as the Copernican system itself*, were not condemned outright, if they seemed to contradict Holy Scripture; they were merely relegated to the rank of 'working hypotheses' (where they rightly belong), with an implied: 'wait and see; if you bring proof, then, but only then, we shall have to reinterpret Scripture in the light of this necessity'. But Galileo did not 443
want to bear the burden of proof; for the crux of the matter is, as will be seen, that he had no proof. Therefore, firstly, he conjured up an artificial black-or-white alternative, by pretending that a proposition must either be accepted or outright condemned. The purpose of this sleight of hand becomes evident from the next sentence:

> Now if truly demonstrated physical conclusions need not be subordinated to biblical passages, but the latter must rather be shown not to interfere with the former, then *before a physical proposition is condemned it must be shown to be not rigorously demonstrated* – and this is to be done not by those who hold the proposition to be true, but by those who judge it to be false. This seems very reasonable and natural, for those who believe an argument to be false may much more easily find the fallacies in it than men who consider it to be true and conclusive. . . .[17]

The burden of proof has been shifted. The crucial words are those in (my) italics. It is no longer Galileo's task to prove the Copernican system, but the theologians' task to disprove it. If they don't, their case will go by default, and Scripture must be reinterpreted.

an animal, all other motions of its members would also cease, so if the rotation of the sun were to stop, the rotations of all the planets would stop too.'[18] Thus he not only assumed, with Kepler, the *annual* revolutions of the planets to be caused by the sun, but also their *daily* rotation round their axes – an *ad hoc* hypothesis with no more 'rigorous proof' than the analogy with the animal's heart. He then concludes that when Joshua cried: 'Sun, stand thou still,' the sun stopped rotating, and the earth in consequence stopped both its annual and daily motion, But Galileo, who came so close to discovering the law of inertia, knew better than anybody that if the earth suddenly stopped dead in its track, mountains and cities would collapse like match-boxes; and even the most ignorant monk, who knew nothing about impetus, knew what happened when the horses reared and the mail-coach came to a sudden halt, or when a ship ran against a rock. If the Bible was interpreted according to Ptolemy, the sudden stand-still of the sun would have no appreciable physical effect, and the miracle remained credible as miracles go; if it was interpreted according to Galileo, Joshua would have destroyed not only the Philistines, but the whole earth. That Galileo hoped to get away with this kind of painful nonsense, showed his contempt for the intelligence of his opponents.

In the *Letter to the Grand Duchess Christina* the whole tragedy of Galileo is epitomized. Passages which are classics of didactic prose, superb formulations in defence of the freedom of thought, alternate with sophistry, evasion, and plain dishonesty.

Arthur Koestler, *The sleepwalkers*, Penguin, Harmondsworth, 1964, pp. 439–445.

Jerome J. Langford

GALILEO, SCIENCE AND THE CHURCH (3)

While Galileo was composing the revised *Letter*, there was a 58
good deal of activity in Rome. Rumor had it that the Copernican theory was about to be banned. Galileo's friends in Rome went to work pumping the authorities to find out just how things stood. Monsignor Giovanni Ciampoli wrote that he had spoken to Maffeo Cardinal Barberini. He reported, on February 28, 1615:

> Cardinal Barberini, who, as you know from experience, has always admired your ability, told me just last evening that with regard to these opinions he would like to see greater caution in not going beyond the arguments used by Ptolemy and Copernicus, and finally, in not exceeding the limitations of physics and mathematics. The explanation of Scripture is claimed by the theologians as their field, and, if new things are introduced, even by a capable mind, not everyone has the dispassionate faculty of taking them just as they are said.

A few days later Dini wrote to Galileo:

> . . . I had a long talk with Cardinal Bellarmine about the points you mentioned . . . As to Copernicus, his Lordship said that he
> did not believe that his work would be forbidden, and, in his 59
> opinion, the worst that could happen to it would be the insertion of a note stating that the theory was introduced in order to save the appearances, or something like that, just as epicycles had been introduced for that purpose. With this reservation, he said, you are at liberty to discuss these matters whenever you wish. Concerning the Copernican system, he said, the greatest obstacle to it seems to be the passage [the sun] "rejoiceth as a giant to run the way" together with the words that follow, which all commentators up to now have understood as implying that the sun is in motion. I replied that Holy Scripture in this passage might simply be using a common mode of expression. He answered that with regard to such a problem we must not be too hasty, just as it would not be right to hurriedly condemn any of these opinions . . .

These two letters together with the Holy Office's quick dismissal of Lorini's denunciation indicate that the acceptability of the new astronomy was still a moot point in Rome.

Within a few days word arrived from Prince Cesi in Rome that a Carmelite friar, Paolo Antonio Foscarini, had just published a book which attempted to show that the Copernican system was not contrary to Holy Scripture. Furthermore, the author had sent a copy to Cardinal Bellarmine and asked his opinion of it. Stillman Drake is perhaps right in saying that this unexpected support from a qualified theologian may have been a crucial factor in Galileo's decision not to accept a compromise. At any rate, Galileo decided to try the ground. He wrote to Dini saying that Copernicus (despite the claims made in the preface of the *De* false which has been demonstrated. But I do not believe that there is any such demonstration; none has been shown to me. It is not the same thing to show that the appearances are saved by assuming that the sun is at the center and the earth is in the heavens, as it is to demonstrate that the sun is really in the center and the earth in the heavens. I believe that the first demonstration might exist, but I have grave doubts about the second, and in a case of doubt, one may not depart from the Scriptures as explained by the holy Fathers. I add that the words "the sun also riseth and the sun goeth down, and hasteneth to the place where he ariseth, etc." were those of Solomon, who not only spoke by divine inspiration but was a man wise above all others and most learned in human sciences and in the knowledge of all created things, and his wisdom was from God. Thus it is not too likely that he would affirm something which was contrary to a truth either already demonstrated, or likely to be demonstrated. And if you tell me that Solomon spoke only according to the appearances, and that it seems to us that the sun goes around when actually it is the earth which moves, as it seems to one on a ship that the beach moves away from the ship, I shall answer that one who departs from the beach, though it looks to him as though the beach moves away, he knows that he is in error and corrects it, seeing clearly that the ship moves and not the beach. But with regard to the sun and the earth, no wise man is needed to correct the error, since he clearly experiences that the earth stands still and that his eye is not deceived when it judges the sun to move, just as it is not deceived when it judges that the moon and stars move. And that is enough for the present.

Jerome J. Langford, *Galileo, science and the church*, revd edn, University of Michigan Press, Ann Arbor, 1971, pp. 58-61 (footnotes deleted).

Jerome J. Langford

GALILEO, SCIENCE AND THE CHURCH (4)

Since Bellarmine's letter to Foscarini was an unofficial but 62
quite definite statement of the Church's attitude toward the new astronomy, we must pause to comment on its major points. The Cardinal urges Foscarini and Galileo to treat the Copernican theory as a hypothesis only. Not aware that the preface to Copernicus's great work was spurious, he thinks that the Canon of Frauenberg never intended it to be taken as a reality. The Jesuits Clavius and Grienberger must have told Bellarmine that the new system saved the appearances better than did Ptolemy's, and he is willing to admit this. But to try to establish the system as fact is asking for trouble. He goes on to quote the Council of Trent and here his theology definitely limps. Trent forbade going contrary to the teaching of the Fathers when their teaching on a subject was unanimous and the subject itself was a matter of faith or morals. Thus for a scriptural interpretation of the Fathers to enjoy unquestionable validity, two requirements had to be met. First, all who wrote on a text had to explain it in the same way. This "unanimous consent" of the Fathers meant that there must be a moral unanimity. If many of the great Fathers interpreted it in one way and no other Church Father contradicted them, the exegesis could be accepted as the universal interpretation of the Fathers. Secondly, the Fathers had to affirm, explicitly or implicitly, that the text under consideration pertained to a matter of faith or morals. Therefore, if there was not unanimous consent 63
or if the interpretation was not proposed as a certain doctrine pertaining to faith or morals, but merely as an opinion or conjecture, it did not necessarily have to be followed Applying this to the case at hand, it would be very difficult to establish that the unanimous verdict of the Fathers was that the sun moved and the earth stood still. Many Fathers of the Church describe the heavens as a Firmament which is unmoved. Nevertheless, on the surface, it would appear that most, if not all, of them did hold that the earth was immobile.

But of the second requirement there can be no doubt. Not one Father can be found who declares that the motion of the heavens or the immobility of the earth pertains to faith or morals. St. Augustine explicitly teaches that it most certainly does not. Next comes Bellarmine's curious distinction between what is a matter of faith *by reason of the subject matter* and what is so *by reason of the one who speaks.* He says that even if the motion of the sun is not a matter of faith from the first, that is, even if astronomy does not fall into the category of faith or morals, it still is propounding scientific theory. They had no intention of expounding the hidden mechanisms of sensible phenomena. Their goal was to teach religion, not physics. As the scholarly Cardinal Baronius phrased it, "The Holy Ghost intended to teach us how to go to heaven, not how the heavens go." St. Augustine and St. Thomas Aquinas clearly taught that the Holy Spirit, speaking through the sacred writers, in no way attempted to reveal a system of astronomy. Thus while Sacred Scripture might or might not accurately describe physical phenomena, it was not meant to be used as conclusive proof for or against any purely physical theory. St. Augustine wrote that: 65

> One does not read in the Gospel that the Lord said: I will send to you the Paraclete who will teach you about the course of the sun and moon. For He willed to make them Christians, not mathematicians.[26]

In commenting on the account of creation as given in the Book of Genesis, Augustine cautioned:

> One could ask which shape and form of heaven must be accepted by faith on the authority of Holy Scripture. Many dispute about these things which the sacred writers passed by in silence, because they are without importance for attaining eternal life ... in short, the Spirit of God which spoke through them did not wish to teach things which contribute nothing to salvation.[27]

St. Thomas Aquinas emphasized repeatedly that in matters of physical science, the sacred writers went by the common conceptions and modes of speech in use among their people. They put down "what God, speaking to men, signified, in the way men could understand and were accustomed to." In questions of this 66
kind, St. Thomas gave two rules to be followed: "First, hold the truth of Scripture without wavering. Second, since Holy Scripture can be explained in a multiplicity of senses, one should adhere to a particular explanation only in such measure as to be ready to abandon it if it be proved with certainty to be false; lest Holy Scripture be exposed to the ridicule of unbelievers and obstacles be placed to their believing. Elsewhere, he remarked that:

> When philosophers are agreed upon a point, and it is not contrary to our faith, it is safer, in my opinion, neither to lay down such a point as a dogma of faith, even though it is so presented by the philosophers, nor to reject it as against faith, lest we thus give to the wise of this world an occasion of despising our faith.[30]

Looking back, we can see that two principles should have been considered by the theological judges of the new astronomy. First, the traditional interpretation was to be held unless solid reasons dictated otherwise. Second, in matters of pure physical science, the Scriptures are not the criterion for establishing one system or forbidding another, since they do not teach science. The correct theological procedure would have been to combine these two principles into a practical and valid norm for solving what appear to be discrepancies between Scripture and science. Had this been done, the opinion that the Scriptures confirmed the sun's motion no choice. He had to carry forth the flame of truth without regard to whom or what it burned on the way. It was not that simple, however. Galileo was convinced that he had the truth. But objectively he had no proof with which to win the allegiance even of open-minded men. It is a complete injustice to contend, as some historians do, that no one would listen to his arguments, that he never had a chance. The Jesuit astronomers had confirmed his discoveries; they awaited eagerly for further proof so that they could abandon Tycho's system and come out solidly in favor of Copernicus. Many influential churchmen believed that Galileo might be right, but they had to wait for more proof. Galileo was asked by his friends to be cautious. To beat the university philosophers at philosophy was one thing: to challenge theologians in theology was quite another. Bellarmine had given him an opening, however narrow it might seem to us, "Prove your theory and we will change our exegesis, otherwise teach it as a hypothesis which saves the appearances." Even today scientific honesty requires a distinction between hypothesis and fact. E. A. Burtt points out that:

> It is safe to say that even had there been no religious scruples whatever against the Copernican astronomy, sensible men all over Europe, especially the most empirically minded, would have pronounced it a wild appeal to accept the premature fruits of an uncontrolled imagination, in preference to the solid inductions, built up gradually through the ages, of men's confirmed sense experience. In the strong stress on empiricism, so characteristic of present-day philosophy, it is well to remind ourselves of this fact. Contemporary empiricists, had they lived in the sixteenth century, would have been the first to scoff out of court the new philosophy of the universe.

Obviously it is not entirely accurate to picture Galileo as an innocent victim of the world's prejudice and ignorance. Part of the blame for the events which follow must be traced to Galileo himself. He refused the compromise, then entered the debate without sufficient proof and on the theologians' home grounds.

Jerome J. Langford, *Galileo, science and the church*, revd edn, University of Michigan Press, Ann Arbor, 1971, pp. 62–69 (footnotes deleted).

Arthur Koestler

4.10

THE PARTING OF THE WAYS

We have followed the events of 1615, from Lorini's denunciation of Galileo's *Letter* and Caccini's denunciation of his personal activities, to the collapse of the case against him in November. The proceedings were conducted in secret, and Galileo had no part in them; but his friends in Rome knew that something was up, and kept him informed of all rumours and developments. Among his informants were Cardinal Piero Dini, Archbishop of Fermo, and Monsignor Giovanni Ciàmpoli. The letters exchanged, during 1615, between these two in Rome and Galileo in Florence, are important for the understanding of the developments which led to the prohibition of Copernicus. 451

On 16 February, Galileo sent a copy of his *Letter to Castelli* to Dini, with the request that it should be shown to Father Grienberger and, if possible, to Cardinal Bellarmine. In his covering letter there were the usual complaints about the hostility surrounding him. He remarked that the *Letter to Castelli* was written in haste and that he was going to improve and extend it; the extended version, as we know, became the *Letter to the Grand Duchess Christina.*

Before Dini answered, Ciàmpoli wrote, at the end of February (my italics): 452

> Cardinal Barberini [the future Pope Urban VIII], who, as you know from experience, has always admired your worth, told me only yesterday evening that with respect to these opinions he would like greater caution in *not going beyond the arguments used by Ptolemy and Copernicus,** and finally in not exceeding the limitations of physics and mathematics. For to explain the Scriptures is claimed by theologians as their field, and if new things are brought in, even by an admirable mind, not everyone has the dispassionate faculty of taking them just as they are said. . . .[23]

A few days later, on 3 March, Dini's answer arrived (my italics):

> With Bellarmine I spoke at length of the things you had written. . . . And he said that as to Copernicus, there is no question of his book being prohibited; the worst that might happen, according to him, would be the addition of some material in the margins of that book to the effect that Copernicus had introduced his theory in order to save the appearances, or some such thing – just as others had introduced epicycles without thereafter believing in their existence. And *with a similar precaution you may at any time deal with these matters.* If things are fixed according to the Copernican system, [he said], it does no appear presently that they would have any greater obstacle in the Bible than the passage '[the sun] exults as a strong man to run his course,' etc., which all expositors up to now have understood by attributing motion to the sun. And although I replied that this also could be explained as a concession to our ordinary forms of expression, I was told in answer that this

* i.e., that they are to be regarded as mathematical hypotheses only, in the sense of Osiander's preface.

good sense and to run no risk whatever. Such a manner of speaking suffices for a mathematician. But to want to affirm that the Sun, in very truth, is at the centre of the universe and only rotates on its axis without travelling from east to west, and that the Earth is situated in the third sphere and revolves very swiftly around the Sun, is a very dangerous attitude and one calculated not only to arouse all Scholastic philosophers and theologians but also to injure our holy faith by contradicting the Scriptures. ...

Second, I say that, as you know, the Council of Trent forbids the interpretation of the Scriptures in a way contrary to the common agreement of the holy Fathers. Now if your Reverence will read, not merely the Fathers, but modern commentators on Genesis, the Psalms, Ecclesiastes, and Joshua, you will discover that all agree in interpreting them literally as teaching that the Sun is in the heavens and revolves round the Earth with immense speed and that the Earth is very distant from the heavens, at the centre of the universe, and motionless. Consider, then, in your prudence, whether the Church can support that the Scriptures should be interpreted in a manner contrary to that of the holy Fathers and of all modern commentators, both Latin and Greek. ...

Third, I say that, *if there were a real proof* that the Sun is in the centre of the universe, that the Earth is in the third sphere, and that the Sun does not go round the Earth but the Earth round the Sun, then we should have to proceed with great circumspection in explaining passages of Scripture which appear to teach the contrary, and we should rather have to say that we did not understand them than declare an opinion to be false which is proved to be true. But I do not think there is any such proof *since none has been shown to me*. To demonstrate that the appearances are saved by assuming the sun at the centre and the earth in the heavens is not the same thing as to demonstrate that *in fact* the sun is in the centre and the earth in the heavens. I believe that the *first demonstration may exist, but I have very grave doubts about the second*; and in case of doubt one may not abandon the Holy Scriptures as expounded by the holy Fathers. ...[30]

The italicized passage under the first heading states clearly that it is admissible not only to expound the Copernican system, but also to say that *as a hypothesis it is superior to Ptolemy's*. This is 'to speak with excellent good sense' so long as we remain in the domain of hypothesis. Under the second heading he paraphrases the legislative decision of the Council of Trent against interpreting Scripture in ways contrary to tradition (directed, of course, not against Copernicus but Luther). Under the third heading the condition is stated which would justify an exception to this rule being made; to wit, that the new cosmology should be 'really proven' (or 'truly demonstrated'). Since no proof has been shown to him, he has 'grave doubts' whether such proof exists; and in case of doubt the request for reinterpreting the Bible must be rejected. He had consulted Grienberger, and Grienberger must have truthfully informed him that no physical proof for the earth's motion had been adduced. He may have added that the absence of stellar parallax and the nine epicycles ascribed to the earth alone, were rather in the nature of a disproof.

Bellarmine had placed the burden of proof for the Copernican system back where it belonged: on the advocates of the system. There were only two possibilities left to Galileo: either to supply the required proof, or to agree that the Copernican

* He evidently refers here to those epicycles which were needed in the Ptolemaic system to explain the apparent retrogression of the planets and which Copernicus dispensed with.

who had preached against it, was reprimanded by the Preacher General of his Order; and that, according to the accepted rules of the game, the scriptural objections could not be refuted on scriptural grounds, only by the scientific proofs which Bellarmine demanded and which Galileo was unable to supply.

After the passage, that I have already quoted, about the stupidity of his opponents, Galileo went on:

> Yet I should not despair of overcoming even this difficulty if I were in a place where I could use my tongue instead of my pen; and if I ever get well again so that I can come to Rome, I shall do so, in the hope of at least showing my affection for the holy Church. My urgent desire on this point is that no decision be made which is not entirely good. Such it would be to declare, under the prodding of an army of malign men who understand nothing of the subject, that Copernicus did not hold the motion of the earth to be a fact of nature, but as an astronomer merely took it to be a convenient hypothesis for explaining the appearances. ...

'The army of malign men who understand nothing of the subject' again obviously included Bellarmine, who had written that he had always understood Copernicus to speak 'hypothetically and not absolutely'.

Perhaps the one genuine sentiment in the letter was Galileo's 458
wish to get to Rome where he could use his 'tongue instead of his pen'. Early in December he arrived in Rome; the final phase of the battle had begun.

6. *The 'Secret Weapon'*

This time there was no triumphant reception at the Roman College. Father Grienberger sent word that it would be better for Galileo to bring convincing scientific proof in support of Copernicus before trying to adjust Scripture to him.[32] The Tuscan Ambassador in Rome, Guicciardini, had warned Duke Cosmo against Galileo's coming to Rome, and Bellarmine, who foresaw the consequences, had also advised against it.[33] But the Duke had given in to Galileo, and on his instructions Galileo took up quarters at the Villa Medici – then the Tuscan Embassy – 'with board for himself, a secretary, a valet, and a small mule'.[34]

I have quoted some samples of Galileo's superb technique in his written polemics. According to his contemporaries, he was even more effective when he used 'his tongue instead of his pen'. His method was to make a laughing stock of his opponent in which he invariably succeeded, whether he happened to be in the right or in the wrong. Here is one Roman witness, Monsignor Querengo, describing Galileo in action:

> We have here Signor Galileo who, in gatherings of men of curious mind, often bemuses many concerning the opinion of Copernicus, which he holds for true. ... He discourses often amid fifteen or twenty guests who make hot assaults upon him, now in one house, now in another. But he is so well buttressed that he laughs them off; and although the novelty of his opinion leaves people unpersuaded, yet he convicts of vanity the greater part of the arguments with which his opponents try to overthrow him. Monday in particular, in the house of Federico Ghisileri, he achieved wonderful feats; and what I liked most was that, before answering the opposing reasons, he amplified them and fortified them himself with new grounds which appeared invincible, so that, in demolishing them subsequently, he made his opponents look all the more ridiculous.[35]

achievements – that its conception can only be explained in psychological terms. It is completely out of keeping with his intellectual stature, the method and trend of his thought; it was not a mistake but a delusion.

Armed with his new 'secret weapon' (as a modern scholar has called Galileo's theory of the tides [39]), he now decided to make a direct assault on the Pope. It seems that all of Galileo's friends who had access to the Pope – Cardinals Dini, Barberini, del Monte, etc. – refused to act as intermediaries, for the mission was finally entrusted to Cardinal Alessandro Orsini, a youth of twenty-two. Galileo wrote down for him his idea of the tides; the sequel is described as follows in Ambassador Guicciardini's report to Duke Cosmo II of Tuscany:

Galileo has relied more on his own counsel than on that of his friends. The Lord Cardinal del Monte and myself, and also several cardinals from the Holy Office, had tried to persuade him to be quiet and not to go on irritating this issue. If he wanted to hold this Copernican opinion, he was told, let him hold it quietly and not spend so much effort in trying to have others share it. Everyone fears that his coming here may be very prejudicial and that, instead of justifying himself and succeeding, he may end up with an affront.

As he felt people cold toward his intention, after having pestered and wearied several cardinals, he threw himself on the favour of Cardinal Orsini, and extracted to that purpose a warm recommendation from Your Highness. The Cardinal, then, last Wednesday in Consistory, I do not know with what circumspection and prudence, spoke to the Pope on behalf of said Galileo. The Pope told him it would be well if he persuaded him to give up that opinion. Thereupon Orsini replied something, urging the cause, and the Pope cut him short and told him he would refer the business to the Holy Office.

As soon as Orsini had left, His Holiness summoned Bellarmine; and, after brief discussion, they decided that the opinion was erroneous and heretical; and day before yesterday, I hear, they had a Congregation on the matter to have it declared such. Copernicus, and the other authors who wrote on this, shall be amended or corrected or prohibited; I believe that Galileo personally is not going to suffer, because he is prudent and he will feel and desire as Holy Church does. [4 March.] [40]

Arthur Koestler, *The sleepwalkers*, Penguin, Harmondsworth, 1964, pp. 451-461.

