The Mangle of Practice: Agency and Emergence in the Sociology of Science
Author(s): Andrew Pickering
Source: *American Journal of Sociology*, Vol. 99, No. 3 (Nov., 1993), pp. 559-589
Published by: The University of Chicago Press
Stable URL: https://www.jstor.org/stable/2781283

The Mangle of Practice: Agency and Emergence in the Sociology of Science[1]

Andrew Pickering
University of Illinois

Some difficult but important issues have arisen in recent social studies of science concerning temporally emergent phenomena and the decentering of the human subject in scientific practice. This essay seeks a constructive clarification of the issues, and links them together, by delineating and exemplifying a view of science as a field of emergent human and material agency reciprocally engaged by means of a dialectic of resistance and accommodation—the mangle.

There is at all times enough past for all the different futures in sight, and more besides, to find their reasons in it, and whichever future comes will slide out of that past as easily as the train slides by the switch. [William James, *The Meaning of Truth*]

Desire only exists when assembled or machined. You cannot grasp or conceive of a desire outside a determinate assemblage, on a plane which is not pre-existent but which must itself be constructed. . . . In retrospect every assemblage expresses and creates a desire by constructing the plane which makes it possible and, by making it possible, brings it about. . . . [*Desire*] *is constructivist, not at all spontaneist.* [Deleuze and Parnet, *Dialogues*]

The sociology of science has always been somewhat marginal to the discipline as a whole, perhaps because of the peculiar difficulties of its subject matter, but perhaps because its specifically sociological features have not

[1] I have been lucky enough to receive much stimulating feedback while preparing this essay. Besides four referees of earlier drafts, I thank David Bloor, John Bowers, Geof Bowker, Nancy Cartwright, Soraya de Chadarevian, Norman Denzin, Irving Elichirigoity, Paul Forman, Dilip Gaonkar, Yves Gingras, Laurel Graham, Mary Hesse, Robert Alun Jones, Bruce Lambert, Bruno Latour, John Law, Peter Lipton, Michael Lynch, Ted O'Leary, Peter Miller, Malcolm Nicolson, Michael Power, Diederick Raven, Simon Schaffer, Steven Shapin, Barbara Herrnstein Smith, Leigh Star, Stephen Turner, and Adrian Wilson. Earlier versions have been presented at the Centre for History of Science, Technology and Medicine, Manchester University, the Science Studies Unit, University of Edinburgh, and the Centre de Recherche en

0002-9602/94/9903-0001$01.50

appeared very interesting. While the philosophical import of sociological analyses of scientific knowledge has sometimes seemed scandalous, their sociological import has remained unremarkable.[2] Recently, however, science studies has become sociologically contentious. Ethnographic, ethnomethodological, reflexive, and actor-network critiques of earlier approaches to the sociology of science, and of the traditional sociological frameworks that they implement, have started to proliferate.[3] The representatives of the actor-network approach have been most outspoken. Bruno Latour writes that "the social sciences are part of the problem, not of the solution," and that "we strongly reject the helping hands offered us by the social sciences" (1988*b*, pp. 161, 165). And, at greater length, Michel Callon suggests that "to transform academic sociology into a sociology capable of following technology throughout its elaboration means recognizing that its proper object of study is neither society itself nor so-called social relationships but the very actor networks that simultaneously give rise to society and to technology. . . . This notion makes it possible to abandon the constricting framework of sociological analysis with its pre-established social categories and its rigid social/natural divide" (1987, pp. 99–100).

Things are happening, then, in the sociology of science that might be of interest to the discipline more generally, and my aim in this essay is

Histoire des Sciences et des Techniques, la Villette, Paris, and at a Department of Accounting and Finance workshop, "The Components of Practice," at the London School of Economics. I am very grateful for comments and discussion at each. The present draft was completed while I was at the Department of History and Philosophy of Science of the University of Cambridge, England, and my work on it was partially supported by National Science Foundation grant SBE91-22809.

[2] Thus the Mertonian approach to the sociology of science was a self-conscious continuation of the structural-functional approach that then characterized mainstream sociology (see Zuckerman [1988] for an extended review). The sociology of scientific knowledge (SSK, alternatively designated as the strong program, relativism, or constructivism) departed from it in seeking to offer an analysis of scientific knowledge itself and thus created some much-needed philosophical turmoil, but again drew upon stock sociological resources: Bloor (1991, 1983) elaborated a Durkheimian vision of the social constraints upon knowledge production, while Barnes (1977, 1982) and Shapin (1979, 1982) followed more Marxist or Weberian lines in characterizing scientists' agency in terms of interests. Collins's work (1992) was somewhat ambiguous in this respect, combining ethnomethodological sensitivities at the microlevel with an interest model at the macro. For the direct extension of SSK into the analysis of technology, see Pinch and Bijker (1984).

[3] For examples, see Traweek's (1992) ethnographic account of particle physics, Lynch's (1992*a*, 1992*b*) argument with Bloor (1992) over ethnomethodological vs. SSK-type readings of Wittgenstein, and the three-way argument between Collins and Yearley (1992*a*, 1992*b*), Woolgar (1992), and Callon and Latour (1992) over the relative merits of, respectively, SSK, reflexivity, and the actor-network approach. (For an earlier phase of the argument between reflexivity and the interest-model version of SSK, see Woolgar [1981*a*, 1981*b*], Barnes [1981], and MacKenzie [1981*a*].)

a constructive clarification of just what. Two points are, I think, clear, both of which are touched on in the quotation from Callon. First, as I will develop it, the critique of traditional sociologies of science depends on a heightened sense of time.[4] The idea that we should try to understand scientific practice in its temporal unfolding is a central theme of recent science studies, and along with this go doubts about traditional explanatory repertoires that center on enduring causes of action. Studies of scientific practice point instead to the *temporally emergent* structure of scientific research. Second, and more spectacular, the critique reflects an increasingly widespread conviction that the analysis of science calls for a decentering of the human subject. As a discipline, sociology has traditionally focused on human individuals and groups as the locus for understanding and explanation, and what is suggested here is a kind of *posthumanist* displacement of our interpretive frameworks.[5] Beyond this, though, things get murky. How we should conceptualize temporally emergent phenomena, how the posthumanist turn is to be accomplished, and how emergence and the displacement of the human subject are related to one another remain unclear, subject to confusion and debate. In what follows, I try to sort out these issues by delineating a general

[4] From one angle this can be seen as a product of the ethnographic studies of scientific practice that began in the late 1970s (with Fleck [(1935) 1979] as a notable precursor): see Latour and Woolgar (1986), Knorr-Cetina (1981), and Lynch (1985). From another angle, it appears as a continuation of the concerns of SSK; see Pickering (1984*b*) and Gooding (1990). Yet another source has been pragmatist studies of science (which take the work of Howard Becker and, esp., Anselm Strauss as their point of departure): see Star (1991, in press) and Fujimura (1992).

[5] By "posthumanist" I want to point to a displacement of the human subject from the center of sociological accounting rather than to an "