Andrew Dickson White

4.11

THE WAR UPON GALILEO

Still, the new truth lived on. Ten years after the martyrdom of Bruno the truth of Copernicus's doctrine was established by the telescope of Galileo. 125

Herein was fulfilled one of the most touching of prophecies. Years before, the opponents of Copernicus had said to him, "If your doctrines were true, Venus would show phases like the moon." Copernicus answered: "You are right; I know not what to say; but God is good, and will in time find an answer to this objection." The God-given answer came when, in 1611, the rude telescope of Galileo showed the phases of Venus. 126

3. The War Upon Galileo

On this new champion, Galileo, the whole war was at last concentrated. His discoveries had clearly taken the Copernican theory out of the list of hypotheses, and had placed it before the world as a truth. Against him, then, the war was long and bitter. The supporters of what was called "sound learning" declared his discoveries deceptions and his announcements blasphemy. Semi-scientific professors, endeavouring to curry favour with the Church, attacked him with sham science; earnest preachers attacked him with perverted Scripture; theologians, inquisitors, congregations of cardinals, and at last two popes dealt with him, and, as was supposed, silenced his impious doctrine forever.

• • •

I shall present this warfare at some length because, so far as I can find, no careful summary of it has been given in our language, since the whole history was placed in a new light by the revelations of the trial documents in the Vatican Library, honestly published for the first time by L'Épinois in 1867, and since that by Gebler, Berti, Favaro, and others. 127

The first important attack on Galileo began in 1610, when he announced that his telescope had revealed the moons of the planet Jupiter. The enemy saw that this took the Copernican theory out of the realm of hypothesis, and they gave battle immediately. They denounced both his method and its results as absurd and impious. As to his method, professors bred in the "safe science" favoured by the Church argued that the divinely appointed way of arriving at the truth in astronomy was by theological reasoning on texts of Scripture; and, as to his results, they insisted, first, that Aristotle knew nothing of these new revelations; and, next, that the Bible showed by all applicable types that there could be only seven planets; that this was proved by the seven golden candlesticks of the Apocalypse, by the seven-branched candlestick of the tabernacle, and by the seven churches of Asia; that from Galileo's doctrine consequences must logically result destructive to Christian truth. Bishops and priests therefore warned their flocks, and multitudes of

Stillman Drake

DISCOVERIES AND OPINIONS OF GALILEO

For the sake of argument we may grant that if in 2
Leopardi's time the physicists had little reason to read the works of Galileo, they have still less reason now. But what about the rest of us? Science now dominates every phase of our culture to a degree that can hardly be exaggerated. It follows that we ought to have an interest in every truly significant phase of this phenomenon which so profoundly affects our lives and thoughts. To the most recent developments in science we are indeed almost obliged to pay some attention, but these are not necessarily its most significant aspects in a cultural sense. So far as scientific facts are con-
cerned, no layman can hope to acquire more than the most 3
superficial smattering; it is a commonplace that today even the best-informed man is not fully in touch with the latest developments in more than a few specialized fields. Facts, however, constitute only a part of what science has to teach us, and they make up neither the most interesting nor the most significant part in relation to the age in which we live. The truly influential and pervasive aspects of modern science are not its facts at all, but rather its method of inquiry and its criterion of truth.

Now those are precisely the things whose introduction created modern science. They were, moreover, first made clear in the writings of Galileo, and perhaps even today there is no other source from which they may be obtained more easily, more clearly, or more entertainingly by the nonscientific reader. It was to the man of general interests that Galileo originally addressed his works, and his remarkable success in explaining his method and revealing his criterion of truth is attested by the prompt and vigorous opposition which he inspired, led by professors who regarded the new method as injurious to philosophy and by priests who believed the new criterion of truth to be inimical to religion. All later attempts to explain scientific method and define scientific truth, however much more logical and thorough, have been considerably less effective.

We may be inclined to take it for granted that we fully understand the implications of scientific method; that we can easily tell whether or not any given statement may properly be called scientific. But the things we take for granted are not always those which we best understand. Thoughtful men of our time are often disturbed to hear the term "scientific" carelessly applied. Some say that it is a term not truly comprehended by anyone unless he has personally confronted laboratory problems or conscientiously designed experiments of his own. No doubt the best way to find out all that is implied by the word "scientific" is to become a scientist, but that is a course not open to everyone. A quite reasonable alternative is to read the writings would do him the equal courtesy of allowing him then to assert that the moon was even more rugged than he had thought before, its surface being covered with mountains and craters of this invisible substance ten times as high as any he had seen.[9] At Pisa the leading philosopher had refused even to look through the telescope; when he died a few months afterward, Galileo expressed the hope that since he had neglected to look at the new celestial objects while on earth, he would now see them on his way to heaven.[10] This sort of good-natured raillery, characteristic of Galileo, was taken up by friends who sprang to his defense while he was occupying himself in new researches. A particularly objectionable opponent named Horky, an assistant of Galileo's old rival Magini, argued that no new stars or planets could exist because astrologers had already taken into account everything in the sky that could have any influence upon the earth and man. Since nature does nothing in vain, and the new planets could serve no purpose, they could not exist. In reply to this it was retorted that the new planets served a very useful purpose, which was to torment Horky and throw the superstitious into confusion.[11] One after another, all attempts to cleanse the heavens of the new celestial bodies came to grief. Philosophers had come up against a set of facts which their theories were utterly unable to explain. The more persistent and determined adversaries of Galileo eventually had to give up arguing and resort to threats.

Meanwhile he had made two more important discoveries and communicated them to Kepler and other serious students, especially to the Jesuits at the Roman College whose support would be most effective of all in Italy. The first was the curious shape of Saturn, which his telescope was unable to resolve into the well-known rings, and which he interpreted as being caused by two stationary satellites adjoining the planet. The other discovery, made after his arrival at Florence to take up his new duties, was of much weightier consequence. It was that Venus passes through a regular series of changes in shape precisely like those of the moon. This discovery proved Ptolemy wrong in a vital part of his planetary theory, for when considered in terms of the relative positions of Venus and the sun it showed that this planet must move not around the earth but around the sun. Copernicus had been puzzled at the apparent absence of such changes, which were required by his theory.[12]

[9] *Opere* xi, 143. This feeble attempt to rescue Aristotle was sponsored by Galileo's most troublesome adversary at Florence, Lodovico delle Colombe (see pp. 79, 148–49, 223).

[10] *Opere* x, 484. The philosopher was Giulio Libri (1550–1610), who had taught both at Pisa and at Padua during Galileo's service in those universities.

[11] *Opere* iii: 1, 177–78. Galileo's defender was his former Scotch pupil John Wedderburn.

[12] *De Revolutionibus* i, 10: "Neither do they grant that any darkness similar to that of the moon is found in the planets, but they assume that these are either self-luminous or are lighted by sunlight throughout their whole bodies." Copernicus refrained from giving his own opinion on the problem. Galileo was much impressed by the fact that this apparent contradiction of the senses had not deterred Copernicus from adhering to the heliocentric system; cf. *Dialogue*, pp. 334–35.

Arthur Koestler

THE TRIAL OF GALILEO

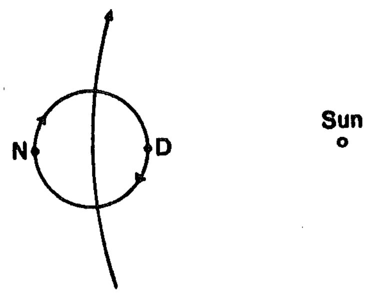
1. *The Tides*

After the issue had been formally decided by the decree of 5 March, Galileo stayed on in Rome for another three months. 'He is of a fixed humour,' the Tuscan Ambassador reported, 'to tackle the friars head on, and to fight personalities who cannot be attacked without ruining oneself. Sooner or later you will hear in Florence that he has madly tumbled into some unsuspected abyss.'[1] In the end, the alarmed Duke ordered Galileo back to Florence.

For the next seven years he published nothing. But his obsession was devouring him. It was the more self-destructive because he could not vent it. He could mutter about 'the ignorance, malice, and impiety of my opponents who had won the day'; but he must have known, without admitting it to himself, that his defeat was really due to the fact that he had been unable to deliver the required proof.

This, I suggest, explains how the delusion about the tides could gain such power over his mind. He had improvised this secret weapon in a moment of despair; one would have expected that once he reverted to a normal frame of mind, he would have realized its fallacy and shelved it. Instead, it became an *idée fixe*, like Kepler's perfect solids. But Kepler's was a creative obsession: a mystic chimera whose pursuit bore a rich and unexpected harvest; Galileo's mania was of the sterile kind. The tides, as I shall presently try to show, were an indirect substitute for the stellar parallax which he had failed to find – a substitute not only in the psychological sense, for there exists a mathematical connexion between the two, which seems to have eluded attention so far.

Galileo's theory of the tides runs, in a highly simplified form, as follows.[2] Take a point on the earth's surface – say, Venice. It has a two-fold motion: the daily rotation round the earth's axis, and its annual revolution round the sun. At night when Venice is at N, the two motions add up; in daytime, at D, they work against each other:



Hence Venice, and with it all the firm land, moves faster at night and slower in the daytime; as a result, the water is 'left behind' at night, and rushes ahead of the land in daytime. This and rational as never before', that the earth moves round the sun.[19]

At this point poor Simplicius turns into a relativist and correctly observes that the curves of the spots would look just the same whether the sun travelled round the earth or the earth round the sun. Salviati proceeds to demolish this objection: if we assume that the sun travels round the earth, the spots will look the same only if we also assume that the sun's axis always remains parallel to itself; and this he finds *'very hard and almost impossible to believe'*.[20] Simplicius, intimidated, pipes down; Sagredo exclaims 'that amongst all the ingenious subtleties I ever heard, I have never met with anything of greater admiration to my intellect or that has more absolutely captivated my judgement'.[21]

One simply gapes. Salviati wins his case by pretending that it was virtually impossible for one heavenly body to travel round another while its axis remains parallel to itself. Yet that is, of course, what the earth does while travelling round the sun: its axis remains parallel to itself at a constant tilt of 23½ degrees. If it was impossible to believe that the sun could move thus, then it was equally impossible that the earth should move thus. Yet in a later section Galileo discusses at great length the reasons *why* the earth moves thus, and explains that the preservation of the fixed tilt of its axis 'is far from having any repugnance or difficulty in it'.[22]

The changing faces of the sunspot-paths were as obvious a consequence of the tilt in the sun's axis as the changing seasons are a consequence of the tilt in the earth's axis. It was as simple as that. But the two pages in which Galileo argues the point against Simplicius[23] are among the most obscure and incomprehensible in the book. He employs his usual tactics of refuting his opponent's thesis without proving his own; in this case not by sarcasm, but by confusing the issue.

There can be no doubt that Galileo's theory of the tides was 486
based on unconscious self-deception; but in the light of the above there can also be little doubt that the sunspot argument was a deliberate attempt to confuse and mislead. To represent the constant tilt of a rotating body as a new and inconceivable hypothesis, when every student since Pythagoras knew that this was the reason why summer followed winter; to obscure this simple issue by the novelty of curving sunspots, while making the complexities of Copernicus appear deceptively simple, was part of a deliberate strategy, based on Galileo's contempt for the intelligence of his contemporaries. We have seen that scholars have always been prone to manias and obsessions, and inclined to cheat about details; but impostures like Galileo's are rare in the annals of science.

The fourth and last day of the *Dialogue* is taken up almost entirely by the theory of the tides, which is elaborated in more detail. The annual variations in the tides are explained by the tilt of the earth's axis, the monthly variations by monthly changes in orbital velocity.[24] Kepler's explanation of the tides by the moon's attraction is rejected with the remark that 'despite his open and penetrating mind' he has 'lent his ear and his assent to the moon's dominion over the waters, to occult properties [gravity] and such-like little fancies'.[24a]

Another surprising thing about the *Dialogue* is that Galileo not only misrepresented the Copernican system as a beautifully simple affair, but seems to have been himself unaware of its complexities. He had never taken much interest in the tiresome

Paul Feyerabend

4.14

AGAINST METHOD

To sum up the content of the last six chapters: 159

When the 'Pythagorean idea' of the motion of the earth was revived by Copernicus it met with difficulties which exceeded the difficulties encountered by contemporary Ptolemaic astronomy. Strictly speaking, one had to regard it as refuted. Galileo, who was convinced of the truth of the Copernican view and who did not share the quite common, though by no means universal, belief in a stable experience, looked for new kinds of fact which might support Copernicus and still be acceptable to 160
all. Such facts he obtained in two different ways. First, by the invention of his *telescope* which changed the *sensory core* of everyday experience and replaced it by puzzling and unexplained phenomena; and by his *principle of relativity and his dynamics* which changed its *conceptual components.* Neither the telescopic phenomena nor the new ideas of motion were acceptable to common sense (or to the Aristotelians). Besides, the associated theories could be easily shown to be false. Yet these false theories, these unacceptable phenomena, are distorted by Galileo and are converted into strong support of Copernicus. The whole rich reservoir of the everyday experience and of the intuition of his readers is utilized in the argument, but the facts which they are invited to recall are arranged in a new way, approximations are made, known effects are omitted, different conceptual lines are drawn, so that *a new kind of experience* arises, *manufactured* almost out of thin air. This new experience is then *solidified* by insinuating that the reader has been familiar with it all the time. It is solidified and soon accepted as gospel truth, despite the fact that its conceptual components are incomparably more speculative than are the conceptual components of common sense. We may therefore say that Galileo's science rests on an *illustrated metaphysics.* The distortion permits Galileo to advance, but it prevents almost everyone else from making his effort the basis of a critical philosophy (even today, emphasis is put either on his mathematics, or on his alleged experiments, or on his frequent appeal to the 'truth', and his propagandistic moves are altogether neglected). I suggest that what Galileo did was to let refuted theories support each other, that he built in this way a new world-view which was only loosely (if at all!) connected with the preceding cosmology (everyday experience included), that he established fake connections with the perceptual elements of this cosmology which are only now being replaced by genuine theories (physiological optics, theory of continua), and that whenever possible he replaced old facts by a new type of experience which he simply *invented* for the purpose of supporting Copernicus. Remember, incidentally, that Galileo's procedure drastically reduces the content of dynamics: Aristotelian dynamics was a general theory of change comprising locomotion, qualitative change, generation and corruption. Galileo's dynamics and its

Ludovico Geymonat

THE COLLAPSE OF THE GALILEAN PROGRAM

1. In his *Dialogue Concerning the Two Chief World Systems* 136
Galileo had two aims: first, to arouse general interest in the problem of Copenicanism among cultured persons, even though they were not versed in astronomy, and to persuade them of the foolishness of the old Peripatetic science; and second, to educate the highest Vatican authorities to the dangers that the Catholic Church would encounter if it insisted arbitrarily in maintaining its attitude of 1616.

The first aim was quickly and overwhelmingly achieved; a unanimous chorus of praise from the widest circle of readers greeted this limpid and delightful book. Galileo received many letters in the early months from his friends and admirers:

"The title of the work, its dedication and preface to the reader, so excited my curiosity that before settng myself to the task of reading it . . . I could not resist skimming eagerly through . . . a part of the text, where there appear new theories and noble observations which you have reduced to such simplicity that even I, of a different occupation, am certain I shall be able to understand at least some parts of it."

"The clearness with which points are explained that seemed incomprehensible must be admired by everyone."

"You have succeeded with the public to a point at which no one else has arrived. . . . Frankly, who in Italy cared about the Copernican system? But you have given it life, and, what really counts, have laid bare the breast of Nature."

"Everything about it pleases me, and I see how much stronger your argument is than that of Copernicus, though his is fundamental. . . . These novelties about old truths, new worlds, new stars, new systems, new nations, and so on, are the commencement of a new age."

But things were to go very differently for Galileo's second aim. 137
In fact, only a few months later there was very discouraging news about the reception of the *Dialogue* in the most influential spheres of the Church. In a letter of June nineteenth, Benedetto Castelli wrote Galileo that he had heard that "Father Scheiner, being in a bookstore with an Olivetan father . . . and hearing that father give to the *Dialogue* the praise it deserves . . . became very agitated; his face changed color and his voice and hands shook so violently as to astonish the bookseller, who told me the story." Two months later Campanella wrote of having heard "with great displeasure . . . that a Congregation of angry theologians is being assembled to probe your *Dialogue*."

A more serious incident, and one which was to determine the entire course of the affair, was the unexpected switch of Urban VIII from the ranks of Galileo's friends to those of his most tions that were hard to reconcile with the responsibilities of the head of Christianity. Confronted by these threatening accusations, Urban VIII clearly perceived the precariousness of his apparent power and commenced to seek ways to bolster it. These were truly dramatic months; wherever he looked, he saw enemies, supporters of the hostile party, even traitors trying to poison him.

In this situation it is no wonder that he quickly gave ear to Galileo's enemies, and fancied that the main purpose of the author of the *Dialogue* was to undermine him with the cultured public. Hence his decision to be avenged, to punish Galileo, to regain his own prestige through the humiliation of the faithless friend.

The punishment of Galileo would also have two other indisputable advantages. On the one hand, it would prove that the Pope was strong enough to put down, through Galileo, his pro-Spanish protector, the Grand Duke of Tuscany; on the other hand, it would show the Catholic world that its leader could defend the true spirit of the Counter-Reformation even by sacrificing, in the supreme interests of dogma, the well-known personal links that had previously bound him to the author of the incriminating work. The condemnation of Galileo would thus in the end assume a new character, a more than personal significance, which made it a genuine political necessity: it "involved not only the personal resentment of the Pope, but the defense of his dignity and authority, and due respect for Catholic discipline and Church decrees; the zeal of mankind and that of the institutions of the Counter-Reformation; and Urban's unyielding will to power over culture and knowledge."

Ludovico Geymonat, *Galileo Galilei*, tr. Stillman Drake, McGraw-Hill, New York, 1965, pp. 136–139.

Olaf Pedersen

GALILEO AND THE COUNCIL OF TRENT: THE GALILEO AFFAIR REVISITED

The first task must be to provide what the Cardinal had asked for, a proof 12
that the Copernican system was true as a physical description of the structure of the universe and thus was more than a purely kinematical device for describing the relative motion of the celestial bodies and the Earth which would only save the phenomena in a merely formalistic or hypothetical way. In his letter to Foscarini Bellarmine had declared that there could be no objection whatever to presenting the system as such a mathematical hypothesis, adding that this was also the way in which Copernicus himself had presented it. This is a clear reference to an anonymous preface to the first edition of Copernicus's book; but it is sufficient to read its introductory chapters to realize that Copernicus himself did in fact consider his theory as a physical account of the universe. Today it is well known that Kepler had shown that this preface was not authentic, but written by the Lutheran professor Osiander who saw the book
through the Press.[40] But of this Bellarmine was unaware, and so was Galileo 13
who nevertheless consistently refused to satisfy himself with an hypothetical interpretation of a theory the material truth of which he was unable to doubt.

There is no doubt that Galileo in these first months of 1615 must have carefully assessed the status of the Copernican theory in the light of his own astronomical discoveries, and that he must have realized that, everything considered, they offered no convincing demonstration of the motion of the Earth. At most, they refuted some of the objections raised against the theory from a physical point of view; for instance, the objection that the Earth could not be a planet since it would then lose the Moon during its travel through space was no longer tenable after the discovery of the satellites of Jupiter. But this did not amount to a positive proof, and neither did the discovery of the phases of Venus since they could be explained on the basis of Tycho Brahe's geoheliocentric system—a fact which must have been known to Galileo although he always preferred to regard the Tychonian system as a despicable compromise. Other discoveries such as the mountains on the Moon, the spots on the Sun, and the nature of the Milky Way were clearly irrelevant in this connection.

At this point one cannot help wondering that Galileo did not look to Kepler's discoveries. In his library he had Kepler's *Astronomia nova* from 1609 in which the first and second of the laws of planetary motion were derived from Tycho's observations.[41] It is true that these laws are of a purely kinematical nature; but it is equally true that it is extremely difficult to connect them with the physical world except within the framework of a heliocentric cosmology. In consequence, a reference to these laws would have, if not proved this cosmology, at least made it extremely probable. But Galileo did not take refuge in Kepler. Perhaps he had not read the *Astronomia nova* with all its intricate mathematics and its jungle of numerical calculations.[42] Perhaps he did not only want to prove the Copernican system—but to prove it himself.

The proof which Galileo finally devised seems to have been ready towards the end of the year 1615 since he brought it with him to Rome where he arrived in December. He explained it to various audiences, pretending that it was a conclusive physical demonstration, and succeeded in persuading the young Cardinal Orsini to present it to Pope Paul v, in the hope that it might influence the supreme head of the Holy Office to look more favourably at Copernicus. The argument seemed impressive and would—if correct—not only prove the motion of the Earth but also solve the old problem of the cause of the tides. Galileo's explanation of this phenomenon was that the flux and reflux of the that Bellarmine would deem it imprudent to depart from the consensus of the Fathers and discard the literal sense of the Biblical passages in question. One possibility would be to maintain that these passages do not fall within the domain covered by the decree, since the motion or rest of the Earth is no matter of faith. But the Cardinal is able to block this way of escape in advance. In fact, he continues by saying that "Nor may it be answered that this [motion of the Earth] is not a matter of faith; for if it is not a matter of faith from the point of view of the subject matter, it is on the part of those who have spoken. It would be just as heretical to deny that Abraham had two sons and Jacob twelve, as it would be to deny the Virgin Birth of Christ, for both are declared by the Holy Ghost through the mouth of prophets and apostles". In other words, faith implies a perfect belief in the credibility of the inspired authors of the Bible with regard to all they have said. It would seem that the great Jesuit theologian comes very close to a purely fundamentalistic point of view. However, this is not the case, for in a following passage of the same letter (already quoted above) he does in fact admit that he would be prepared to reconsider his position if there were a real—that is, scientific—proof of the motion of the Earth. Such a proof would have to be accepted with the corollary that we would then have to admit that there are passages in Scripture the literal meaning of which we cannot understand.

7. *Galileo as a theologian*

This was the way in which the decree of the Council was understood by the most influential theologian of the Holy Office. In consequence, Galileo had to consider all the main elements of Bellarmine's position. We have seen already that he tried to provide a scientific proof which would be strong enough to make the Cardinal change his mind (the theory of the tides). But months before this alleged proof was ready he tried to tackle the theological arguments expounded in the letter to Foscarini. In his "Letter to Madama Christina" (v, 309–48)[47] Galileo tried to argue three essential points. First, that the matter had been brought to the Roman Court for reasons based on wrong premises;
second, that astronomical theories cannot be matters of faith; and third, that 17
there would be no difficulty in establishing a harmony between the new cosmology and the teaching of the Bible if the latter was interpreted according to ordinary exegetical principles of long standing in the tradition of the Church, but at variance with the literal sense of the decree of Trent.

• • •

After this attempt to explain to the theologians the difficulty and seriousness 20 **4.18**
of the task they themselves must perform at the impending process, Galileo at last plucks up his courage to state his position on what he very well knew was the heart of the matter—the exegetical decree of the Council of Trent: "Next let us answer those who assert that those physical propositions of which the Bible speaks always in one way, and which the Fathers all harmoniously accept in the same sense, must be taken according to the literal sense of the words without glosses or interpretations" (v, 330). This clearly is a reference to the decree. But the reference to the literal sense reveals that Galileo is here speaking of the decree in the restricted sense in which it was understood by Ludovico delle Colombe and others. This seems strange since one may safely assume that by now Galileo must have acquainted himself with the precise wording of the most important ecclesiastical document touching on the case. The explanation must be that he wished to deal with the more popular misinterpretations of the decree before he was prepared to consider the implications of its authentic doctrine.

Galileo begins this part of the discussion by carefully distinguishing between physical statements which are merely a matter of opinion (for instance, whether the stars are animate beings) and those which are based "on experiment, long observation, and rigorous demonstration" (for example whether the Earth is moving or at rest, or whether or not the heavens are spherical). The first type of statements cannot be based on human reason alone and here it must be decree is also confirmed by the fact that ordinary Council decrees are in general agreement with the Fathers since they are concerned with what is *de Fide*, but actually "abstain from enjoining us to receive physical conclusions as matters of faith, and from censuring the opposite opinions as erroneous" (v, 337). Finally, as an afterthought, Galileo remembers that the Fathers are not agreed on all astronomical questions in their interpretation of the miracle of the Sun in Joshua 10:12 seq. In fact, (Pseudo-)Dionysius thought that not only the Sun, but the *primum mobile* and consequently all the heavenly bodies stood still,[55] an opinion in which St Augustine concurred.[56] This is not really relevant here since Galileo has already shown that the decree covers only the cases where the Fathers agree.

The ultimate conclusion of this long and most important part of the treatise is that since "this particular dispute does not occur among the ancient Fathers, it must be undertaken by the learned men of this present age" (v, 338). This is spelt out in a long section in which Galileo tries to show that the miracle in Joshua may be interpreted as being compatible with the Copernican cosmology. There is no need here to go into this particular contribution to Old Testament exegesis. Galileo has already made his point with his analysis of the presuppositions and implications of the Tridentine decree, and the rest of the letter can be summarized in very few words: Do not condemn the Copernican system without examining it!

8. *Conclusion*

With his "Letter to Madama Christina" Galileo had entered a territory which was not his own and it is easy to point to some of the unavoidable shortcomings of his treatise, which seems to have been written in some haste and is much less systematically composed than his other writings. It clearly was the work of an amateur in theology who knew what he wanted to do but not quite how to do it.

Thus Galileo rests much of his case on quotations taken from patristic sources which are used in a rather selective way. His principal authority is the *De genesi ad litteram* by St Augustine from which he culls at great length. In addition St Jerome is quoted on more than one occasion.[57] But this is about all, apart from the single reference to Tertullian.[58] The Greek Fathers seem to lie outside his horizon if we ignore the references to Pseudo-Dionysius. Thus there is no systematic or comparative analysis of the exegetical principles of patristic times. Galileo satisfies himself with picking somewhat at random a small number of statements which can be used in support of his case.

From the scholastic period Galileo only quotes St Thomas Aquinas,[59] and from the later Middle Ages and Renaissance there are references to Paul of Burgos (v, 337), "Abulense" (*i.e.* Bishop Alfonso Tostado of Avila; v, 337 and 344), and Cajetan (v, 347). Among contemporary authors Galileo quotes Pererius,[60] and Diego à Stunica (v, 336), and has a stray reference to a commentary on Joshua by the Portuguese Jesuit Magellan. Non-Christian writers are represented only by Josephus (v, 337). These were Galileo's authorities, and it is worth noticing that he seems to have been unaware of the fact that both the motion of the Earth and the use of anthropomorphic language in the Bible had been dealt with at great length by Nicole Oresme whose work would have provided Galileo with a wealth of arguments, had he but known it.[61] Even more remarkable is the fact that he avoids any reference to the extremely radical cosmology of Cardinal Nicholas Cusanus whose high ecclesiastical position would have lent considerable support to Galileo's case. This is all the more strange considering the fact that about this time Foscarini (in an undated letter) drew Galileo's attention to Cusanus (xii, 216). The explanation of this omission may be that Cusanus's views had deeply influenced Giordano Bruno whom, of course, it would be better not to drag into this controversy.[62] He also passed over in silence the fact that his favourite metaphor of the Book of Nature had found its classical expression in the *Theologia naturalis* (1436) by Raymond Sebond since the introduction to this seminal work had been suppressed by the Council of Trent.[63]

arguments. But throughout the letter one can read between the lines a deep personal concern for the even graver risk the Church was taking by a mechanical application of and reliance on the exegetical decree of Trent—the risk of scandalizing the faithful by becoming unable to distinguish between important and unimportant questions in religion as well as in science. For when all is said and done, the routinely quick handling of the case through the offices in Rome clearly reveals that the "qualificatores" did not realize the seriousness of the matter. To them the Copernican hypothesis was a matter of very little importance in comparison with the apparent necessity of upholding a "literal" interpretation of the exegetical principles of the Council. This was their stance, and Galileo knew it. Hence his letter, and hence we must conclude that the "Galileo affair" was not about astronomy. It was about the Council of Trent.

Journal for the history of astronomy, vol. 14, no. 39, 1983, pp. 12-14, 15-17, 20-23, 23-24.

5

SCIENCE AS TEXT

P. B. Medawar

IS THE SCIENTIFIC PAPER A FRAUD?

Dr Medawar, Nobel prizewinner and former Reith lecturer, is Director of the National Institute for Medical Research, Mill Hill

I HAVE chosen for my title a question: is the scientific paper a fraud?* I ought to explain that a scientific 'paper' is a printed communication to a learned journal, and scientists make their work known almost wholly through papers and not through books, so papers are very important in scientific communication. As to what I mean by asking 'is the scientific paper a fraud?'—I don't of course mean 'does the scientific paper misrepresent facts', and I don't mean that the interpretations you find in a scientific paper are wrong or deliberately mistaken. I mean the scientific paper may be a fraud because it misrepresents the processes of thought that accompanied or gave rise to the work that is described in the paper. That is the question, and I'll say right away that my answer to it is 'yes'. The scientific paper in its orthodox form *does* embody a totally mistaken conception, even a travesty, of the nature of scientific thought.

The Traditional Form

Just consider for a moment that traditional form of a scientific paper (incidentally, it's a form which editors themselves often insist upon). The structure of a scientific paper in the biological sciences is something like this. First, there's a section called the 'introduction' in which you merely describe the general field in which your scientific talents are going to be exercised, followed by a section called 'previous work' in which you concede, more or less graciously, that others have dimly groped towards the fundamental truths that you are now about to expound. Then a section on 'methods'—that's O.K. Then comes the section called 'results'. The section called 'results' consists of a stream of factual information in which it's considered extremely bad form to discuss the significance of the results you're getting. You have to pretend that your mind is, so to speak, a virgin receptacle, an empty vessel, for information which floods into it from the external world for no reason which you yourself have revealed. You reserve all appraisal of the scientific evidence until the 'discussion' section, and in the discussion you adopt the ludicrous pretence of asking yourself if the information you've collected actually means anything; of asking yourself if any general truths are going to emerge from the contemplation of all the evidence you brandished in the section called 'results'.

Of course, what I'm saying is rather an exaggeration, but there's more than a mere element of truth in it. The conception underlying this style of scientific writing is that scientific discovery is an *inductive* process. What induction implies in its cruder form is roughly speaking this: scientific discovery, or the formulation of scientific theory, starts with the unvarnished and unembroidered evidence of the senses. It starts with simple observation—simple, unbiased, unprejudiced, naive, or innocent observation—and out of this sensory evidence, embodied in the form of simple propositions or declarations of fact, generalizations will grow up and take shape, almost as if some process of crystallization or condensation were taking place. Out of a disorderly array of facts, an

So in the process of deduction, discovery and proof can depend on the same process. But in scientific activity they are not the same thing—they are, in fact, totally separate acts of mind.

But the most fundamental objection is this. It simply is not logically possible to arrive *with certainty* at any generalization containing more information than the sum of the particular statements upon which that generalization was founded, out of which it was woven. How could a mere act of mind lead to the discovery of new information? It would violate a law as fundamental as the law of conservation of matter: it would violate the law of conservation of information.

In view of all these objections, it is hardly surprising that
Bertrand Russell in a famous footnote that occurs in his *Principles* 378
of Mathematics of 1903 should have said that, so far as he could see, induction was a mere method of making plausible guesses. And our greatest modern authority on the nature of scientific method, Professor Karl Popper, has no use for induction at all: he regards the inductive process of thought as a myth. 'There is no need even to mention induction', he says in his great treatise on *The Logic of Scientific Discovery*—though of course he does.

Now let me go back to scientific papers. What's wrong with the traditional form of scientific paper is simply this: that all scientific work of an experimental or exploratory character starts with some expectation about the outcome of the inquiry. This expectation one starts with, this hypothesis one formulates, provides the initiative and incentive for the inquiry and governs its actual form. It's in the light of this expectation that some observations are held relevant and others not; that some methods are chosen, others discarded; that some experiments are done rather than others. It's only in the light of this prior expectation that the activities the scientist reports in his scientific papers really have any meaning at all.

Hypotheses arise by guesswork. That's to put it in its crudest form. I should say rather that they arise by inspiration; but in any event they arise by processes that form part of the subject matter of psychology and certainly not of logic, for there is no logically rigorous method for devising hypotheses. It is a vulgar error, often committed, to speak of 'deducing' hypotheses. Indeed one does not deduce hypotheses: hypotheses are what one deduces things from. So the actual formulation of a hypothesis is—let's say a guess; is inspirational in character. But hypotheses can be *tested* rigorously—they are tested by experiment, using the word 'experiment' in a rather general sense to mean an act performed to test the deductive consequences of a hypothesis. If one formulates a hypothesis, one can deduce from it certain consequences which are predictions or declarations about what will, or will not, be the case. If these predictions and declarations are mistaken, then the hypothesis must be discarded, or at least modified. If, on the other hand, the predictions turn out correct, then the hypothesis has stood up to trial, and remains on probation as before. This formulation illustrates very well, I think, the distinction between on the one hand the discovery or formulation of a scientific idea or generalization, which is to a greater or lesser degree an imaginative or inspirational act, and on the other hand the proof, or rather the testing of a hypothesis, which is indeed a strictly logical and rigorous process, depending upon deductive arguments.

This alternative interpretation of the nature of the scientific process, of the nature of scientific method, is sometimes called the 'hypothetico-deductive' interpretation, and this is the view which Professor Karl Popper in the *Logic of Scientific Discovery* has persuaded us is the correct one. To give credit where credit is surely due, it is proper to say that the first professional scientist to express a fully reasoned opinion upon the way scientists actually think when they come upon their scientific discoveries—namely William Whewell, a geologist and, incidentally, the Master of

Joseph Gusfield

THE LITERARY RHETORIC OF SCIENCE

This paper is part of a larger study of how knowledge is used in strategies for the solution of public issues. I examine research papers on the issue of drinking and driving, treating the scientific document as a literary, artistic product. Principles of literary criticism, utilized in the analysis of narrative, drama and poetry are applied to the presentation of research to show how statements of fact are given scientific legitimacy and how the literary formulation transfers such statements into rhetorical prescriptions for action. Theorizing and conclusion-making are shown to involve presentational devices of literary selection and language which confer policy implications upon them.

PROLOGUE: WHAT IT'S ALL ABOUT

The Rhetoric of Research! The title imposes an obvious contradiction. Research is Science; the discovery and transmission of a true state of things. Rhetoric is Art. The Aristotelian definition defines it as "the faculty of observing in any given case the available means of persuasion" (Aristotle, 1941:1329). As such, Rhetoric is an art useful for the politician, the journalist, the speaker, the artist – the man or woman who seeks to move people to action. "It is chiefly involved with bringing about a condition, rather than discovering or testing a condition" (Bryant, 1965:18; Winterowd, 1968:14). It was the skill perfected by the Sophists and is associated with such non-scientific and nefarious processes as advertising, propaganda and politics. It is the artist who needs rhetoric to produce a deliberate effect in the audience. Art is Art and Science is Science and the twain shall not meet. Is it not to replace Rhetoric and Art that Science has come into the world?

It has been customary to distinguish efforts to persuade through language – the activity of the artist – or through logic – the activity of the scientist. Albert Hofstadter has made this difference between scientific and literary uses of language the crux of his distinction between the two functions of Art and Science. The literary artist, maintains Hofstadter (1955), uses language as a significant vehicle for his or her activity. *How* objects and events are described or explained is more important than the subject matter of the narrative or poem. For the scientist this is not the case. Language is only a medium by which the external world is reported. That which is described and analyzed is not itself affected by the language through which it is reported.

"Put generally, the scientist searches for *items which are involved with each other in patterns of dependence* . . . the scientist's language is not one of these items . . . he must not allow his language to become part of the content of his assertion.

"The character of the imaginative object achieved by the artist depends on the character of the language he employs, whereas the language of the scientist does not operate within the involvement pattern he formulates" (Hofstadter, 1955:294-5).

This is what I call the "windowpane" theory. It insists on the intrinsic irrelevance of

*This paper grew out of research conducted while on a Guggenheim Fellowship. I am indebted to the John Simon Guggenheim Foundation for support. I have also gained from the comments and advice of Richard Brown, Bennett Berger, Kenneth Donow, Paul Filmer, Frederick Jameson, Mary Johnson, Marcia Millman and Kingsley Widmer.

so purely artful. They also include the assertion that such analysis makes a difference, that the artistic side of science is a significant part of the scientist's display of the external world. Not only will I show the artistic side of science but I will use that exposition to understand the product—the theory, generalization or conclusion to which the work of the scientist has led. Most importantly, I will use it to shed light on the practical actions which emerge as prescriptive through this process of artful science.

My attention is not fixed on *the* Scientific Enterprise. It is both more modest and more searching. I narrow my vision at one set of windows—the research studies in the area of "driving while under the influence of alcohol." It is in the course of a larger book-length study of knowledge and policy in this area of social control that I was drawn to think about the literary qualities of scientific presentation. Certain aspects of that work led me to undertake a more careful reading of 45 major research papers which have been alleged to be the bases for much political, legal and medical policy toward "drinking driving" in the past 23 years, in Europe and the United States.

While I will allude to several of the papers in this area, one report has been chosen for a more thorough analysis. My method, especially in treating papers as narratives, is such that I need to analyze the framework of the paper as a unit. To do this for 45 papers is impossible in a short paper. Accordingly, I have resorted to a literary device common to science—the use of the synecdoche—a literary device of representation in which a part substitutes for the whole. I selected one article for thorough concentration, to represent the system of literary analysis applied to a scientific document. This paper (Waller, 1967), "Identification of Problem-Drinking among Drunken Drivers," was chosen for two conscious reasons: (1) It has been influential, frequently cited by other research persons and in governmental documents as a base for advocating particular policies. (2) It represents a number of studies and papers which, in recent years, have operated as persuasive elements in a transformation of strategy toward the control of auto accidents associated with alcohol use.

Enough procrastination. On with the play!

Act I: Scientific Style: The Rhetoric of Method

How does the scientist proceed to establish his/her claim to be "doing science" and thus to be read in a "scientific" way by the audience? In the frame of the windowpane, the scientist does not ask for that "willing suspension of disbelief" with which Coleridge maintained the theatre audience accepted a stage in London as the Italian balcony of Romeo and Juliet. How does the scientist, however, act as dramatist, setting a stage and persuading his readers to treat his/her work as one type of production rather than another?

In this act of my play, I am developing the literary *genre* of the scientific report and my topic is not its content but its form. I will make use of two literary modes of analysis. Following Kenneth Burke (1945), I will examine the *dramatistic keys* in use, examining the use of scene, act, agent, agency and purpose. The objective here is to "uncover" the placement of responsibility for describing the action described. With analysts of narrative fiction such as Henry James, Percy Lubbock (1957) and Wayne Booth (1961), I will utilize the concept of *voice* to explicate the relation of observer to observed and observer to audience which influences the point-of-view or stance of the writer toward his subject and his/her audience.

The Keys of Dramatism

What is the scenic surrounding of the paper? Scientific papers are not published randomly; they appear in a setting. The paper, "Identification of Problem-Drinking among Drunken Drivers," did not appear in *Playboy* magazine, in a collection of American short stories or in a work on freshman composition. It is "placed" in the *Journal of the American Medical Association* (Waller, 1967). That setting establishes a claim for the paper to be taken as authoritative fact and not as fiction ducts, his/her claim to veracity is not an essential part of the claim to artistic acceptance. James Joyce need not be accepted as a reliable producer of accounts of Dublin in the early 1900s in order to appreciate *The Portrait of the Artist as a Young Man*. The reader need not even know the author to appreciate the novel. But where the author attests to a world of real properties, his/her integrity and competence to report is a question.

The dilemma between personalizing and removing the agent seems to be solved in all but one of the 45 papers by a device of identification through role. In this paper, following the title appear the name and credentials of the author: Julian A. Waller, M.D., M.P.H. At the bottom of the page, in footnote form, the author is described as someone connected with an organization: "From the Bureau of Occupational Health, California Department of Public Health. Dr. Waller is now with the Bureau of Chronic Diseases" (p. 124). Thus the agent is described in a role (medical and public health) and in an organization.

Having told the audience about his professional competence and acceptance, the author must now move out of the limelight if the document is to be untainted by the obvious presence of the observer. The language chosen performs this function through an emphasis on the externality of the source of action and through the passive character of the agent. *Viz*: "It was decided to use this latter method" (Schmidt and Smart, 1959:632); not: "We decided. . . ." "The test indicates there is a significant difference" (Borkenstein et al., 1964:188); not: "Based on the test, we concluded. . . ." "Data . . . confirm a self-evident fact" (Holcomb, 1938:1084) and "Recent reports have suggested . . ." (Waller, 1966:532) are other examples I culled from a variety of the papers studied.

In Waller's paper (*Nota bene*: I have taken to personalizing the product), such circumlocutions are frequent. The active voice is absent. In the lead sentence the author (by inference) writes: "It is increasingly becoming apparent . . . ." But to whom? Throughout the paper the conclusion or result is portrayed as emerging from an external world of data or tables. "Differences were found . . . ."; "This finding necessitates the reevaluation."

Agency

What this pattern of rejection of personal terms or active voice does is to place the source of action in the agency or method. Waller's paper creates a style consistent with "windowpane" theory by establishing a reality outside the observer. The style reinforces this externality and provides the basic epistemological assumption; by use of the same method different observers must reach the same conclusions.

Both the identification of the author in the beginning and the passivity of the style support the portrayal of a procedure in which the observer is governed by a method and by the rules of scientific integrity in relation to the method. He continues to make this point throughout the paper by following a regimen of meticulous attention to details and thereby avoiding a judgment by the reader that he has been less than scrupulous in following the method. Thus the percentages are given in decimals, such as 19.3% or 6.1% for samples ranging from 150 to 19 (Table I, p. 125). Where discretion had to be used, the event is meticulously described to avoid the implication of whimsy or bias:

> One nondrinking driver involved in an accident also appeared in the drunken-driver sample. *For purposes of analysis* [italics mine] he was considered as a separate person in each group (p. 125).

Purpose

That the author means to persuade his audience of certain conclusions is both evident and explicit. The importance of method substantiates the overall style of detachment. He means to persuade, but only by presenting an external world to the audience and allowing that external reality to do the persuading. Thus the language must be emptied of feeling and emotion. The tone must be clinical, detached, depersonalized. His language must not be "interesting," his descriptions colorful or to say: "I will give you, the reader, all the knowledge and factual information that I have. We will reason together and achieve a consensus through fact and reason. You, as a rational person, cannot *but* reach the same conclusion as I."

This ratio of author to audience can, of course, be distinguished from others where the ratio is more or less than unitary equivalence. When the author tells instead of showing, he claims authority and distance from a viewpoint above the reader, in command of greater skill or special knowledge, as a scientist addressing a lay public or as Joseph Fielding writing in his novels. When the opposite is the case, the author shows the reader and leaves to him or her to make of it what he or she wishes, as in a Pirandello play or in some modern forms of ethnography. Even in our sample of papers, we found one or two instances where the author presented a set of findings and would not interpret or order them into a set of conclusions (Gerber, 1963).

Distance and Subject

Who is the author in relation to the subjects he studies? Is he one of them? Is he decidedly not among those he describes? What is his stance and distance toward the object of study? The endless discussions of objectivity and political morality in science since Marx have continuously posed the questions of point-of-view as a major one for the ethics of the scientist. Recently Robert Merton has posed this question in spatial terms by referring to "insiders" and "outsiders" (Merton, 1972). The problem is analogous to that of distance in literary analysis.

The clinical style of Waller's paper preserves the stance of the outsider by looking at drinking drivers as a group of whom the author, and thus the audience, is not a part. He has neither loyalty nor economic interest in them. They are "objects of study" and not members of the audience. His paper is not addressed to drinking drivers. Nowhere are any of the sampled groups referred to as including the author or the audience. Both author and audience are presented as "outsiders." Not drinking drivers themselves, they take the stance of observers of the non-self.

Is the stance one of equivalence? In the first paragraph, the author speaks of the need to reevaluate "current methods for preventing driving after drinking" (p. 124). Throughout the paper there is no doubt whose "side" he is on. nor is there any evidence of an effort to persuade the audience of the wisdom of this "side." It is taken for granted. The author, and inferentially the audience, are superior to the subjects in that they are distant from and above the behavior of drinking and driving.

The stance or viewpoint of the author is one of equivalence with his audience but superiority to his subjects. As an appeal to the persuasive power of reason, it is the style of Science to minimize Rhetoric, to negate and downplay evidence of viewpoint. Wayne Booth's characterization of fiction is inconsistent with the "windowpane" theory of Science:

> In short, the author's judgment is always evident to anyone who knows how to look for it .... As we begin to deal with this question, we must never forget that though the author can to some extent choose his disguises, he can never choose to disappear. (Booth, 1961:20)

You, the reader, and I are now at the end point of Act I, where the playwright reveals his dialectical hand. By now you have probably suspected what is the case. I will begin to assert that A is non-A, that what Wayne Booth has written about Fiction, I will assert about Fact. Art and Rhetoric have not been sent into perpetual exile to live outside the walls of Science and Knowledge. With or without passport, they steal back into the havens of clinical and antiseptic scholarship and operate from underground stations to lead forays into the headquarters of the enemy.

CURTAIN

END OF ACT ONE

rows the range of matters connected with the events being studied. The language used leads toward one particular channel of narrowing and away from others.

The concept of the "drinking driver" emphasizes the agent and minimizes the scene or the act or the agency as possible elements in accidents. "Drinking driver" imputes, as I see it, an attribute of selves. There are drinking drivers and non-drinking drivers as there are male and female drivers, old and young drivers, competent and incompetent drivers. Both "drinking driver" and "drunken driver" lead to the search for attributes of the person which exist and extend before, during and after the action of driving. Even within the terminology of "driver" other circumlocutions in use, though less frequent, direct attention toward aspects of the driving situation. "The alcohol-impaired driver" or the "intoxicated driver" place the driver in a context and make the extensiveness of the attribute less certain and more ambiguous.

It is possible also to dispense with the "driver" as an object and to describe the "same" phenomena with terms which indicate placement or scene or act. The phrase "persons engaged in drinking driving" or even "drinking driving" contrast with "drinking driver" by underlining the situational character of the event being examined.

These differences between "driver" and "driving" are not random choices of grammar. They reflect the significant perspectives of psychology and sociology, respectively—the difference between a drama of agent and a drama of scene. In his title and in his opening sentence, Waller has pulled the audience into the perspective of psychology and into a search for abiding characteristics of the personalities of persons.

Metaphor: The Transformation of the Drinking Driver

Waller's paper is one of several major research studies which have resulted in a transformed perspective toward the drinking driver in governmental and legal circles. His study design influences which of two major perspectives should be utilized in thinking about drinking drivers and in developing policies to minimize drinking driving. These perspectives are expressed through two central terms or metaphors: the *social drinker* and the *problem-drinker*. What is happening in this paper can be expressed as the dramatic re-conceptualization of the drinking driver from the metaphor of the social drinker to the metaphor of the problem-drinker.

I refer to these as "metaphors" because they are used to extend the meaning of primary data. They are not descriptions of the factual information collected but are instead presentations of that data in a form which creates linkages to something already known by the audience. They heighten perception by extending the primary data into another realm. Utilizing Max Black's interaction theory of metaphor (Black, 1962), Mary Hesse has stated the view of metaphor I used here:

> The metaphor works by transferring the associated ideas and implications of the secondary to the primary system. These select, emphasize or suppress features of the primary; new slants on the primary are illuminated; the primary is seen through the frame of the secondary. (Hesse, 1966:232).

It is the major conclusion of Waller's paper that, contrary to the then conventionally held view, "a substantial proportion" of drinking drivers are problem-drinkers and *not*, as formerly believed, social drinkers. A "large" number of drinking drivers also had arrest records involving the use of alcohol or had been diagnosed by one or more community service agencies as having a problem involving alcohol use. Such records and/or diagnoses were found only among a "small" proportion of the non-drinking drivers.

It is again the first paragraph in which the author tells his audience the nature of the two types found among drinking drivers: the drinking driver *qua* social drinker and the drinking driver *qua* problem-drinker. Present methods for diminishing drinking driving as-drinkers" and "problem-drinkers." All of these are types with which there is familiarity, in actual or vicarious experience, or both. They constitute stock themes in popular literature and drama as well as in news and common talk. The idea of the "killer drunk" is one such character whose irresponsibility and commitment to an hedonistic style of life creates tragedy for others. The very use of the term "drunken" in the title constitutes an invocation of the theme in the context of persons who have come to official attention equally through arrest for drinking driving or through accidents in which the offense is uncovered. So too, the usage, "social drinker," carries an implication of contrast to "drunken." Suppose Waller had referred to "problem drinkers" and "social drunks"? In this alternative usage, he would have referred to that category of persons who, on occasion, drink to drunkenness but whose action does not express an addictive problem. This behavior is quite common in a large segment of Americans (Cahalan et al., 1969).

The typology thus operates to label and stigmatize the drinking drivers as "problem-drinkers" and to exonerate and label the "social drinkers" as responsible citizens who have slipped but whose dereliction is not a reflection of a personal flaw. The root term of "normal" and "pathological" continues to place these groups in the image of the archetypal forms. The drinking drivers are analogized to the problem-drinkers and characterized within the terms of the myth of the drinker as deviant, outcast and stigmatized. "Engulfed in the deluge of alcohol-related problems" they are described in the following way:

> The central theme in the lives of the drunken drivers seems to be alcohol. Almost three quarters of their many arrests involved drinking. Their marriages often were in a state of dissolution because of excessive drinking. Among drunken drivers, arrest reports commonly observed that the person had been arrested for assaulting his wife when he arrived home intoxicated and she began scolding him for his alcoholic pattern. (p. 129)

Waller has given the audience a strong depiction of the stock figure of the drinker as deviant. Agency workers, whose records Waller utilized, had found drinking problems in only one-fourth of the cases and made a medical judgment of alcoholism in only one-tenth. Waller explained this contradiction to his findings by saying that the agency workers had used the stock image of gross intoxication and the "skid row bum." Nevertheless, his portrayal comes close to that stock figure. Writing of the sample of persons with arrest warrants out for failing to answer citations in non-drinking, moving violations, Waller writes that they "*also* [italics mine] represent a population with profound psychosocial pathology" (p. 129). In analyzing the drinking drivers, he refers to a subtype as "sociopathic." Summing up, the author says that "it is not possible to escape the conclusion that this group of persons does not learn from a punitive experience" (p. 129).

If the problem-drinker has been stigmatized as responsible for drinking driving and the drinking driver stigmatized as a deviant drinker, in this process the "social drinker," the "solid citizen" of conviviality, has been "taken off the hook" and absolved from deviance. The contrasting archetype—the social drinker—is an "erring driver" when he strays. He is a man of rationality and basic good will who "can be dissuaded." Such men are not very likely to be drinking drivers. When he is the drinking driver, a "social drinker *might* [italics mine] err once in the excessive use of alcohol sufficient to result in his arrest but he would not be likely to repeat his indiscretion" (p. 126). If not exactly heroic, the social drinker is neither villainous nor venal.

The implicit use of stock forms has enabled the author to produce a morality play in which drinking driving is an arena for the expression of personal and moral character. The social drinker is Everyman—rational, socially responsible, given to occasional and human lapses of conduct but basically law-abiding, controllable and controlling and responsive to norms of social cooperation. The Boy Scout of the highways, he can be trusted to carry out the dictates of a rational and inter-

ACT III: THE RHETORIC OF SOCIAL HIERARCHIES

The drama of drinking driver research, especially as exemplified in Waller's study, may now be seen as a dialectical process in which the main actor—the drinking driver—has been transformed from acceptable social drinker to stigmatized problem-drinker. In what follows, I assert that this transformation also involved the disestablishment of one form of social hierarchy and its replacement by another. Through the medium of research as drama, the phenomenon of hierarchy is itself re-presented and emerges as something different from its appearance at the opening curtain.

The Hierarchy of Drinkers as Social Hierarchy

The analysis of drinking drivers and their drinking patterns, as signified by Waller's study, has provided the audience with a shift in the hierarchical character of the major actors. In this shift the social drinker has been "upgraded" by being exonerated from the charge of responsibility for auto accidents. The "drinking driver" has been "degraded" by being equated with the "problem-drinker." That very equation further stigmatizes the already labeled deviant status of the "problem-drinker." As the play has been acted out, what was down has come up and what was up has come down. The social drinker regains the aura of Everyman while the drinking driver is now "pathological"—marginal and deviant. In this fashion the gap between law-abider and law-avoider has been widened. The social drinker has moved up the hierarchy of deserved esteem while the drinking driver has, in a veritable *double entendre*, been "put down" (Burke, 1945).

This distinction, and its correlative hierarchy, is also one that is congruent to social structure. It is invested with hierarchies of class, race and ethnic diversities. Akin with many recent studies of drinking driving, Waller does more than locate the drinking driver on a spectrum of drinkers. He also locates the drinking driver, *qua* problem-drinker, in the social structure of American society. Consistent with other studies of the drinking driver, Waller's supports the view of drinking drivers and problem-drinkers as resident in the lowest income and status levels (Cosper and Mozersky, 1968; Hyman, 1968). He reports that Negroes comprised 49% of the drunken drivers but only 25% of the driver population of Oakland, site of the study. Drivers of Mexican or American Indian descent were 11% of the drunken drivers, but 2-4% of the total population of drivers. Even though qualified, the image of the low status of the drinking driver emerges. In discussing information about arrest records, Waller writes:

> Nonwhite drivers consistently had larger proportions with arrests, and had more arrests per person. However the differences were not significant at $P < 0.05$ except for drivers of Mexican or Indian extraction with violations. (p. 128)

In an earlier companion study (Waller and Turkel, 1966), the authors found a statistically significant difference based on race. Among auto fatalities, Negroes were more likely to have had alcohol present in the blood than were whites and were also more likely to have a record of arrests for public intoxication or evidence of cirrhosis of the liver or both. Blood alcohol levels were higher among drinking whites than among drinking blacks, however. Commenting on these findings the authors remarked:

> There is reason to believe that the predominant subculture among American Negroes is fairly tolerant toward the use of alcohol, explaining the observed differences. However, heavy drinking is not over-represented among Negro fatalities. (Waller and Turkel 1966:535)

Note that in both papers the image of the Negro as problem-drinker is introduced and then qualified, rather than being presented directly as a qualified or ambiguous image.

What has happened is that the problem of drinking driving has now been located at the troubles stem from his indulgence and lack even the sense of being the outcome of otherwise laudable motives carried to excess, as Macbeth "o'erleaps" his own ambitiousness.

What deeply distinguishes this drama from many literary tragedies is its *"deus ex machina"*—the intervention of the author, the audience and the profession of alcoholism treatment. An array of hope, in the form of counselors, screeners and practitioners, is available to redeem the pathetic drinker through the vehicle of therapy. This Utopian strain of social engineering lifts the drama above the mood of despair and finality which high mimetic tragedy entails. No agency for the blind can help Oedipus nor can old age assistance restore Lear.

Scientific Feeling: The Union of Form and Substance

I have now pushed myself into a patent contradiction. The style of a non-emotive language has created a whole bucketful of emotions—comic, tragic, pathetic. But the language of science is not quite that of literature and the arousal of feeling is much more ambiguous here than on the stage. While Waller describes the problem-drinker he does not tell us, the audience, how we are to feel toward that central character. When I call the drinking driver "pathetic," I do so by reference to my feelings aroused by the paper and those I expect the "ordinary" reader would have, as indicated by my experience with how such people as those likely to read Waller's paper have responded to the problem-drinker in other contexts and other places.

It is significant that the language through which the writer has tried to describe his characters is couched in the non-emotive forms already described above, rather than the more direct phrases with which a novelist or playwright might try to arouse feeling in his readers. It is the muting of feeling that is itself the characteristic mood, emotion or feeling of the paper, viewed as a literary document. In this is its author's stance toward the object of study—the drinking driver—and whatever feeling he might otherwise arouse in the reader. The drinking driver stands as an object outside the emotional ambit of the writer and the reader. In this sense, pathos is to be checked, limited and even obliterated as a reaction of the audience.

It is important that the only clear use of emotional language appears in the paper when the author describes not the actors—the drinking drivers—but those officials who create policy toward the actors. In the quotations above, I the author emerge from hiding to score past policy as a "pathetic monument" and to view it with "utter amazement." Here is anger, scorn and irony.

The avoidance or limitation of feeling provides the writer, and therefore attempts to persuade the reader, with the necessary accompaniment to his identification with the "society" as victim. To see the "problem-drinker" in highly differentiated or individual terms or to view him as an object of emotional concern would make the problem of drinking driving less clear and the objective of social control more problematic. To be punitive, as the law has been according to Waller, or benevolent, as an "underdog sociology" might entail (Becker, 1967; Gouldner, 1968), would be to adopt a very different stance: less functional, less efficient, less concerned with rational maximization of benefits and minimization of costs. Waller's critique of law and his support of medical therapy is an argument based on effectiveness for social functioning. The drinking driver is neither villain nor hero. He must be helped because he creates "trouble" for other folks, such as his readers.

Form and substance combine. In placing the drinking driver downward in the social structure and in constructing him as a neutral object, control is enhanced. In order to consider the social costs and benefits, the reader and the policy-maker are cautioned to mute their feelings toward the specific and particular qualities—loathsome or appealing—of drinking drivers and see them as types and view them from the stance of the organization and the society. Both language and feeling, imagery and emotion, are those of Olympian hierarchy and organizational logic—*sans passion, sans irae.*

My friend answered, "Yes. His diction is terrible." As Schutz (1970:245-62) and more recently Goffman (1974) have both shown and told, there are many possible realities within which an event can be framed. At this level, the literary criticism of Science is a way of revealing other processes, other realities at work besides the "normal" method of scientific discovery and verification. What is important and significant at least in the analysis of the drinking-driver literature presented here, is that such an analysis heightens our recognition of the ways in which the objects—in this case drinkers and drivers—are being transformed in a dramatic presentation.

But at another level, represented by the sections analyzing the transformations of meaning in the objects of policy, the dramaturgical and artistic components of Science are not so consistent with the view of Science as positive knowledge. Examined reflexively, my own words carry a ring of skepticism toward the policies which are presented by Waller as flowing from an objective body of knowledge. It is not that the author is "wrong" in concluding that drinking drivers can be seen as problem-drinkers. It is that his interpretation involves theater—it involves a performance and a presentation which contains an element of choice and both enlists and generates a context, a set of meanings which give content and imagery to his data. The analysis of the document as a literary performance has revealed the human actions through which the transformation of the social drinker into the problem-drinker has occurred. It is not, at least in this analysis, that the data is challenged. What is at stake, however, is the necessity of the interpretation and the close connection between that interpretation and its form of presentation, its artistic element. It is in underlining the tenuousness and ambiguity of conclusions that I cannot blink at having called into question the certainty and stability of scientific interpretation. My perspective here is akin to Peter Gay's depiction of the confidence and "ideological myopia" of the Enlightenment *philosophes*:

> They never wholly discarded that final, most stubborn illusion that bedevils realists—the illusion that they were free from illusions. (Gay, 1966:27)

I bring this paper to a close with three suggestions of what is potentially available through a development of science as a literary genre. First, it heightens the self-understanding and awareness of what we, social scientists, do as we transform research into written reports, into words which have connotations in action as well as abstract knowledge. Second, it enables the analyst of social issues to appreciate and explore how he or she contributes to what Herbert Blumer (1971) calls "the process of collective definition" through which objective conditions are transformed into public problems and policies.

Last, there is a contribution which such analysis can make to public policy. In being attentive to the elements of language and choice involved in giving meanings to data, the analyst calls attention to the singularity and selective activity through which policy implications are drawn. In doing this, it becomes more likely that social scientists and others can create, explore and develop the potential variety of other interpretations and policies which would otherwise remain unnoticed and unavailable. It is this capacity to recognize the context of unexamined assumptions and accepted concepts that is among the most valuable contributions through which social science enables human beings to transcend the conventional and create new approaches and policies.

REFERENCES

Aristotle
1941 Basic Works. Tr. by Richard P. McKeon. New York: Random House.

Becker, Howard
1967 "Which side are you on." Social Problems 14:239-47.

Black, Max
1962 Models and Metaphors. Ithaca: Cornell University Press.

Turbayne, Colin
1962 The Myth of Metaphor. New Haven: Yale University Press.
Waller, J. J.
1967 "Identification of problem-drinkers among drunken drivers." Journal of the American Medical Association 200:124-30.
Waller, J. J. and Henry W. Turkel
1966 "Alcoholism and traffic deaths." New England Journal of Medicine 275:532-6.
Winterowd, W. Ross
1968 Rhetoric: A Synthesis. New York: Holt, Rinehart and Winston.

American sociological review, vol. 41, 1978, pp. 16–34.

Charles Bazerman

WHAT WRITTEN KNOWLEDGE DOES: THREE EXAMPLES OF ACADEMIC DISCOURSE

Knowledge produced by the academy is cast primarily in written language—now usually a national language augmented by mathematical and other specialized international notations.[1] Language, however, is not an inert vessel. The ancient philosophic and aesthetic debate over the relationship of form and content should caution us to consider the influences the languages of knowledge might have on the shaping of knowledge.[2] Recently linguistic interest in scientific language has produced several descriptions of the syntax of scientific prose in English (Huddleston; Gopnik; Lee).[3] Syntactical studies, however, are concerned only with the patterns of symbols stripped of context and meaning. To understand what language conveys we must look to the contexts in which language operates and to which language refers. Statements do things and talk about things. To put it more formally, we may say that documents serve specific functions within historical and social situations to continue, add to, and transform a group interaction.[4] In carrying on the interaction, nevertheless, documents—particularly knowledge-bearing documents—make representations of objects, actions, and knowledge that exist beyond confines of the interaction. Fleck, Kuhn, Popper, Toulmin, and Ziman have each developed a theoretical model defining the respective roles of social situation and reference to the objects of nature within scientific communications.[5] More recently,

* Fred Baumann, Robert Merton, Norman Storer, Harriet Zuckerman, and members of the Seminar in the Sociology of Science at Columbia University deserve credit for their extensive comments and suggestions on an earlier version of this paper. Responsibility for errors and opinions remains, of course, mine.

1 The limitation of this paper to consideration of the formal printed documents that comprise the permanent record of knowledge excludes consideration of the significant role of informal communication—both spoken and written—in the creation and dissemination of knowledge. Within limited communities informal communication may even serve as the primary channel of publication: informal communication also seems to influence citation patterns (and perhaps patterns of cognitive influence) in formal printed publications. See Diana Crane, *Invisible Colleges*, Chicago 1972; and Donald Edge and Michael Mulkay, *Astronomy Transformed: The Emergence of Radio Astronomy in England*, New York 1976. On the other hand, it may be argued that because talk and other informal communication rely on the prior literature of the field and are aimed at the eventual production of a new document, informal communication must be understood in relation to formal publication. The work of sorting out the full set of relations between formal and informal communication remains to be done.

2 The cognitive consequences of the advent of written forms of language are explored in Jack Goody, *The Domestication of the Savage Mind*, Cambridge 1977; and Eric Havelock, *The Greek Concept of Justice*, Cambridge, Mass. 1978, and *Origins of Western Literacy*, Toronto 1976. The cognitive consequences of the advent of printing are explored in Elizabeth Eisenstein, *The Printing Press as an Agent of Change*, 2 vols., Cambridge 1979.

3 R. D. Huddleston, *The Sentence in Written English*, Cambridge 1971; Myrna Gopnik, *Linguistic Structures in Scientific Texts*, The Hague 1972; and Lee Kok Cheong, *Syntax of Scientific English*, Singapore 1978.

4 See Ludwig Wittgenstein, *Philosophical Investigations*, New York 1953; J. L. Austin, *How to do Things with Words*, Cambridge, Mass. 1962; and John R. Searle, *Speech Acts*, Cambridge 1969.

5 Ludwik Fleck, *Genesis and Development of a Scientific Fact*, Chicago 1979; Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Chicago 1962; Karl R. Popper, *Objective Knowledge*, Oxford 1979; Stephen Toulmin, *Human Understanding*, Princeton 1972; John Ziman, *Public Knowledge*, Cambridge 1968.

of persuasion attempted, in the structuring of the argument, and in the charge given by the author to the readers (i.e., what the author would like the readers to do after being convinced by the article).[11]

Finally, the author is represented in several ways within the text. The human mind stands between the reality it perceives and the language it speaks in; statements reflect the thoughts, purposes, observations, and quirks of the individual. The individual can be seen in the breadth and originality of the article's claims, in the idiosyncracies of cognitive framework, in reports of introspection, experience, and observation, and in value assumptions. These features add up to a persona, a public face, which makes the reader aware of the author as an individual statement-maker coming to terms with reality from a distinctive perspective.

Although the four contexts (and the features that indicate them) are separated here for analysis, they are mutually dependent in each text. An observation concerning one has implications for the others. The depth of the interdependence is evident if one considers that the perception and thought of both author and audience are shaped for the most part by the same literature, and that literature provides the accepted definition of the objects discussed. On the other hand, shared interest in and observation of objects of study draw the literature, author, and audience together.

An author, in deciding which words to commit to paper, must weigh these four contexts and establish a workable balance among them. A text is, in a sense, a solution to the problem of how to make a statement that attends through the symbols of language to all essential contexts appropriately. More explicitly, an article is an answer to the question, 'Against the background of accumulated knowledge of the discipline, how can I present an original claim about a phenomenon to the appropriate audience convincingly so that thinking and behaviour will be modified accordingly?' A successful answer is rewarded by its becoming an accepted formulation.

Each of the contexts, when abstracted from the writer's task of embodying complex meaning in a specific text and when viewed singly as a theoretical problem in communication, can appear to raise overwhelming epistemological difficulties. The kinds of difficulties that arise from such monochrome analysis are suggested by a slight renaming of the four factors we have been considering: language and reality; language and tradition; language and society; and language and mind. Exclusive concern with the language-creating mind leads to a subjective view of knowledge which makes uncertain the reality perceived and which rejects the cognitive growth of cultures. Viewing in isolation the effect of tradition on statement-making may lead one to misjudge accumulated statements—whether called paradigms or authority—as juggernauts, flattening out observed anomalies and individual thought. Perceiving statements only within the process of social negotiation of a socially constructed reality ignores the individual's powers of observation and language's ability to adjust to observed reality. But the most common errors arise from language considered only in relation to reality: on one side the naive error of assuming that language is an unproblematic reflection of reality, and on the other side the sophistry that language is arbitrary, radically split from nature, with no perceiving cognitive selves and no trace of rational community to heal the split.

The three texts examined below represent three different solutions to the problem of writing knowledge: James Watson and Francis Crick, 'A Structure for Deoxyribose Nucleic Acid'; Robert K. Merton, 'The Ambivalence of Scientists'; and Geoffrey H. Hartman, 'Blessing the Torrent: On Wordsworth's Later Style'. The different balance of contexts established in each article derives in part from the differences in contexts—different types of objects studied, differently structured literatures, audiences of differing homogeneity, and different role expectations for the authors. The origin of the papers in separate fields (molecular biology, sociology, and literary criticism) representing the three traditional divisions of the academy (sciences, social sciences, and humanities) of course accentuates the differences on all fronts; however, these examples should not be over-read as typical of large divisions of knowledge. They represent only three spots on the map of knowledge, and it is as yet unclear where on the map they lie, or even what the map looks like.

11 Latour and Woolgar, and Knorr (see note 6) seem most interested in the persuasive and other effects texts have on their audiences; the process of text creation is seen to have the primary goal of persuasion. In this they follow Joseph Gusfield, 'The Literary Rhetoric of Science', *American Sociological Review*, 41, 1976, 16-34.

nucleic acid identifies elements of structure—e.g., the ribose configuration without an oxygen—as well as letting us know that the substance is to be found within cell nuclei. Thus the name is in this case overdetermined with respect to reality; we know more about the substance than we need to for purely identification purposes. 366

At this point we can see how the accumulated knowledge of the field (represented by the literature) is incorporated into the language. The isolation of elements and the theory of chemical combination, as well as the idea that substances can be analyzed chemically, are all implicit in the name of the object. More than that, the name reveals the gradually emerging orientation of chemistry to describe most features and processes through structure. Even the linguistically oldest component of the name, *acid* has been transformed through redefinition as chemical knowledge and orientation have changed. In Bacon's day the word *acid* meant only sour-tasting; then it came to mean a sour tasting substance; then, a substance which reddens litmus; then, a compound that dissociates in aqueous solution to produce hydrogen ions; then, a compound or ion that can give protons to other substnaces; and most recently, a molecule or ion that can combine with another by forming a covalent bond with two electrons of the other.[16] The tasting and taster vanish as the structure emerges.

The task of assigning a structure relies on a further assumption, that nature arranges itself in geometrical ways; theories of forces account for this remarkable correspondence between the symbolic representation of geometric shapes and the repeating arrangement of matter in nature. Geometry as a study is the product of human consciousness, but geometric forms are claimed to preexist human invention. Thus the task of the molecular biologist is not to create a structure that approximates nature, but to discover and express in human terms the actual structure resulting from all the forces and accounting for the behaviour and appearance of the molecule. The claim of representing an actual structure rather than creating an approximate model results in a strong requirement for correspondence between data and claim. This correspondence, as we shall see below, is the main criterion of persuasion offered to the audience.

The few words of text discussed so far convey much about the object and the knowledge developed through the history of chemistry and biology, yet such compact transmission of information reveals no literary genius on the part of the authors. The dense communication is inherent in the names of objects and tasks. That a mere naming of parts conveys such precise and full meaning indicates how much the historical genius of the discipline is embodied in the development of its language.

The analysis of the first sentence is not yet finished. The first five words, 'we wish to suggest a . . . ,' reveal much about the joint persona and contribution of the two authors. Despite the usual convention of avoiding the first person in scientific papers, the authors do assert their presence through the word *we*. That direct presence, however, is immediately subordinated to the object under consideration, the structure of D.N.A. Moreover, the authors are only *suggesting*, and the suggestion has only an indefinite article; whether *a* suggestion turns out to be *the* structure depends on nature. *Wish to suggest* is a form which implies humility before the facticity of the object, yet the phrase also has the boldness of the authors' presumption that their claim indeed will be confirmed by nature. Mild speech is possible because the suggestion will gain all the force it needs from the observation of reality; nature will stand up for scientists. The locution *wish to suggest*, appropriate here, might sound pompous in a branch of knowledge which does not find such immediate confirmation in nature.

Science will as well stand up for scientists, for the authors also subordinate themselves to scientific knowledge as currently constituted. By identifying their subject within the language of scientific disciplines, they are implicitly putting their original contribution within the framework of existing scientific knowledge. The placement and titling of the paper itself suggest how much the originality of the paper is subsumed within a highly structured framework of knowledge. The article is within a section entitled 'Molecular Structure of Nucleic Acids' and is followed by another article of the same class, 'Molecular Structure of Deoxypentose Nucleic Acid'.[17] The Watson-Crick article discusses 367

16 *Oxford English Dictionary*, compact edition, New York 1971, p. 20; *Webster's New Collegiate Distionary*, Springfield, Mass. 1953, p. 8; *American Heritage Dictionary*, Boston 1976, p. 10.

17 *Nature*, 171 (April 25, 1953), pp. 737, 738.

presence through the symbolic systems of words and numbers, but the symbols are more than approximate metaphors. The names point to discrete objects, and the geometry is of nature itself. Scientific language, as a symbolic system with a commitment to reform itself in accordance with replicable observation of nature, becomes more than an arbitrary symbolic system.[18]

After this long description of the model, only brief mention is made in paragraphs nine and eleven of the evidence in hand that confirms the model and the evidence still needed to provide a rigorous test. Acceptance of the model depends on the confirming evidence; therefore, the sketchiness of the discussion of evidence might seem surprising. But once the model is described, the existing evidence needs only be referred to because it is generally available and can be interpreted by any competent molecular biologist. Similarly, the construction of new tests is within current technology. The other researchers must satisfy themselves that the model fits past evidence and new tests. It is up to nature to persuade the readers, not the authors.

Just as the ninth and eleventh paragraphs present only limited persuasion, the tenth paragraph presents only limited guidance to the readers about how the model might be applied. The comment that the model is probably not applicable to R.N.A. may be primarily to eliminate R.N.A. as a competitor for the biological slot of genetic carrier (as was then thought more likely than D.N.A.).

After mentioning the genetic implications of the structure, the paper has finished its primary scientific business. The thirteenth paragraph promises greater detail in later publication. This later publication primarily was devoted to spelling out the genetic copying mechanisms.[19] Nonetheless, it is this first short article that counts as the primary statement of knowledge and is the one usually cited.

The last paragraph pays its respects to some aspects of the social system of science: prepublication criticism, access to unpublished evidence and ideas, and funding. To those who know the history of this discovery, these few thanks and the earlier criticisms of competitive work recall a web of social intricacies and inchoate psychological reaching toward discovery.[20] These prepublication facts of life are recognized by working scientists as necessary preconditions of publishable work; nonetheless, these preconditions of discovery do not enter the actual argument of the publication. In the article, competition is dealt with only in cognitive terms, discovery is presented as a fait accompli, and the social system is appended only as a courtesy, a polite nod at the end.

Dependence on the community of the discipline is even more fundamental in the language used, the prior knowledge, and the accepted perception of the object of study, yet even this cognitive dependence on the scientific community is not given explicit recognition. The article cites only work immediately relevant to the assessment of claims made in the article. The six footnotes document only articles presenting competing claims that were criticized or offering supporting data.

In order to maximize the tightness of fit between nature and its symbolic representation, all the relations between language and other contexts—the literature, the audience, and the authors—are both harnessed to and driven by the relationship between language and nature. Society, self, and received knowledge are present in the research report, but they are subordinated to the representation of nature. The criterion of correspondence between statement and object governs all of the contexts.

18 Harriet Zuckerman, 'Cognitive and Social Processes in Scientific Discovery: Recombination in Bacteria as a Prototypical Case' (unpublished manuscript, 1974; revised 1975), discusses the resistances to discovery created by misleading names and the processes by which definition is corrected through discovery. The inaccurate naming impedes, but does not prevent, discovery; ultimately observation of the object leads to corrected knowledge. In the case Zuckerman studies, 'bacteriologists believed that bacteria were asexual *by definition*' (emphasis hers) because bacteria were classified as schizomycetes, from the Greek meaning 'fission fungi' (p. 8). In 1946 Joshua Lederberg's discovery of sexual recombination in the bacteria *E. coli.*, however, led to a revised definition of the classification schizomycetes, despite the literal meaning of the etymology.

19 J. D. Watson and F. H. C. Crick, 'Genetical Implications of the Structure of Deoxyribonucleic Acid', *Nature*, **171**, May 30, 1953, 942-67; J. D. Watson and F. H. C. Crick, 'The Structure of DNA', *Cold Spring Harbor Symposia on Quantitative Biology*, **18**, 1953, 123-31; and F. H. C. Crick and J. D. Watson, 'The Complementary Structure of Deoxyribonucleic Acid', *Proceedings of the Royal Society*, **A223**, 1954, 80-96.

20 See note 14.

incorporating a new topic of priorities before he can place and accept the topic of ambivalence as worthy of study. Indeed, the large quantity of examples of the phenomenon cited throughout the essay are, in part, necessary to confirm to the reader that this topic does exist.

Since the topic of ambivalence involves a critique of the field, the writer has a special problem with respect to the scholarly audience, all of whom presumably are subject to the cognitive lapse which is under discussion. Merton must challenge the readers while still maintaining their good will and attentiveness. To overcome audience resistance and ease the shock of self-recognition, Merton creates a strong presence of his own viewpoint and an atmosphere of camaraderie that assumes temporarily that the audience is already with him. He begins with statements of great certitude and only later fills in the background of concepts that make the opening statement possible. This technique bears similarity to the way Hemingway opens *To Have and Have Not*: 'You know how it is there early in the morning in Havana with the bums still asleep against the walls of the buildings; before even the ice wagons come by with ice for the bars'.[21] The reader is drafted into a club, and only gradually is he filled in on the experience he presumably shared from the beginning. The reader is companionably drawn into the world populated by sleeping bums and bars and early morning adventures in Havana. In Merton's essay, the atmosphere of agreement takes the edge off the challenge and creates enough good will for the argument to unfold. Further, Merton withholds explicit discussion of sociologists' group involvement in the problem until the entire mechanism has been laid out, the giants of science implicated, a few confessions cited, and dispassion praised. Moreover, eminent psychologists and sociologists are identified as having the courage of self-examination on this matter before the readers are asked to consider their own cases.

After introducing the problem, in the second paragraph Merton identifies the mechanism of the ambivalence, thereby localizing the phenomenon in a theory of the operations of science. The metaphor of conflict of forces is drawn from physics, and Merton is careful to label it as metaphor by the phrase 'can be conceived of'. There is no claim here of measurable forces as there would be in physics. Metaphors are underconstrained in meaning; by their nature they are only suggestive and approximate. One resorts to metaphor only when the thing to be described is partially or imprecisely known, and one must look to correspondences with better known objects. Even in the best of metaphors the correspondence between the thing being described and the metaphorical representation is only partial. In any specific case, however, the metaphor may be the best available description and, when combined with other underconstrained terms and contextual clues, may create a web of approximate meanings surrounding the actual thing, such that a meaning develops adequate to the situation. The second sentence provides a second underconstrained meaning to support the metaphor of resistance: 'Such resistance is a sign of malintegration of the social institution of science which incorporates potentially incompatible values. . . .' Of all the sentences in the article, this sounds the most typically sociological, precisely because it attaches the topic to familiar sociological concepts. The terms of this sentence, however, are abstract, some of variable or disputed meaning, some metaphoric, and all in a complex syntactical relationship that makes the imprecision additive, if not geometrical. Further, resistance is only 'a sign', not a particular sign or the only sign. Here the indefinite article is a true indefinite, unlike Watson and Crick's 'a structure', where near at hand observations of nature can fix the structure as unique.

Such underdetermination of language provides further reason for requiring the good will of the audience. A sympathetic audience is more likely to expend the effort to reconstruct from partial indicators the meaning most congruent to the argument—a process that may be called reading in the intended spirit. The unsympathetic reader, however, can find in underconstrained meanings enough inconsistency, contradiction, and unacceptable thought to mount a serious attack. Even such ordinary appearing terms as 'scientific accomplishment' or turns of phrases as 'as happy as a scientist can be' rely on many loosely defined conceptual assumptions; they can easily disintegrate under a hostile reading.

In the third paragraph the author turns from an invisible social structure which is claimed to generate the ambivalence to the more visible 'overt behavior that can be interpreted as expressions of such resistance'. Even these overt manifestations of trivialization and distortion, nonetheless, are not directly measurable

21 Ernest Hemingway, *To Have and Have Not*, New York 1937, p. 1.

experience the imaginative life embodied in it. Insofar as the poem can be reduced to easily understood, verifiable claims—'normalized', in Hartman's term—the poem is of little interest.

This concern with the aesthetic moment of the poem requires that an existential bond be created among poet, critic, and reader. In the process of conveying the poetic moment, the critic's sensibility plays the central role. The poem, the literature, and the audience's perception are all mediated through the critic's vision. The critic perceives new dimensions of the poem, uses the literature to allude to his own aesthetic experience, and asks the audience to accept a new way of reading the poem. The poetic text and its context, the accumulated experience of literary criticism and literary texts, and the audience's critical judgement and expectation of poetry do constrain what the critic can persuasively state, yet the critic has considerable power to transform all of them.

In one sense the object of investigation, a sonnet entitled 'To the Torrent at the Devil's Bridge, North Wales, 1824', is a known and discrete phenomenon. It is printed in the collected works of William Wordsworth; apparently no scholar has questioned the attribution to Wordsworth, the dating, or the purity of the text. The poem is easily reproduced, as is done at the beginning of the essay. Moreover, some elementary literary techniques and a few well-known biographical facts seem to explain the apparent features of the poem, as Hartman demonstrates in the third through the sixth paragraphs. The topic of the essay, consequently, appears to be fixed in a framework even more complete than that which surrounds D.N.A., to the point where the topic appears trivial. Here, though, the essay sets the framework aside as not revealing the important knowledge of the poem.

That important knowledge is a complex state of mind beyond naming. Hartman can only try to reevoke it through description, contrast, analogy, and reconstruction of context. As Hartman states at the end of the second paragraph in what is the closest approximation of a thesis in the essay, 'Uncertainty of reference gives way to a well-defined personal situation, that is easily described, though less easily understood'. The outside of the situation, captured in the description, is distinguished from the inside of the moment, which counts as understanding. The poem, as verbal artifice, conveys something beyond the words.

The title of the essay indicates the true subject: 'Blessing the Torrent' is an act accomplished through the poem. Six of the essay's seven sections are devoted to recreating the existential moment of blessing. The subtitle 'On Wordsworth's Later Style' indicates that the act of this poem is similar to the acts of others of Wordsworth's later poems, but this similarity is only discussed in the last section of the paper, and no other poem is examined in sufficient detail to establish that it is the vessel of a similar moment. This reading of one sonnet can only provide an analogy for the reading of others, making the other poems more accessible; any more specific claim of equivalence among poems would suggest a reductive normalization. Each poetic moment is itself and no other.

The essay is structured to make the poet's state of mind accessible in all its fullness to the reader, to widen gradually the reader's consciousness of the central issue of the poem. The essay opens with a consideration of the literal meaning of the opening question of the poem: 'How art thou named?' Each of the following sections grows out of an issue raised in the previous one in order to open up the central, opening question. In a sense, each section progressively uncontains the flood.

The epigraphs of Hölderlin, Stevens, and Joyce prepare a first reading of the poem by setting the river in motion as one of a poetic family of floods, puzzling and uncontainable. The first section by raising issues of form—the untitled, unplaceable fragment versus the named, closed sonnet—localizes this particular flood, but raises the problem of understanding the localization. The second section takes up the theme of localization to examine biographical information that raises problems about what the poet could be meaning. At this point the critic brings in other samples of Wordsworth's writing to show the poet's way of thinking about these issues. The writings of other poets are examined to show what Wordsworth did not mean. By the end of the second section the formal solution to naming collapses as the critic points to the inadequacy of the poet's diction to fulfill the domesticating function of the sonnet.

The third section examines this dilemma through the text of the first half of the poem, where the poet explains the problem and proposes a first, inadequate solution. The fourth section discusses the acceptance of the inability of language to localize, as developed in the second half of the poem. Against this reading of poetic moments. Even though a Hölderlin poem may shed light on a Wordsworth poem, however, they remain separate, with separate lives to be evoked and with no fixed relationship to each other. Hartman does not even attend to the historical task of tracing influence and literary tradition, which would establish at least some formal connections between poems.

The last type of literature is the testimony of Wordsworth and his intimates concerning his state of mind and poetic intentions. This category includes letters, journals, and Wordsworth's other poems when they are used in an evidentiary way. As with the previous types of literature, these documents are used only to illuminate Hartman's perception of the dynamics of the poem under study, and they are interpreted through that perception. Thus Hartman uses a letter in which Wordsworth copied the poem not as an honest reflection of the poet's state of mind, but to recall another time when Wordsworth criticized just such attitudes as expressed in the letter. This juxtaposition, not at all evident in Wordsworth's letter by itself, prepares Hartman's criticism of the absurdity of the conventional reading and introduces the existential paradox which becomes Hartman's theme. Thus all the references, from the most scholary historical geography to the most poetic evocations, serve only to recreate the consciousness Hartman perceives embodied in the poem.

The critical and poetic literatures have an additional important, but implicit, role: the language of the essay invokes and evokes concepts and aesthetic experiences from the entire history of poetry and poetic criticism. The literary vocabulary on one level appears to be purely technical, not unlike the technical vocabularies of molecular biology or sociology. Terms such as *topos, apostrophe, sonnet, turn, enjambment* and *sublime* are the critic's basic conceptual equipment, learned as part of professional training. On another level, however, the literary terms are more than technical, for each reverberates with former uses and examples. One can know and understand *deoxyribose* on the basis of modern chemistry alone, but to understand the *sublime* one must not only have read Longinus and be familiar with the ensuing critical debate to modern times, one must have experienced a wide range of poems that embody the development and variation of that concept. Even terms that do not refer directly to experience—*sonnet*, for examply—rely on wide literary experience. That a poem has fourteen lines, particular rhymes and meters, and a turn is of some outward interest, but of greater importance is that the poem stands in a tradition that began as a representation of love, became increasingly introspective and confessional, then took on religious and philosophic concerns, fell into disuse as uncongenial to the concerns of the eighteenth century, and was finally revived by the romantics. To understand the term sonnet is to be sensitive to the wide range of consciousness and experience it has served to realize. Moreover, to understand the term's use in a phrase such as 'Though the sonnet as a form is a domesticating device . . .' one must remember the courtly lover torn by love yet graceful in his meters, Donne in religious turmoil tearing at the form, Herbert turning the sonnet in on itself, and Milton in grief, blindness, and civil war finding repose for the space of fourteen lines. In comparison, the sociological and psychological terms used by Merton—e.g., *ambivalence, denial*, and *integration*—do have histories in the literature, and familiarity with the original texts helps reveal how the terms are used, yet the history of the field and the experience of reading the entire corpus is not evoked in the use of the terms.

Because the experience embodied in the poetic literature and interpreted through the critical literature is implicit in the literary vocabulary, the terms take on an added subjective element. Not only does Hartman use the critical vocabulary to elucidate the subjective experience of the poem as he perceives it, his use embodies his own entire experience of literature—his experience of Longinus, Milton, and even Joyce. Moreover, in trying to communicate his perceptions he is relying on the subjective experiences each of his readers have of literature. Each reader has intimate familiarity with a different range of literature, and each reader gives each text a different reading. One's personal anthology personally interpreted comprises the individual's share of the corporate knowledge and is the basis of that individual's sensibility.

In the chain of consciousness from poet to critic to reader, the enterprise rests on the quality of the mediating critic's sensibility. Of course one can read a poem without benefit of a mediating critic, and some schools of thought suggest the best reading is the least tutored. If one turns to a critic, however, the reader must believe that the critic perceives things that would not be apparent to the reader. A critic's persuasiveness, therefore, depends in part on establishing a persona of perceptivity, if not brilliance. Reputation, which is prior to any given article, no which to measure Watson and Crick's claims and to suggest how the claims might be applied; therefore, the authors do not urge, but rather leave the audience to judge and act according to the dictates of science. The sociological audience, sharing no uniform framework of thought or criteria of proof, must be urged, persuaded, and directed along the lines of the author's thoughts. The literary audience, concerned with private aesthetic experience, must find the critic's comments plausible, but more important must find the comments enriching the experience of reading; evocation of the richest experience is persuasion.

In their essay Watson and Crick take on a humble yet proud authorial presence: the humble servants of nature and their discipline, filling in only a small piece of a vast puzzle and subject to the hard evidence of nature and the cold judgement of their peers—yet the proud originators of claims that have the potential ring of natural truth and nearly universal professional acceptance. Merton stands more uncertainly before his discipline and nature, neither of which holds the promise of clear-cut judgement and unequivocal support, yet through the force of argument he hopes to establish some certainty. Curiously, the literary critic Hartman, who has the least responsibility to establish certainty, must take on the most demanding role: appearing to have insight greater than that of his readers. Since his contribution cannot be measured in terms of a
claim to be judged right or wrong, the quality of his whole sensibility is up for 379
judgement.

As stated at the beginning of the essay, the texts examined are not necessarily typical of their fields and the contrasts revealed by analysis cannot be taken as defining the features of a spectrum of knowledge. We cannot even begin to speculate on what uniformities with what variations exist within disciplines or whether patterns of differences emerge among disciplines until many more examples have been examined and statistical indicators found to test the generality of conclusions. This analysis, nonetheless, does suggest terms on which typicality can be explored and through which the symbolic knowledge of different disciplines can be compared. The terms of the analysis here provide concrete means for investigating the character of the endeavours of different disciplines, at least as those endeavours appear through the public record of publication.

Moreover, the terms of this analysis suggest how texts serve as dynamic mediating mechanisms, creating those elusive linguistic products we call knowledge. In focusing attention on texts, this analysis looks through the texts to the realms represented in the texts. Texts bring together worlds of reality, mind, tradition, and society in complex and varying configurations, and knowledge is in those words that sit in the middle.

APPENDIX 380

I J. D. Watson and F. H. C. Crick, 'A Structure for Deoxyribose Nucleic Acid', *Nature*, **171**, April 25, 1953, pp. 737-38, complete.

II Robert K. Merton, 'The Ambivalence of Scientists' in Norman Storer (ed.), *The Sociology of Science*, Chicago 1973, pp. 383-412. Excerpted, pp. 383-85.

III Geoffrey H. Hartman, 'Blessing the Torrent: On Wordsworth's Later Style', *Publications of the Modern Language Assocation*, **93**, March 1978, pp. 196-204. Excerpted, pp. 196-97.

8

SCIENCE AND THE MEDIA

B. S. P. Shen

SCIENCE LITERACY

Today, science affects almost every aspect of our lives, and we can expect its dominance to be even greater in the future. It is thus in the interest of everybody, scientist or not, to gain a better understanding of science and its applications, if only to learn how better to utilize its benefits and avoid its pitfalls. Such an understanding might be called "science literacy."

Science literacy can be many things, from knowing how to put together a nutritious meal to knowing how to enjoy the laws of physics. Skilled popularizers are needed to make scientific subtleties clear to the layman, and the mass media and the schools can help to bring popularized science to the public.

In order to put these varied activities into perspective, I find it helpful to distinguish three forms of science literacy, which differ among themselves not only in their objectives but often also in their audience, content, format, and means of delivery. The three forms may be called "practical," "civic," and "cultural" science literacy. Of these, practical science literacy is without a doubt the most urgently needed and frequently the most neglected.

Benjamin S. P. Shen is the Reese W. Flower Professor of Astronomy and Astrophysics, Chairman of the Department, and Director of the Observatory at the University of Pennsylvania. This article is adapted from his chapter in Communication of Scientific Information, *edited by S. B. Day and published recently by S. Karger AG, Basel, Switzerland. Address: 4N6 David Rittenhouse Laboratory E1, University of Pennsylvania, Philadelphia, PA 19174.*

Practical science literacy

Close to a billion people in the world today live in deep poverty, with virtually no access to the kind of practical scientific knowledge of health, nutrition, and modern agriculture that could ease their plight to some degree. A great deal of human suffering has resulted from a lack of such vitally needed knowledge; this is the "information gap" at its worst. Practical science literacy offers a partial antidote, even though it can do little to correct the socioeconomic inequities that usually are at the root of the problem.

By practical science literacy, I mean the possession of the type of scientific and technical know-how that can be immediately put to use to help improve living standards. Since the most basic human needs are food, health, and shelter, it is no surprise that much of practical science literacy has to do with just those needs. To take one example, it is becoming fashionable in some cities and towns of Africa, Asia, and Latin America for mothers to bottle-feed their babies rather than breast-feed them, despite a widespread protein shortage. Unfortunately, the water used to prepare the formula is often contaminated. This, combined with the lack of human antibodies in cow's milk, has led to a much higher mortality rate among bottle-fed infants than among breast-fed ones, according to recent reports. There is thus an immediate need to change the attitude of young parents in those countries by bringing to them the message that breast-feeding is safer and better for babies.

Examples like this abound. The availability of a few pieces of essential scientific information can mean the difference between health and disease, life and death. In recent years, the Green Revolution has improved the grain productivity in a few regions of the world, but its success has depended not only on an increased availability of fertilizers, pesticides, and irrigation but also on an increased level of science literacy among farmers, who must know how to make optimum use of the new seeds.

The need for practical science literacy is by no means confined to developing countries. Whether a supermarket shopper with only a dollar to spend for meals—not a rare occurrence nowadays—should buy protein-poor cake or protein-rich cottage cheese is such a vitally important decision that it should only be made with a knowledge of the principles of nutrition. The United States has one of the highest infant mortality rates in the world, and a recent study showed that a lack of basic health information on the part of parents in the poorer communities is a major cause. In consumer protection efforts in the industrialized nations, practical science literacy can serve as a useful adjunct to legislative safeguards.

The delivery of practical science literacy to the vast number of people who need it is a complex task requiring a concerted effort in mass communication. In late 1975, the communication satellite ATS-6, now in orbit, will be used in an experiment by India to deliver health and agricultural information via community television to various rural regions of that country. Simisues that are not science-related. Civic science literacy would merely allow him to do the same in the case of science-related issues. The scientific background of the average layman a few generations from now will undoubtedly be greater than it is today, but, even today, I cannot think of a single public issue whose technical aspects cannot be readily understood by the layman when shorn of all jargon and minutiae and when its essence is explained in nontechnical language by skilled popularizers of science. The single most difficult task for civic science literacy, I suspect, will not be to explain science to the layman but rather to convince him that he has no reason to shy away from it.

To achieve a minimum functional level of civic science literacy, at least two things need to be done. First, the public must be far more exposed to science than it is today. The reporting of science news over the air and in newspapers should be increased both in quantity and quality. Science teaching in primary and secondary schools should also be made more effective in order to provide the foundation for a lifelong familiarity with science and an awareness of its social implications. Second, the complexities behind specific science-related public issues must be analyzed in plain language for the average citizen on a continuing basis by specialists in explicating science. They should also help the layman separate the nontechnical policy aspects of an issue from its technical aspects. Here again, the electronic and print media are essential for communicating the analyses to the citizen, through special reports of the kind we now see only at election time, during a manned space flight, or when some crisis intervenes.

Compared with practical science literacy, the attainment of a functional level of civic science literacy will be a more protracted endeavor. Yet it is a job that sooner or later must be done, for science-related public issues can only increase in number and importance in the future. In trying to advance civic science literacy, it is essential that the effort not be allowed to degenerate into a public-relations ploy for science or for scientists. The temptation to mix science literacy with science propaganda will be strong, but it must be resisted.

Cultural science literacy

When a student takes a course in physics-for-nonscientists, when an artist reads a magazine article on DNA, or when a lawyer watches a television program on the Crab Nebula, they are engaged in improving their cultural science literacy. They do this in the same spirit in which a science student might study ancient history, an engineer read poetry, or a physician delight in classical tragedies. Cultural science literacy is motivated by a desire to know something about science as a major human achievement; it is to science what music appreciation is to music. It solves no practical problems directly, but it does help bridge the widening gulf between the scientific and humanistic cultures.

One of the problems of cultural science literacy is that it is at present available to only a comparatively small number of people. Its reach today barely extends beyond the intellectual community. Every effort should be made to increase its accessibility so that, ultimately, everyone who is interested will have the opportunity to enjoy science in his leisure time as readily as he might enjoy the arts and letters. To this end, the mass media must be utilized. Two recent series shown on public television represent steps in the right direction: Michael Ambrosino's "Nova" and Jacob Bronowski's "Ascent of Man."

Another problem relating to cultural science literacy is more subtle. In my recent service as chairman of a committee concerned with the public understanding of science, I have talked to hundreds of people about science literacy, and I have come across a small but vocal school of thought which frowns upon the contents of much of today's cultural science. This school is of the opinion that it is neither sufficient nor desirable for a layman to know a little about science: cultural science literacy is a concession to mediocrity. The layman, it is held, should go through a certain amount of technical preparation in order to be able to perceive at least the elegance, say, of a particular mathematical formulation of quantum mechanics. This is a little like saying that a layman wanting to know something about ancient history should first learn to appreciate the finer technical points of historiography and archaeology.

Viewpoints such as this reveal the arrogance of science in our century. The average layman, after all, has other things to do, and, unless he is a student, there is no reason why he should want to gain more than an acquaintance with the salient facts, premises, and conclusions of science and with the general forms of scientific reasoning, all of which can be readily put in nontechnical language, as the better works of science popularization have amply demonstrated over the years. We should accept the fact that it is not the purpose of cultural science literacy, or of any kind of science literacy, to train science hobbyists or future scientists. A little knowledge, carefully presented and well understood, can be a very good thing.

Although on the surface cultural science literacy seems to be completely devoid of the utilitarian objectives so basic to the other two forms of science literacy, in at least two respects it does exert a significant influence on human affairs. First, despite its relatively small audience, the contents of cultural science preferentially reach the current and future opinion leaders and decision makers in many communities. Because of this, cultural science literacy may in the long run affect human events very profoundly indeed. Second, cultural science literacy can greatly influence what has been called "the new irrationalism." In the past few years, a host of superstitious and occult beliefs, such as astrology, food faddism, cosmic catastrophism, and the notion that plants have emotional lives, have found favor among younger Americans, especially those of college age, and are often mistaken by them for serious science. Fortunately, cultural science literacy can reach the student portion of this audience without difficulty. Although irrationalism as a philosophical attitude will probably forever be refractory to rationalistic arguments, experience has shown that even a small dose of cultural science can exorcise many

J. B. S. Haldane

HOW TO WRITE A POPULAR SCIENTIFIC ARTICLE

J. B. S. Haldane, 'How to write a popular scientific article', *New Scientist*, vol. 105, 4 April 1985, pp. 29–30.

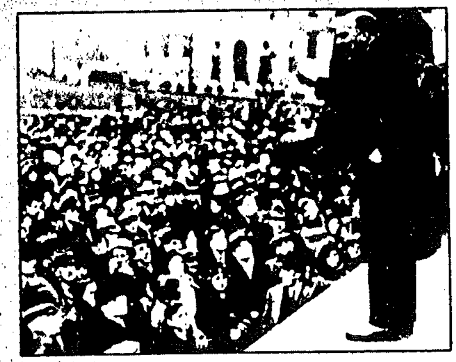
'A man of violence by temperament and training'

J. B. S. HALDANE was perhaps the most charismatic scientist in a generation that did not lack outsize personalities. His main contribution to science was to lay the foundations of evolutionary genetics, but his interests were far more wide-ranging than that.

When Mendel's laws were rediscovered in 1900, most advocates were anti-Darwinian, seeing mutation rather than natural selection as the driving force of evolution. Haldane, with his English contemporary R. A. Fisher, and the American Sewall Wright, showed that there was no contradiction between the two theories, and developed the neo-Darwinist theory which is still accepted today. His book, *Enzymes*, published in 1930, was one of the first serious contributions to enzyme kinetics. He played a major part in converting human genetics from a prejudice into a science. He had a life-long interest in respiratory physiology, acquired as a boy when he acted as assistant to his father, the physiologist, and culminating in the Second World War with his research on the physiology of diving. With Oparin, he was the first to emphasise the importance, for theories on the origin of life, of the fact that the atmosphere of primitive Earth was probably a reducing one. He published on topics as diverse as the dance of bees to the origin of the solar system, from the respiration of bacteria to the mathematics of fluctuations in price.

But what made Haldane a great essayist was the scope of his interests outside science, particularly his interest in religion and politics. He was hostile to religion, yet fascinated by it. Today, if we exclude back-woods creationists, religious leaders tend not to quote the mysteries of biology as an argument for their beliefs. Things were different when Haldane was young. I do not doubt that his choice of biochemistry and evolution theory as his research subjects arose because it was there that the last citadel of mystery lay. At the same time, he recognised that religion is central to culture.

It is characteristic that when, in his 60s, Haldane went to live in India, he read widely in Hindu mythology. Although musically deaf, he had a continuing love of poetry, which he read in a surprising number of languages, and was liable to recite in moments of excitement.



Haldane addresses a meeting of the United Front in Trafalgar Square, 1937

Haldane's political activities started with the rise of Hitler, and ended soon after his fall. In the 1930s, he joined the Communist Party, and became a leading propagandist for resistance to fascism. After the Second World War, he became increasingly disillusioned with the Party, and with the policies of the Russian government. This was brought to a head by the Russian treatment of the "Mendel-Morganist" geneticists. I do not think, however, that he ceased to believe in the desirability of socialism.

Haldane's life was a good deal more turbulent than is usual for academic scientists—sometimes too turbulent for the comfort of his colleagues. He fought in France during the First World War—and admitted to enjoying it. I think he enjoyed feeling afraid. In 1925, he was deprived of his readership at Cambridge for immorality (he had been cited in the divorce proceedings of a woman he later married), although he was reinstated on appeal. His diving experiments were both dangerous and painful.

Haldane retired from University College not to cultivate his garden, but to start a new life in India. And his response to a major operation for cancer was a comic verse—not, I think, one of his best (he had had a colostomy), although it did include the admirable couplet, "So now I am like two-faced Janus/The only God who sees his anus".

As a final indication of Haldane's unpredictability, I quote from one of his last essays. It opens, "I am a man of violence by temperament and training. My family, in the male line, can, I think, fairly be described as Kshattriyas. From 1250 to 1750 we occupied a small fort commanding a pass leading from the hills to the plains of Scotland."

He goes on to say, of the First World War, "In the war of 1914-1918, I was on several occasions pitted against individual enemies fighting with similar weapons. . . .This was war as the great poets have sung it. I am lucky to have experienced it." Oddly, this essay was not in praise of the ennobling role of war. It was a plea to Indian biologists to follow Gandhi by establishing a non-violent biology.

John Maynard Smith

▶ severely criticised for "dragging in" references to Marx in my articles in the *Daily Worker*, though I think I refer to Engels more frequently. But a number of my readers are familiar with the works of these authors. Engels said certain things about change, as Heraclitus said very similar things before him, and Bergson and Whitehead after him. But for one of my readers who has read Heraclitus, Bergson, or Whitehead, a hundred have read Engels, so I prefer to cite him . . .

A popular scientific article should, where possible, include some news. I try, as a general rule, to include one or two facts which will not be familiar to a student taking a university honours course in the subject in question, unless his teachers keep well up with the periodical literature. As there is often a lag of five years between the publication of a discovery and its inclusion in a textbook, this is not very difficult in peacetime. But is not very easy at present, in view of the number of libraries which have closed down, and the absence of many European and some American periodicals. Of course some care is needed in appraising new work. A very large number of alleged discoveries are not confirmed by subsequent workers. One well-known English populariser of science has a perfect genius for picking out discoveries of this kind for the announcement to the public. If, like myself, the writer is actually engaged in research, and has seen a number of his own bright ideas go west, he is less likely to fall into this particular trap.

In the early stages of popular writing it is well to write out a summary of the article, though I rarely do so myself. Here is a possible skeleton for an article on cheese.

Introduction. A well-known fact, say the shortage of cheese.

Central theme. Cheese manufacture.

Why is it important. Cheese as the cheapest food containing large amounts of "good" protein. Vitamins and calcium in cheese.

Connections with other branches of science. Rennet compared with any other enzyme preparations used in industry, for example in confectionery and tanning. Other uses of specific microorganisms, for example in brewing. Why putrid cheese is safer than putrid meat.

Practical suggestions. How to increase our cheese output. Combating mastitis in cows. Cattle-feed and fertilizers. Should cargo space be devoted to cheese rather than meat? Need for scientific planning of national food supply.

How much of this you can get in depends upon the length of your article and your capacity for compression. If you are writing for a highbrow journal you may quote the passages on cheese from Euripides' *Cyclops*, if for a lowbrow, any of the jokes about the smell of cheese.

That is one way of doing it. But other writers would show cheese as part of the Mysterious Universe. We do not understand protein synthesis, nor the extreme specificity of some enzyme actions. Cheese-making is part of the pre-scientific activities by which we still keep a communion with nature. Cheese is a natural food, and beef is not. And so on. I think this is an anti-scientific attitude. But you can sell that sort of stuff, and [illegible] over a certain amount of genuine knowledge while doing so. Everyone m[illegible] write popular scientific articles in his own way. I have only described one way, and I do not claim that it is the only way, or even the best possible way. □

ANATOMY OF A SCIENCE STORY

Editor's Note

They tell us modern society is just too technically complicated for journalists to understand or to report to the public. 'They," in this case, are a growing number of media critics who are concerned that the American people may not be getting an accurate or fair picture of the fast-breaking changes in science and technology today, changes which have a great impact on all our lives.

They point out that today's news increasingly involves computers, nuclear power, outer space, toxic chemicals, lasers, and recombinant DNA research. And they worry that oversimplification and sensationalism in the media's coverage of these developments may lead to false hopes and unfounded fears.

These subjects are certainly complex and significant, but are they more complex and significant than the developments our grandparents and great-grandparents read about in their newspapers? Imagine the problems faced by journalists trying to explain what makes an airplane fly. Or the complicated process by which sound waves become electronic impulses

continued on page 2

NARRATOR: "It is of great importance that the general public be given the opportunity to experience, consciously and intelligently, the efforts and results of scientific research. It is not sufficient that each result be taken up, elaborated and applied to a few specialists in the field. Restricting the body of knowledge to a small group deadens the philosophical spirit of a people and leads to spiritual poverty."

—Albert Einstein

WILLIAMS: I sure can't improve on the way Einstein put it, but I do agree. There is a certain excitement in science. As a science writer, I talk to these people who are so wrapped up in their work and so enthusiastic, actually excited about it, that I feel an obligation to convey a sense of that excitement and enthusiasm in my stories.

MATTHEWS: Einstein states the role of the scientist very well. A scientist can be extremely excited by a breakthrough that he or a colleague makes, and clearly that excitement, that enthusiasm, and the potential of that result have got to be made available to the general public.

NARRATOR: Today, more than ever, science and technology touch each of our lives in ways we never dreamed possible. Yet it is just as difficult now, if not more so, for science information to move from a researcher to the public through magazines, newspapers, radio and television. The barriers to communication are multiple and affect not only the process but the people—the scientists, the journalists, and the rest of us.

PERLMAN: Jacob Bronowski said that the world today is powered by science, and for the lay public to abdicate their interest in science and turn it over to a group of scientists presumably armed

continued on page 3

Media Resource Service

MRS Update
page 10

other looked at their heart function using the carbohydrate loading technique.

NARRATOR: Abnormal heart patterns showed up when the four volunteers ate a low carbohydrate diet for three days and then exercised on a stationary bicycle while attached to a heart monitoring device. This abnormal heart pattern has been documented before by other researchers. It sometimes shows up before the heart goes into a state of uncontrolled contraction and expansion called fibrillation.

CAMPBELL: There have been some indications in the literature that persons who normally train for a marathon might be subject to abnormalities that would suggest fibrillation that would lead to a heart attack.

NARRATOR: The researchers feel there is a possibility the skeletal muscles are robbing energy from the heart.

CAMPBELL: And that is probably what is taking place in this circumstance—the cardiac muscle was robbed of the carbohydrates that might otherwise be made available to it because of the need of the skeletal muscle tissues to perform their task.

NARRATOR: The researchers warn that the greatest danger to the athlete comes during the carbohydrate depletion phase when stored energy is low and the athlete is still training hard. Leklem and Campbell both recommend eating a balanced diet during periods of strenuous exercise.

The Time Barrier

The research project nears completion, and here's the first potential barrier to communication. Should the information be released to the public before it has been officially reviewed by the scientific community, either at a major scientific meeting or in a scientific journal? According to George Streisinger, a biologist at the University of Oregon, peer review of such research is an internal accuracy check.

STREISINGER: It also checks for omissions in methods—something that you've done wrong or serious faults in reasoning and other things like that. You may believe you've found something, when in fact it is due to your not having washed your bottles properly or something else totally trivial. So, it's really important. What you find may not be so, and peers—other scientists—look at your results to check whether there are any serious problems.

NARRATOR: But the issue can be a little more complicated than that. Researcher Jim Leklem and Russ Mitchell, a *Corvallis Gazette-Times* reporter working on our story, review part of their process.

LEKLEM: The study, when finally completed, was written up at two different points by the graduate students involved (they're very critical in this step) and then presented at another scientific meeting. It was at that point, after the study had essentially undergone peer review, that we felt we could present this information not only to the scientific community, but at the same time to the general public.

MITCHELL: Considering the gravity of the story—hundreds of thousands of people have run marathons since the study was done, and have trained in a way that could potentially lead to heart attack—I think it would be incumbent upon scientists to release this kind of information as soon as they're certain that the conclusions are correct.

LEKLEM: I felt uncomfortable about that, because what we had found was well done, well documented, and significant enough that it should perhaps have been conveyed to the public earlier. Part of that responsibility lies with ourselves in not pursuing that more vigorously than we did. Part of it has to do with policies that were developed about this kind of publication in general, and about this kind of information, before it has really had adequate peer review.

> My rule for dealing with results of research is to wait until the work has been refereed. I have had too many experiences where today's breakthrough turns out to be less than that a year later.

report the presentation of a paper at a meeting or the publication of a paper in a journal when it happens to arrive at your desk at a news magazine or newspaper. That's a pretty artificial thing.

NARRATOR: The next potential barrier is here at the point of contact. Who instigates the meeting? Scientists appear reluctant to seek out journalists.

MATTHEWS: Difficult problem. I know very few members of the faculty here who on their own initiative will go to the press and try to encourage the press to write articles on their work. Rather, it tends to develop through a third person—the department chairman or some other person who is aware of the work.

NARRATOR: In some cases journalists are leery of scientists who seek them out.

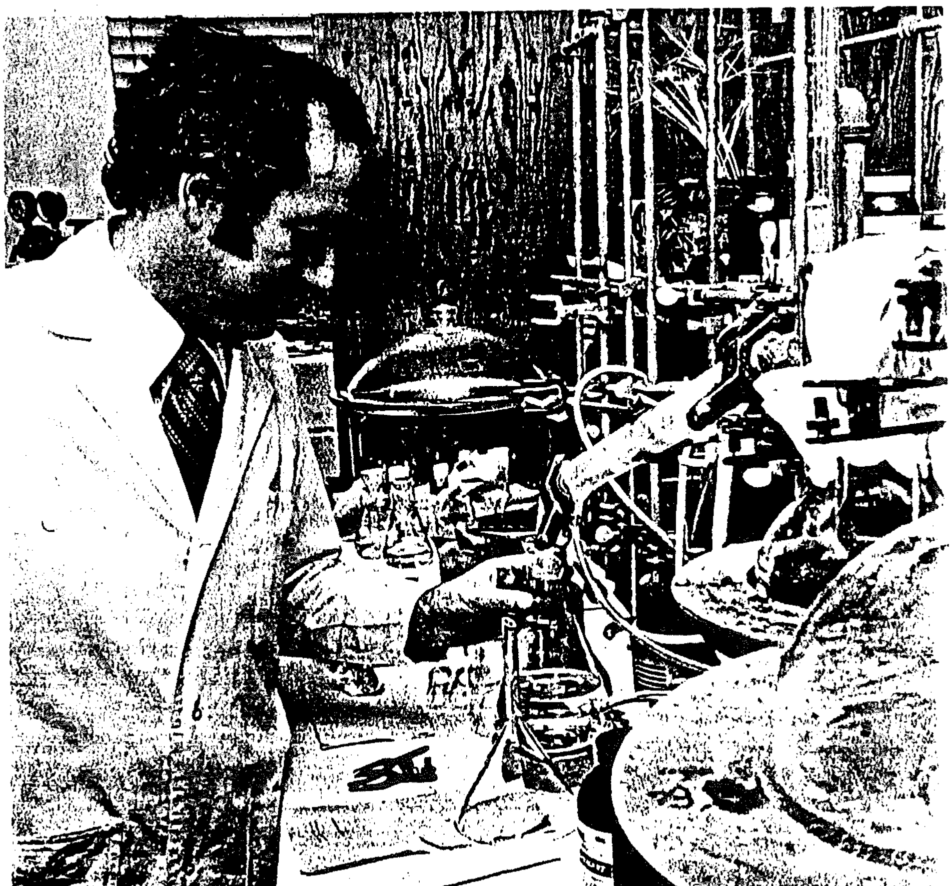
COLBY: I have always been skeptical of scientists, or people who say they are scientists, coming to me with things. I will look for proof that he or she has published in the field and is accepted by peer review. There is that test one has to make. In a few cases, people of obvious mental instability have come in claiming to have invented or done various things, and you have to dismiss them.

NARRATOR: Some reticence occurs when the scientists feel their work is inconclusive or easily misunderstood. This becomes a sensitive question, especially at publicly funded institutions. It concerns the public's right to know.

SCANLAN: In general, I believe in the idea that the public has a right to know. I think where that becomes sticky, in some instances, is where the results of, say, a certain line of investigation comes out in a somewhat gray area, particularly in terms of knowing what the public health significance of the findings are.

DAVIS: In those instances where a researcher has conducted research properly and finds that the results are inconclusive and it is an extremely sensitive area, the researcher feels put in a position of judging whether or not the public should be informed of the results, however inconclusive—to warn the public that there may be a problem, or on the other hand, to sensationalize it and scare the public. My opinion is that in a public university, if there is a question like that one, one always has to come down on the side of advising the public.

NARRATOR: In the case of the project we are following, Russ Mitchell received a press release from Oregon State University indicating that the results of this project would be presented at a national scientific meeting in Minneapolis. Mitchell interviewed the two scientists before their trip and



Dr. Richard Scanlan of Oregon State University

LEKLEM: I think that if the journalist takes the time to interview the individual, (a) personally or (b) over the phone, it really becomes very important, necessary, ethical for that journalist to communicate back what's written before it is then released in any format.

NARRATOR: Although a survey has shown that science writers appear united against the idea of actually allowing a source to read a story before release, many of the writers we talked to do check back to various degrees for their own benefit. While working on the story we are following, Russ Mitchell checked back with sources more than once.

MITCHELL: I'm writing the story now and I'm going to call back Dr. Campbell tonight because there are some aspects of the story that I'm not 100 percent sure about and I want to check those. Where I think I would balk is if a scientist should ask me to bring in my entire story for review.

PERLMAN: A quick and dirty answer is that it depends on the way in which he asks. If he asks in a tone that says, "Hey buddy, I don't trust you and you are going to screw this story up," I resent that, because I am as competent in my field as the scientist is in his field. On the other hand, I want to get things right.

RENNER: As for submitting the story to sources before I put it on the air or before a print journalist puts it in the newspaper, I don't agree with that. It's the old thing of you don't want the dog guarding the smokehouse. You don't have a politician approving stories before they go on; I think the same is true with scientists.

As for submitting a story to sources before I put it on the air, I don't agree with that. You don't want the dog guarding the smokehouse.

NARRATOR: As a freelance magazine writer, Janet Hopson sees checking a story as an important part of her writing process—but only for accuracy, not for style.

HOPSON: I'm not out there doing investigative reporting and trying to blow the cover off some terrible scandal. My interest is in educating the public about science, just like scientists. So I will usually send a copy of my story to every source that I interview, so that they can read it. I explain when I do this that I am interested in the accuracy check from their point of view, and I will still determine stylistic matters.

The Credibility Barrier

NARRATOR: Our story is about ready for release, but as it goes out, one more potential barrier appears. If a scientist feels poorly treated by the press, he or she becomes a very reluctant source the next time around, and a major barrier to communication is established early in the pathway. The headline is one potential culprit, but the writers have little control over headlines.

PERLMAN: The most frequent complaint that a scientist makes is about the headline on a story. Of course, scientists always assume that we reporters write the headlines. And of course, we don't. We not only don't write the headlines, we are not even in the office when the headlines get written. I write a story and turn it in by 5:00 or 6:00 at the end of the day and go home; the headline for that story isn't written for two or three hours and the headline may change in the course of the night as more news comes in.

COLBY: But most scientists get very embarrassed or even hostile if they feel that their work has been overplayed. I think that it goes back to the original question of the scientists understanding the media more. If they understand that this could happen and are prepared to accept it, then relations are a lot smoother.

PERLMAN: I often say to my scientist friends, "I challenge you to write an abstract of your 12-page article *in four words.*" That's what a headline is. It cannot be done. That's why headlines are often oversimplified, sensationalized, etc.

ARCH: That's where the most danger is encountered in science writing. I think the problem is trying to make more of a piece of work than it can

S. Dunwoody

8.4

A QUESTION OF ACCURACY

Abstract—**Many engineers and scientists condemn the inaccuracy of media coverage of science and technology but have never really pinned down what they mean by "inaccuracy." This article examines research that assesses the accuracy of mass media science reporting. The author also gleans from the research ways in which engineers and scientists can improve the chance of getting their information accurately into the media through interviews with journalists.**

FOR many engineers and scientists, the epitome of bad communication is an account of their work that gets published in a newspaper or aired over the local radio or television station. The reason? Inaccuracy. Somewhere between the discussion with a reporter and publication or airing, the information gets garbled. It may emerge awash in speculative comments. Crucial details may be lopped off. Minor points may be emphasized while major ones are relegated to the bottom of the story. And the headline, of course, may make even the most hardened scientist blanch.

It's a familiar litany. Survey after survey finds that scientists' main complaints about media science stories involve inaccuracies. Everyone knows at least one colleague who has sworn off all journalists because she was "burned" by a reporter who got it wrong.

But few scientists and engineers have given careful thought to what they mean by "inaccuracy," and fewer still have contemplated strategies that can help minimize accuracy problems when they are dealing with journalists. Mass communication research has addressed both of these problems to a limited extent. This article discusses some of the research findings and how those findings can be translated into action by scientists and engineers who are committed to getting information to the public via the media.

DIMENSIONS OF THE PROBLEM

Researchers in recent years have been investigating the nature of relationships between scientists and journalists. Generally, they get mixed results when they ask scientists to evaluate the accuracy of journalistic accounts. For example, among 73 scientists involved in a study by Tichenor et al. [1], slightly more than 40 percent disagreed with the statement that science news is "generally accurate." Tankard and Ryan [2], in a survey of 193 scientists, found that 44 percent disagreed with the statement that "science news coverage is generally accurate." And a recent survey of 111 scientists at two Ohio universities by Dunwoody and Scott [3] found that 51 percent cited inaccuracy or distortion as a reason for being critical of mass media coverage of science.

But although hefty proportions of scientists in these surveys pinpointed inaccuracy as a problem, they were counterbalanced by almost equal proportions of respondents who found media coverage to be generally accurate or who maintained positions of neutrality on the issue.

Additionally, two of the studies pointed out inconsistencies among respondents' evaluations of accuracy. In the Tichenor study [1], while 59 percent of the sample rated science news in general as "generally accurate," 95 percent rated an article quoting themselves as "generally accurate." Similarly, in the survey by Dunwoody and Scott [3], scientists rated the accuracy of stories about their own work more highly than they did the accuracy of science news in general. A third study bolsters the general finding that scientists may be more critical of media science coverage in general than they are of stories about themselves. Pulford [4] asked scientists to rate the accuracy of media stories about their work and found that 73 percent of the 143 respondents found the articles to be generally accurate.

Thus it is probably misleading to suggest, as many scientists do, that *most* scientists find the media to be inaccurate when it comes to covering science and technology. Rather, assessments of accuracy seem to be highly situational and as likely to be positive as negative in many circumstances.

INACCURACY DEFINED

Inaccuracies cited by scientists can generally be classified into two groups: objective and subjective.

Objective inaccuracies, sometimes called inaccuracies of *fact*, are mistakes that can be recognized as such by all parties. For example, everyone will agree that a name has been misspelled once the correct spelling is made known. Or if a percentage gets transposed—let's say from 37 percent to 73 percent—in a news story, both scientists and journalists will readily acknowledge that an error has been made.

Subjective inaccuracies, on the other hand, are defined as errors in *meaning*. The decision to label something inaccurate is dependent on individual interpretation. For example, a scientist may suggest that a story is inaccurate because "relevant information has been omitted." No two individuals may agree on what is or is not relevant information.

All of the accuracy studies found that when scientists complained about inaccuracies, they were talking primarily about subjective ones. In the Tichenor study [1], scientists mentioned "overemphasis on the unique" as the greatest perceived problem. The next most serious criticism was "omission of relevant information." In the Tankard and Ryan study [2], "relevant information about method of study omitted"

Manuscript received May 4, 1982; revised July 14, 1982.

The author is a former newspaper science reporter and now assistant professor who teaches science reporting and conducts research in science communication at the University of Wisconsin, School of Journalism and Mass Communication, Madison, WI 53706, (608) 263-3389.

lication rated the resulting stories as more accurate than did those scientists who had no opportunity to check facts. Clearly, reviewing information for accuracy is a good idea. But the strategy of allowing the source to check facts before publication is a controversial one within journalism and such requests often generate tension between reporter and scientist. Deciding how to approach the question of fact checking requires some idea of why journalists and scientists come to blows over the idea.

THE INFORMATION-REVIEW DILEMMA

On the face of it, it seems puzzling that journalists and scientists disagree on this point. Both professions are wedded to the principle of accuracy; inaccuracy is grounds for firing in both cases. Yet what is viewed by the scientist or engineer as a reasonable request is interpreted by the journalist as a highly aberrant one.

The discrepancy, I think, lies not so much in differences in commitment to accuracy but rather in different *expectations* that are grounded in the vastly different enterprises in which the parties are engaged.

Scientists and engineers are heavily involved in publishing. But the emphasis is on writing their own articles for publication in journals. In such instances, checking proofs of articles for accuracy is part of the business. Additionally, manuscripts are often distributed to colleagues for feedback before being submitted to a journal—another process that helps the author make sure he has not missed important points. Except in those cases where priority may be threatened, engineers and scientists can afford to take weeks to worry about manuscripts; the publishing process itself takes months. The point is that scientists and engineers maintain a relatively high level of control over their manuscripts during the publishing process and have both the time and the opportunity to make changes when they are needed.

Most journalists, on the other hand, don't have that kind of control over their stories during the publication process. The journalistic production process is segmented, with different individuals performing different tasks along the way. In most newspapers, for example, when the journalist finishes writing a story and hands it to an editor, she relinquishes control over the story at that point. Editors may change the story. They certainly will decide where it goes in the newspaper. And they will write a headline for it. All of these tasks will be done without consulting the original writer. In fact, the next time that journalist will see her story is when it emerges in print. It may have survived intact but more often it is published in an abridged form with which the reporter may or may not agree. The point, however, is that the writer's control over her work is minimal.

Another element often lacking in the journalistic production process is time. Since the product in most cases is generated on a daily basis, deadlines are frequent and unyielding. Particularly when stories have a timely angle, they are often scheduled for publication only hours (sometimes minutes) after they are written. On that kind of a schedule, checking facts is a luxury.

Both the journalist's lack of control over the production process and lack of time mediate against fine tuning a story once it is written. But perhaps even more important a barrier to granting a scientist's or an engineer's request to examine the story prior to publication is a journalistic norm that strongly discourages prepublication censorhip.

The norm became established in a world where the vast majority of a journalist's sources are *not* scientists and engineers. Instead, they are politicians, business people, school board members, and football coaches. Experience quickly taught journalists that these individuals are often interested in presenting their best sides to the public, and a chance to "check facts" could quickly turn into an opportunity to censor the negative comments in a news story. Thus, refusal to show a story to a source became standard operating procedure in newsrooms. The practice is so universal that individuals regularly tapped as sources don't even ask. Many reporters with years of experience have never encountered such a request from a source.

Until they run into an engineer or a scientist. Among news sources, these people *are* aberrant. And journalists who aren't prepared for the fact-checking question quite naturally react negatively.

How does one deal with this situation? There seems to be no ideal solution. Some scientists and engineers ignore the journalist's dilemma and adopt a black-and-white approach: no manuscript review, no interview. This strategy, not surprisingly, drastically reduces the extent of the scientist's or engineer's contact with journalists. Other scientists adopt a policy at the opposite end of the spectrum by granting interviews with no accompanying request for fact checking. Scientists who have become popular media sources are well known for giving up such review. But, as Goodell indicates [7], they can afford to do so because they are highly skilled at controlling the accuracy of information by "talking in quotes," a strategy which ensures that much of their own phrasing gets into print.

Engineers and scientists between these two extremes often adopt strategies on the basis of negotiations with journalists. Rather than demanding prepublication review of the entire manuscript, they may set as a condition for an interview the right to check only the facts in a story, perhaps on the basis of having them read by the reporter over the phone. Others adopt a strategy of encouraging rapport with the journalist in the interview and then urging him to call with any questions during the writing stage.

Other scientists and engineers vary their strategy with the reporter. For example, they may demand the right to review facts before publication when dealing with an unfamiliar reporter but may not make that demand of reporters with whom they have worked in the past and whose skills they respect.

Journalists, too, are beginning to vary their procedure with different sources. Science reporters who have encountered the fact-checking request regularly have usually worked out strategies for dealing with it that sources find acceptable. Although submitting the entire story for review is rarely an option, these journalists are more likely to call engineers and scientists to check facts than are other reporters.

SUMMARY

Checking information for accuracy before publication is a good idea. But the scientist or engineer who decides to do so

WHEN IS A SCIENTIFIC FACT NOT A SCIENTIFIC FACT

FEEDBACK

WHEN is a scientific fact not a scientific fact? When it appears in a press release, apparently. Early this month a release on acid rain and fossil fuel from Britain's National Coal Board summarised the work of three scientists from the Institute of Terrestrial Ecology. One finding it quoted was: "Ozone naturally diffusing down from the stratosphere has been found to be a major factor in the death of some tree species . . ."

Naturally occurring ozone killing trees? Shouldn't the foresters of Europe know about this?

Sadly, the truth is more mundane. The actual words of the report the release was based on were: "we were in danger of ignoring the well-documented events in San Bernadino Valley, California, where ozone has been found to be a major factor in the death of some species of trees."

The authors then explain that ozone can occur naturally or as a pollutant when oxides of nitrogen combine with hydrocarbons in the presence of sunlight. This pollutant ozone "can be transported considerable distances and . . . in sufficient quantities can cause much damage to plants". Nothing about natural ozone killing trees, you notice.

Journalists should be aware of the risks of cobbling together different sentences and presenting them as a quote but at least one hack swallowed this piece of creative writing. Readers of *The Scotsman* are presumably living in fear of sea breezes destroying their potted plants.

Did the coal board feel apologetic? Not a bit of it. Peter Heap, the press officer who wrote the report, said: "I'm not a scientist, but I feel that I have accurately represented the views of the authors of the report."

Perhaps a scientist would think otherwise. □

'When is a scientific fact not a scientific fact', *New Scientist*, vol. 103, 30 August 1984, p. 42.

D. Nelkin

SCIENCE AND MEDIA: UNEASY RELATIONSHIP

Science and media: uneasy relationship

By Dorothy Nelkin
Special to The Globe

Press coverage of science has greatly increased in recent years, with at least 16 newspapers having added special sections for science features and science news. Popular science magazines have proliferated and they sell. Science-related policy issues – environmental and health risks, heart transplants, food and drug regulation, new scientific and technological developments – often appear as front page news.

Yet the relationships between scientists and journalists are strained. Scientists often accuse journalists of inaccurate, sensational and biased reporting. "Whatever we tell a reporter is bound to come out wrong," they say. Many scientists regard the press like politics, as a dirty business, threatening the purity of their work. They are fearful that their research will be misrepresented or that being featured in the press will jeopardize their relationship with their colleagues. Likewise, many journalists consider scientists unintelligible, uncommunicative and arcane.

A positive image

These attitudes are puzzling, for the images of science that appear in the press are usually optimistic, positive, even promotional. While the tone of optimism about science is sometimes tempered as promises are balanced with perils, the dominant message is clear. Scientists are problem solvers, authorities, the ultimate arbiters of truth. Science is a key to progress in our high-technology, information-based society, a solution for intractable dilemmas, a rational basis of public policy. It is a source of legitimacy, a means of certainty, an institution we can trust.

Even the reporting of problems in science idealizes the institution. For example, while consumer fraud is reported as a ripoff, scientific fraud is a scandal, a betrayal, a waste of talent that tarnishes and taints scientific institutions. Religious metaphors are common – scientists have "succumbed to temptation," committed a "scientific sin." This language idealizes science as a pure activity, a higher and an almost spiritual calling.

Reports on the role of science in situations of risk have attracted the greatest criticism. This is a difficult area of science reporting because of technical uncertainties and scientific disputes. But what is the image of science conveyed by reports on risk? A few quotes from the saccharin controversy are instructive: "The battle royale over the safety of artificial sweeteners is expected to be resolved by scientists next month." "Scientists will decide whether saccharin causes cancer." Scientists are the ultimate problem solvers, the expects, the discoverers of truth. While reporters who cover situations of risk often express disillusionment, it is the industries, utilities or government agencies that lose credibility. Science is represented as the way out, the solution to problems of risk.

science is described as an arcane activity outside of and above the sphere of normal understanding, and, therefore, beyond our control.

Independent investigation and critical inquiry are rare in this area of journalism. While the press publishes criticism of art, theater, music and literature, science is spared. While political writers aim to analyze and to criticize, science writers seek to elucidate and to explain. There are few Walter Lippmanns or I. F. Stones of science who write regularly in the press. Unaggressive in their reporting, many journalists are, in effect, retailing science more than investigating it, identifying with their sources more than challenging them.

Science can be conveyed in the press as an activity of an esoteric elite or an integral part of social life, as an uncontrollable endeavor or as the result of conscious choice. The public would be better served if both scientists and reporters encouraged a spirit of critical inquiry in science journalism that suggests the limits as well as the wonders of science and the nature of important policy choices. The role of science journalism, after all, is not to promote science but to enhance our ability to make informed judgments about research practices and policies that may affect our work, our health and the quality of our lives.

Nelkin is an author and Cornell University professor who is currently working on a book about science and technology in the press.

D. Nelkin, 'Science and media: Uneasy relationship', *Boston Globe*, 14 May 1984.

R. Schibeci

STUDENTS, SCIENCE AND THE MEDIA IN AUSTRALIA

A set of articles in *Search* has dealt with science and the media; for example, Parsons (1977) noted that scientists are generally dissatisfied with media coverage of science. A separate set of articles (in journals devoted to science education) has addressed the issue of students' perceptions of science and scientists; thus, MacDonald & Bridgstock (1982) examined the views of a sample of students at Griffith University, and found (as have many others) that the students' images are generally negative and stereotyped. More recently, however, attempts are being made to determine what relationships may exist between the presentation of science in the media on one hand and student perceptions of science and scientists on the other. That is, is it possible to draw causal inferences between images presented in the media and student images of science and scientists?

There has been much written about these general areas in both the USA and the UK (Schibeci, 1984) but this paper confines itself to studies conducted in Australia. Firstly, it reviews studies which have been conducted in Australia of science and the media. Secondly, it reviews Australian studies of student perceptions of science and scientists. Thirdly, it attempts to link these two areas of inquiry.

Science and the Australian media

A number of systematic studies of science and the Australian media have been conducted by Webb and his co-workers. These have included analyses of the following: the ABC productions *Towards 2000* (Zadnik & Webb, 1982) and *The Science Show* (Webb, Glover, Wild, Adeloju & Bowers, 1980), and newspaper coverage both of energy issues (Bascombe & Webb, 1981) and the 49th ANZAAS congress (Chivilo, Staniford, Unkovich & Webb, 1980).

The analysis of the *Towards 2000* television series resulted in the following conclusion: 'With its emphasis on the novel, unusual and extravagant, *Towards 2000* was afflicted by the "break through syndrome" that is all too common in media treatment of science and technology'. This 'gee whizz' syndrome is a feature of the portrayal of science and technology in the media in the USA (Schibeci, 1984). The analysis of the ABC's *Science Show* confined itself to a classification of segments of the program by disciplines (meteorology, biology, science policy and so on). No comment was offered on the image of science and technology being presented.

Bascombe & Webb (1981) noted, in an analysis of newspaper coverage of energy issues by the *West Australian* during a four-month period, that a relatively small proportion (0.41%) of the newspaper was devoted to energy issues, although in editorials and letters to the editor, energy issues were more evident. They also noted that energy conservation policies of various groups received little attention. In addition, Chivilo *et al* (1980) concluded that newspapers, in reporting on the 49th ANZAAS congress, appeared to confine themselves to presenting summaries of papers presented, with little evaluative comment. That is, few reporters availed themselves of the opportunity to interpret and analyse the papers. This possibility for 'in-depth' analysis, which is sometimes cited as a strength of the printed media, was certainly not exploited.

What can we say, on the basis of these studies, about science and the media in Australia? Obviously, the coverage given to science is relatively small, given its important influence on the community. This confirms the earlier observation made by Parsons (1977). Further, we can say that the little science that is portrayed is generally unrepresentative of the wide spectrum of scientific practice. That is, if a student were to rely exclusively on the media for a portrayal of science, the student would come away with a stereotyped view indeed. *Do* students have stereotyped images?

Student perceptions of science and scientists

We examine studies of the perceptions of science and scientists of Australian students at the primary, secondary and the post-secondary levels.

The author is lecturer in Science Education, School of Education, Murdoch University, Western Australia.

Post-secondary students

One study was found which reported perceptions of post-secondary students. This study was the one cited at the beginning: the study of the images of a group of students at Griffith University by MacDonald & Bridgstock (1982). These authors interviewed 55 students drawn from the five Schools of study at the university. They were thus able to probe beyond the responses normally given in self-report surveys. The students in the sample were asked to provide their views of university science students because the authors claimed that earlier research indicated that respondents generally did not distinguish between practicing scientists, science teachers and university science students. (The study reported above, of course, suggests that high school students *do* distinguish between science teachers and scientists.)

MacDonald & Bridgstock reported that the 'core' of the image of scientists was consistent with studies reported in other countries; that is, scientists were generally perceived to be narrow-minded, hard-working and job-oriented. In addition, however, they reported specific attributes revealed by their interview approach: scientists were perceived to be male chauvinists with Ocker attributes. The authors wondered about the source of the 'core' of the stereotyped image. They eliminated the science teacher as a source, because none of the respondents identified a teacher as the source of their images. Whatever the source, they argued that 'the most basic, widely held ideas about scientists are acquired well before tertiary level' (79).

One of the possible sources of these images is popular culture, and, in particular, the mass media. To this possible source we now turn.

Science, students and the mass media

A review of the overseas literature devoted to a study of images of science and scientists in popular culture reveals a number of important features. The first is that the image of scientists in popular culture is generally a negative one. Scientists are usually portrayed either as mad or so dedicated to truth that they were completely insensitive to their colleagues and families. These images are rarely counterbalanced by portrayal of 'normal' scientists, those who were neither mad nor impervious to human emotions. Secondly, the image of science portrayed in popular culture does not reflect the actual way in which science progresses. Thus, for example, the slow, painstaking way in which scientific knowledge is gradually built up is rarely portrayed. More commonly, the 'gee whizz' syndrome is evident in which the crucial breakthrough appears as a result of five minutes of serious thinking! This appears to be particularly true of material produced in the USA.

The limited number of Australian studies of science and the mass media reviewed in this paper does not lead to substantially different conclusions. It is interesting to speculate, therefore, on the possible influences of the mass media in shaping the early (and lasting) perceptions of students.

It would be attractive to suggest that the naive (or distorted) image of science and scientists in the media is responsible for students' negative (or distorted) images. However, we must be cautious in drawing such an obvious conclusion from the limited data reviewed in this paper. Indeed, it would be inappropriate to reach this conclusion even if the larger set relevant studies in the USA and the UK were to be included.

In a recent review of the literature on informal learning in science, Lucas (1983) divided out-of-school sources of learning into those which 'are intentionally established to teach, and those that teach accidentally' (3). Further, he noted that the intentions of the learner were important: in some instances, the potential learner actively seeks out the knowledge; in other instances, the learning occurs from an accidental encounter. According to Lucas, the press and electronic media generally 'provide instances of interactions where neither the producer nor the consumer intended to be involved in a science learning episode' (18). An obvious exception to this generalisation are materials produced with a specific educational intention, such as a science television program produced specifically for viewing during school hours.

Lucas (1982) described a number of ways in which interactions could occur between two sources of learning, *A* (the overt school curriculum) and *B* (an intentionally educative source or an accidental source of learning). For example, media use of 'mixed blood' and 'blood lines' may inhibit understanding and learning of the particulate gene theory in biology. On the other hand, *A* can inhibit learning from source *B* when oversimplified, but well-taught accounts of hormonal cycles in humans may produce confusion when viewing a more realistic TV program on the complexities of control of the reproductive cycle. The list continues: there are many ways in which *A* and *B* may interact. Further, studies of the influences of television on student learning have suggested 'that the interaction between this electronic medium and the learner are complex.

B. Beckworth

8.8

HOW MAGAZINES COVER SEX DIFFERENCE RESEARCH

A debate has continued for ten years now over genetic explanations for human social behavior. The controversy first centered on sociobiological theory; more recently the focus has been on sex hormones and brain structures as explanations for social differences, especially between the sexes.

Scientific journals have covered that debate to a certain degree, although much of their coverage is skewed in favor of genetic explanations and away from positions taken by their critics. Readers of *The New York Times*, for example, are usually informed about these issues through the filter of science writer Boyce Rensberger, a sociobiology enthusiast.

But what about the public in general? What do nonscientists or nonacademics know of "genes-and-gender" theory and the debate about it? Over the last five years, mass circulation magazines have taken on genes-and-gender science as a favorite topic. The ongoing debate over the validity of those theories, however, has not met with the same enthusiasm. Most of the popular press announce genes-and-gender "findings" without giving their readers an inkling of the existence of critics.

Genes-and-gender science has sold well in a whole spectrum of popular magazines. In 1981, *Newsweek* devoted six pages to the behavioral sex differences in an article titled "How They Differ — and Why." *Discovery* ran "The Brain: His and Hers" that same year, and *Science 82* followed with a "He and She" article the next year. *Science Digest* ran an 8-article feature in 1982, asking "Are Sexual Standards Inherited?" and answering with an enthusiastic yes. *Redbook* and *Parents* 1980 articles advised parents how to treat their sons and daughters in light of the "new research." *McCalls* printed its own study of maternal "baby hunger" in 1981 and followed up in 1982 with an article on how men cope with women whose urge have babies they don't feel or understand. *Ladies Home Journal* chimed in with a "fun piece" on women's maternal instinct. *Playboy* spent seven months in 1982 exploring biological explanations behind sex-role differences. *Cosmopolitan* published four articles on the topic that same year, with titles such as "Is Anatomy Destiny?" and "Why the Sexes Still Rage at Each Other."

Genes-and-gender articles span the full spectrum of popular magazines, from newsweeklies to science, sex, education and women's magazines. Most fail to mention there is no consensus among scientists that such connections make logical or empirical sense. In effect, such coverage panders to conventional sex-role prejudices by telling readers "science" supports their biases. Popular magazines use genes-and-gender theories as justification for keeping things as they are. The five most popular science magazines — *Science Digest*, *Science 84*, *Discover*, *Omni* and *Pyschology Today* — have printed genes-and-gender articles virtually ignoring the debate going on about the issue. Those five have a combined circulation of 2.3 million. In contrast, *Science for the People* which has extensively critiqued gene-based social theories, has a readership of approximately 4,000.

Popular science magazines seem eager to use genes-and-gender research to justify problematic social behavoirs. In 1982-3, *Science Digest* ran four articles treating genetic explanations for polygamy, rape, depression in women and the sexual double standard as hard news. As recently as May, *Science 84* posited as evolutionary adaptations female child battering, female infanticide and third-world nutritional neglect of women.

A One-Sided Picture

Sex magazines are equally intent upon applying genetics to sex roles. "Word has begun to leak out from the cool, impartial world of scientific inquiry," writes *Playboy* in 1982, "that men and women are chemically and behaviorally as different as two sides of the same coin." In its seven-article series, *Playboy* goes on to describe in detail the theories and data of 55 genes-and-gender researchers without citing a single critic. When the authors of that series were asked during a June 1983 televison debate why they had included no criticism in their presentation, they replied that this had not been their topic.

Barbara Beckwith is a freelance writer, former high school teacher, and longtime member of SftP.

sex-roles and sexual violence. A February 1981 *Playboy* concludes males are "compelled by their gender to be rogues" and advises men: "If you get caught fooling around, don't say the Devil made you do it. It's the Devil in your DNA." In April 1981, *Playboy* suggests rape is very likely "a strategy genetically available to low-dominance males that increase their chances of reproducing by making more females available to them than they would otherwise." The February 1981 *Psychology Today* makes the same suggestion. Genetic explanations for rape were fully critiqued by Val Dusek in the January/February 1984 *Science for the People*. But 960,000 more people will hear *Pyschology Today's* interpretation.

Even more alarming is *Cosmopolitan's* eagerness to justify male sexual violence. A May 1982 article promises to "provide a new perspective on human rape, wife-beating and other forms of sexual aggression." *Cosmopolitan* then describes an assortment of non-human "sexual tricksters," "mating game conflicts" and 'death by sex." The article concludes with advice to its (female) readers to take "guilt-ridden nightmares from the closet, sweep out tangled webs of Freudian fantasies, and simply have fun." The astonishing message: rape and wife-beating are dismissable as gene-based and fun.

Magazines whose readership is not sex-oriented focuses on sex-roles instead of sex. A 1980 *Commentary* article uses genes-and-gender research as proof that affirmative action quotas and textbook "indoctrination into sexual equality" should be stopped. A 198 *Education Digest* article, citing brain research, proposes setting up different learning sequences for boys and girls "to allow for their separate predispositions." Clearly, genes-and-gender research is popular in part becuase it justifies traditional sex differences. Another part of the explanation lies in how journalists view science in general.

Science Journalists: Not Critically Oriented

According to Rae Goodell, associate professor of science writing at the Massachusetts Institute of Technology, the press tends to take "an upbeat, 'science saves' view of science." "Journalists react with awe, excitement or resentment toward scientists, but all too rarely with common sense," writes Goodell in a November/December 1980 *Columbia Journalism Review* article." Since science journalism programs have generally not been critically oriented, according to Goodell, science journalists have allowed themselves to be intimidated by high-status scientists. "Most science journalism education has a trade orientation that views science news as value-free, apolitical hard news." Such an education doesn't prepare journalists to look for more than one side to a science story.

Ohio University journalism proffessor Sharon Dunwoody points out that science writers tend to work in "inner-circles," sharing story ideas, sources and background information. As a result, they often publish the same news instead of making independent judgements about what is worth printing, writes Dunwoody in a 1980 *Science, Technology and Human Values* article.

An Historical Parallel

A striking parallel with the current popularization of genes-and-gender science is found in the early part of this century when popular magazaines promoted the theories of the newly founded "science" of eugenics. Led by *Popular Science Monthly* edited by James McKeen Cattell (a major figure in the mental testing movement), these magazines inundated the public with articles describing the latests "findings" on the genetic basis of criminality, racial differences in intelligence and the genetic inferiority of many immigrants. For instance, a series of articles was published in the years 1913-1915 in Cattell's journals with the titles "Going through Ellis Island," "A Study of Jewish Psychopathy," "Mendelian Inheritance of Feeblemindedness," "The Biological Status and Social Worth of the Mullato," "Heredity, Culpability, Praiseworthiness and Reward," "Eugenics with Special Reference to Intellect and Character," "Immigration and the Public Health," "A Problem in Educational Eugenics," "Women in Industry," "Economic Factors in Eugenics," "The Racial Element in National Vitality," "Eugenics and War" and "Families of American Men of Science." All promoted the ideas that social behavior and aptitudes were genetically determined. These popular articles included ones written by leaders of the eugenics and mental testing movements, such as E. C. Thorndike, C. B. Davenport and Cattell himself.

Similarly, *Atlantic Magazine* in 1914 published "The Decadence of Human Heredity" suggesting reduced "multiplication of the defective classes" and increased fecundity of Harvard and Yale graduates. The January, 1914 issue of *Outlook* featured an article by Theodore Roosevelt, "Twisted Eugenics," which urged sterilization of eugenics to prevent the "race" from dying out. The *Saturday Evening Post* was also a great fan of the eugenicists, including articles such as "The Great American Myth" and "Plain Remarks on Immigration from Plain Americans" (in 1924) which called on Americans to "breed the black sheep out of our flocks." Other popular magazines, newspapers and the respected *Scientific American* also presented the views of the eugenicists uncritically.

The result of this barrage of uncontested eugenics propaganda in the popular media was to provide strong popular support for the sterilization laws, miscegenation laws and for the Immigration Restriction Act of 1924 which tremendously reduced the influx of immigrants from the supposedly "inferior" nations. Let us hope that the current wave of enthusiasm for biological determinist theories among the contemporary couterpart of the magazines of the early 1900's does not lead to similarly strong consequences.

— Jon Beckwith

McCalls: "How Different Are Girls and Boys?" by Dr. Lee Salk, 9/79.
"Baby Hunger" by Lois Leiderman Davitz, 11/81.
"How Men Really Feel About Babies" by Lois Leiderman Davitz, 6/82.

Ms.: "The 'Math' Gene and Other Symptoms of the Biology Backlash" by Nancy Tooney, 9/81.

"Watch Out: Your Brain May Be Used Against You" by Vivian Gornick, 5/82.
"Tired of Arguing About 'Natural Inferiority'?" by Naomi Weisstein, 11/82.

Newsweek: "Just How the Sexes Differ," 5/18/81.

Omni: "Sex and the Split Brain," 8/83.
"Animal Feminism" by C. Johmann, 8/83.

Parents: "The Truth About Sex Differences" by Susan Meunchow, 2/80.

Playboy: "Darwin and the Double Standard" by Scott Morris, 8/78.
"Why Do Men Rape?" by Richard Rhodes, 4/81.
"The Sexes: A Mystery Solved?" by Jo Durden-Smith and Diane de Simone, 1/82.
"The Sexual Deal: A Story of Civilization" by Jo Durden-Smith and Diane de Simone, 2/82.
"The Sex Life of the Brain" by Jo Durden-Smith and Diane de Simone, 3/82.
"The Sex Chemicals" by Jo Durden-Smith and Diane de Simone, 4/82.
"The Perils of Paul, the Pangs of Pauline" by Jo Durden-Smith and Diane de Simone, 5/82.
"The Main Event" by Jo Durden-Smith and Diane de Simone, 6/82.
"Prisoners of Culture" by Jo Durden-Smith and Diane de Simone, 7/82.

Psychology Today: "Sexual Selection in Birdland" by D.P. Barash, 3/78.
"Eros and Alley Oop," David Symons interviewed by Sam Keen, 2/81.

Quest: "Male and Female—Why?" by Jo Durden-Smith, 10/80.

Reader's Digest: "The Other Difference Between Boys and Girls" by Richard Restak, 11/79.
"Is There a Superior Sex?" by Jo Durden-Smith and Diane de Simone, 11/82.

Redbook: "Should We Treat Our Son and Daughter Just Alike?" by T. Berry Brazelton, 8/80.

Science 82: "He and She" by Melvin Konner, 9/82.
"Symons Says" by Roger Bingham, 1-2/83.
"Margaret Mead: The Nature-Nurture Debate and On Being Human" by Boyce Rensberger, 4/83.

Science Digest: "Sexual Choices" by Mary Batten, 3/82.
"Sociobiology: Rethinking Human Nature" by Richard Nalley with contributing articles by Mary Batten, Nancy Solomon, E.O. Wilson, Rachel Wilder, 7/82.
"Sex Machines" by Duncan Anderson, 4/82.
"Birth of Your Sexual Identity" by Jo Durden-Smith and Diane de Simone, 9/83.

Time: "Why You Do What You Do: Sociobiology, A New Theory of Behavior," 8/1/77.
"Male Dominance Revisited" by John Leo, 9/22/80.

B. Beckworth, 'How magazines cover sex difference research: Journalism abdicates its watchdog role', *Science for the People*, July/August 1984, pp. 18–23.

B. L. Welch

DECEPTION ON NUCLEAR POWER RISKS

8.9

There has been an effort on the part of the atomic energy establishment—industry and government—for the past 20 years to hide from public view the risks inherent in nuclear power.
—Harold P. Green

I consider this bureaucracy [AEC and its successors] about the most arrogant and contemptuous of public interest among federal agencies, as manifested by its . . . ready distortion of facts to fit the agency's position, as in the "Executive Summary" of the Rasmussen report on reactor safety.
—George B. Kistiakowsky

Substantial evidence suggests that for a quarter of a century the U.S. federal bureaucracy and the nuclear industry have deliberately deceived the public about the risks of nuclear power.[1,2] Facts appear to have been systematically withheld or distorted and calculations biased in order to present nuclear power in a favorable light. Discrepancies exist between what responsible scientists told the public—and sometimes the Congress—about the risks of nuclear power and what they knew the information available to them actually suggested.

The scientific community has a responsibility to assure that these violations of the public trust and of scientific ethics are formally recognized and defenses against their repetition are erected.

Most persistent and flagrant have been the attempts to

- "normalize" the public's perception of nuclear accident casualties by comparing them with those resulting from more familiar accidents, emphasizing only early fatalities while ignoring or downplaying the major effects of nuclear accidents, namely, delayed cancer and genetic change; and
- use pseudo-quantitative statistical probabilities in order to make it appear that the chance of a serious nuclear accident is negligibly small.

Prime examples of these abuses are found in the 1975 Atomic Energy Commission's *Reactor Safety Study* (hereafter referred to as the *Study*).[3]

Seven months before the draft of the $4 to $5 million *Study* was released in August 1974, and before the calculations were even completed, scientists began selectively citing preliminary results to Congress and the public without qualification. This misleading use of the *Study* began on January 13, 1974 with national press conferences at which AEC scientist-administrators equated the expected consequences of a nuclear accident to those of a large airplane crash, and the chance of such an accident occurring to that of being killed by a meteor or of being bitten by a poisonous snake while crossing a busy street in Washington, D.C.

Before the National Press Club on January 21, 1974, the AEC Chairman stated that "the melting of a nuclear reactor core is not an extraordinarily large accident, as some have believed." Ten days later before the Joint Committee on Atomic Energy (JCAE) the Chairman again downplayed the consequences of a nuclear accident: "If a meltdown should occur, the *Study* indicated that it would not be an extraordinarily large accident."

On May 16, 1974, the *Study*'s director and the AEC Chairman (again in JCAE hearings) stated that the general conclusions of the emerging *Study* were that the risk of, and from, nuclear accidents is minimal, that the most likely consequence is "quite modest, in fact, no worse than many other kinds of accidents such as fires and airplane crashes that society has experienced."

This same theme dominated the press statements and testimony in Congress that accompanied the formal release of the draft and final *Study* reports, respectively, in August 1974 and October 1975.

In none of the testimony or releases were the limiting assumptions of the *Study* stated. It was not stated, for example, that projected accident consequences were based solely upon the number of *early* fatalities. Delayed consequences to health were ignored. This was so although:

- The threat to health of radiation exposure was known from experience in World War II and from extensive controlled experiments conducted in the national laboratories under AEC jurisdiction.
- Bernard Pasternack, an AEC consultant on the formulation of casualty estimates for reactor accidents, had in 1964 emphasized the question, "Should not some allowance be made for . . . long-term effects of radioactive materials?"
- In JCAE hearings on January 24, 1974 and March 28, 1974, and also earlier in personal conversations with key AEC administrators and *Study* staff scientists, I had protested

fatalities due to other natural and man-caused accidents.[8] *Science* also quoted the *Study* director's estimate of "the 'bottom line,' a core meltdown, followed by failure of all backup safety systems; all during the worse possible weather conditions. . . .This, Rasmussen said, could lead to some 2,300 deaths." Unmentioned was the fact that these were only the *early* deaths; that this was only a worst "calculated" accident, not the worst possible; and that it did not (despite the *Study* director's claim to the contrary) assume the "worst possible weather."

Prompted by Representative Morris K. Udall, the Nuclear Regulatory Commission appointed an ad hoc risk assessment review panel in 1977 to review the *Reactor Safety Study*. Panel findings were released in September 1978, and resulted in an NRC policy statement of January 19, 1979, which stated that the Commission:

- "does not regard as reliable the *Reactor Safety Study*'s numerical estimate of the overall risk of a reactor accident."
- "withdraws any explicit or implicit past endorsement of the Executive Summary . . . a poor descriptor of the contents of the report [which] has lent itself to misuse in the discussion of reactor risks," and that
- "cogent comments from critics either were not acknowledged or were evaded and that, in general, the record of response to valid criticism was weaker than it should have been."

Yet, the panel, although critical of the *Study*, was in many ways overly generous:

- The panel acknowledged that the "Executive Summary," "which is by far the most widely read part of the report among the public and policy-makers, does not adequately indicate the full extent of the consequences of reactor accidents," labeling it a "poor descriptor" of the *Study*, and one which lent itself to "misuse." But no indication was given of the remarkable extent to which the "Executive Summary" was misleading, nor of the extent to which it actually was used to deceive.
- The panel noted that many of the probabilities entered into the *Study*'s calculations were "subjective"; "the accuracy of many of the absolute probabilities calculated therein are not as good as claimed"; "the analysis suffers from a spectrum of problems, ranging from lack of data . . . to the invention and use of wrong statistical methods"; and "one key deficiency is the use . . . of some methodological and statistical assumptions that lack credibility." Yet, the review panel characterized the *Study* as a "conscientious and honest effort *to apply the methods of fault-tree/event-tree analysis to an extremely complex system, a nuclear reactor*" (italics mine).

Literally, the latter statement applies only to the *Study*'s analysis of reactor failure and the consequent release of radioactivity. But it has been widely quoted out of context, giving the erroneous impression that the review panel had judged the *Study* to represent a "conscientious and honest effort" in the broadest sense—a subject on which members of the review panel disagreed.[9,10]

The panel in no way absolved either the repeated failure of the scientists involved in the *Study* to publicly acknowledge the inherent limitations of their data, or the serious problems of probability splitting, consequence reduction and misleading presentation and representation of the *Study*'s results.

Neither the staff, contracting scientists, nor the administering AEC scientists involved, in view of their general professional competence and experience, could be considered naive. Thus, it is difficult to avoid the conclusion that they knew well that the "Executive Summary," the public statements about the *Study* and the assurances to the Congress painted a misleading picture of the *Study*'s merits and of the risks and probable consequences of a nuclear accident.

The repeated claim of AEC and contracting scientists that a nuclear core meltdown need not be considered a major accident could hardly have been made in ignorance of the *Study*'s estimate that the radioactivity released to the environment would be sufficient to potentially cause "maximum calculated consequences" in one meltdown out of ten, depending only upon the size of the surrounding population and the prevailing weather. This chance for a maximal release of radioactivity in the event of a meltdown so closely approximates certainty that to make the suggestion that a core meltdown is other than intolerable is deceptive and irresponsible.

While the *Study* was in process, claims were repeatedly made that it was being conducted *at* the Massachusetts Institute of Technology and was "independent" of the AEC. While the chairman of MIT's nuclear engineering department was the titular "director" of the *Study*, his involvement was part-time and distant; the *Study* was actually conducted at AEC headquarters in Germantown, Maryland, largely by, and under the routine supervision of, long-term members of the AEC staff. On the basis of AEC documents released under the Freedom of Information Act to the Union of Concerned Scientists and interviews with key NRC officials, *Science*, in world's most lethal non-nuclear explosion.

• A nuclear accident that caused 110 early deaths might cause 70,000 non-malignant and 31,500 malignant thyroid neoplasms and 14,000 cancer deaths. The latter is eight times more deaths than caused by all airplane crashes within the U.S. in one year.

• The "worst calculated accident, after causing 3,300 early deaths and 45,000 cases of early radiation sickness, might cause 250,000 thyroid neoplasms and 45,000 deaths from cancer. These deaths approximate the annual U.S. motor vehicle accident toll, or all U.S. deaths in the Vietnam war.

7. For instance:

• The reference accident for the "largest calculated consequences" involved an exposed population of "10 to 15 million people." But in calculating casualties, it was arbitrarily assumed that only 10 million were exposed.

• The upper bound for the probability of a core meltdown was estimated as one chance in 3,333 per reactor-year. But risk calculations arbitrarily assumed a meltdown frequency of one chance in 20,000 per reactor-year.

• The *Study* said that a capability to provide the intensive supportive medical care that would be essential for the survival of the many cases of acute radiation sickness in even a moderate sized nuclear accident does not exist, and "isn't even well planned." Yet, in calculating casualties the availability of such care was assumed.

• The casualty figures cited were only the *average* of the number of casualties expected at 100 present and prospective reactors distributed at 68 sites nationwide. Yet, the figures on which the averages were based sometimes varied by more than a factor of 250.

• Casualty calculations used weather data collected at reference reactor sites during only one arbitrarily selected year. Even within that year, no attempt was made to recognize the worst weather that actually occurred. Yet, the number of casualties during a reactor accident is highly dependent upon weather.

• Casualty calculations assumed an evacuation model which was known to be inapplicable to a major population center. Yet under some conditions a city may sustain more casualties from an accident occurring at a reactor 25 to 30 miles distant than within the city itself.

• Casualty calculations assumed that during an accident evacuation within 5 to 25 miles of the reactor would be necessary within only a 45 degree arc; that evacuation would be unrealistically rapid; and that the wind would not shift and that there would be no wind shear.

• An unjustifiable proliferation of arbitrary subdivisions of population and weather, and the use of the invalid assumption that weather and population exposure to radiation are statistically independent made the calculated probability of a maximum consequence accident unduly small.

• The *Study*'s assumptions about the long-term biological effects of radiation did not reflect the full range of conservative professional opinion.

• Risk calculations assumed negligible contributions to overall risk by fire, floods, earthquakes, tsunami, tornadoes, hurricanes, external power supply failure, human error, common mode failures, basic deficiencies in reactor design or acts of violence.

8. *Science*, "Nuclear Safety: Calculating the Odds of Disaster," 185 (1974), 838.

9. F. von Hippel, statement before House Committee on Interior and Insular Affairs, Subcommittee on Energy and the Environment," 96th Cong., 1st Sess., *Reactor Safety Study Review* (Feb. 26, 1979), 199-209.

10. The chairman of the review panel, while conceding that the "Executive Summary" was "used as a political instrument" called the *Study* itself a "big, conscientious, honest job." See Harold Lewis, statement, *Reactor Safety Study Review*, 30-33, 41-45, 123-128.

11. It was concluded that the AEC chose the *Study's* director

> "not only for his technical knowledge and national stature but because it regarded him as a 'friend' of nuclear power. . . . The staffers assigned to the study were apparently poorly insulated from bureaucratic pressures that could undermine the study's integrity. . . . Evidence suggests that the atmosphere and circumstances under which the Rasmussen study was conducted were anything but conducive to obtaining an impartial study" [*Science*, "Reactor Safety: Independence of Rasmussen Study Doubted," 197 (1977), 29].

An early communication to the AEC was also cited in which the *Study's* director acknowledged that "the sensitive nature of these studies will require careful control of all official information releases"; and an AEC memo quoted which stated: "We have a role . . . in . . . helping with the matters of tone, credibility and appearances." It was judged "evident from the AEC documents that some staffers who worked on the study were highly sensitive to the fact that, depending upon their nature, their findings could undermine public confidence in nuclear power." Memoranda were also quoted which expressed concern that:

> "The facts may not support our predetermined conclusions. . . . Our RSS report must find that the A-E [the architect-engineer], the licensee, the vendors and the AEC did an adequate job . . . to assure safety and reliability. . . . What information do we need to gather to support such a finding?. . . The information we seek should . . . serve to engender confidence about the AEC's role in assuring high quality workmanship and Q-A [quality-assurance] practices; it should not have the effect of raising unanswerable questions."

B. L. Welch, 'Deception on nuclear power risks: A call for action', *The Bulletin of the Atomic Scientists*, September 1980, pp. 50–4.

V. Godin

THE MEDIA ARE THE MEDIUM

Closing the gap between the nuclear industry and the public

by Victor Godin

In 1959, the writer C.P. Snow delivered a lecture at Cambridge entitled *The Two Cultures and the Scientific Revolution* in which he discussed the barriers that had arisen between the literary or communication culture and the scientific culture. In his lecture, he insisted that closing the gap between the two communities is essential because when they have grown apart "no society is going to be able to think with wisdom."

Snow, a popular fiction writer and a scientist, was uniquely capable of understanding both cultures. At Cambridge in the 30s and 40s, he was, in his own words, "Privileged to have a ringside view of one of the most wonderful, creative periods in all physics."

He believed the literary segment of society, because of its increasing incomprehension of science, was shifting from unscientific to anti-scientific. After listening to his literary

Victor Godin: Both the media and the public have changed since the days of the early 1960s when science could do no wrong.

manipulated by fear. Richard Wirthlin, who is generally recognized as a genius among public opinion pollsters, entrenched the politics of fear in the 1980 U.S. presidential election. Wirthlin's recipe was new and simple. Instead of taking polls to find out what people wanted and then making promises, why not take polls to find out what they were afraid of and then soothe their fears? The strategy worked well in the 1980 U.S. election and again in 1984. The polling industry rushed to imitate this success.

The name of the game in the media today is entertainment, and the best entertainment plays on fear.

History reveals that one of society's fears relates to new technology. The contemporary media, as part of the entertainment industry, understand that one of the surest ways to achieve high ratings is to play on public fear of technology. This is not to suggest all investigative reporting is irresponsible. That would be unfair. But the name of the game in the media today is entertainment, and the best entertainment plays on fear.

A 1984 CBC Radio story, entitled *"Canada and the Bomb – The Korean Connection"* illustrates this phenomenon. On the basis of several interviews held with people in the industry – interviews focusing on technical discussions between Canadian and Korean scientists in the area of electricity production – the program, *Sunday Morning*, aired a story suggesting Canadians were helping Korea develop a nuclear weapon. CBC reporters had spent hours at Chalk River talking to Atomic Energy of Canada Limited personnel, but not one word was quoted in the program.

On that particular Sunday, my home telephone rang virtually nonstop. Eager reporters from all over Canada were determined to get (pardon the pun) an explosive angle. Of the dozens of calls, not a single reporter was remotely interested in technical information. They wanted insights on intrigues, secret deals and international pressures. Although a simple explanation of this project was available and repeated, it was ignored. The reporters had made up their minds, and their only objective was to find somebody unwary enough to give them the ammunition they needed. No reporter was successful in getting incriminating information because it didn't exist. The allegations were unfounded. That did not prevent the headline writers and the cartoonists from cranking out material which ranged from hysterical to racist. Fear sells, facts don't.

Given this background, it's easy to become pessimistic, but much can be done to remove the fear element from media coverage and introduce facts.

First, the nuclear industry must face the fact that an overwhelming 75% of Canadians form opinions on the basis of media coverage. That's where the industry must make its yards. The issue of public opinion must be put into perspective, however. For years, the industry has been told that 50% of Canadians are opposed to nuclear energy. Surprise! That does not appear to be true and likely never was. Using a common practice in public opinion sampling on nuclear issues, the pollsters grouped people who are concerned about nuclear power with those who are opposed, making no attempt to segregate. Most people are likely concerned about highways, but not opposed to them.

A recent Gallup Poll, using modified criteria, found that approximately 17% of the Canadian public is firmly opposed to nuclear power with an opinion based on knowledge. About the same number are absolutely in favor of nuclear power, but the vast majority, more than 60%, sit on the fence. This 60% of the public will be influenced mainly by the media, so as a first step, they must be given undistorted information through the media before they will feel comfortable with the industry.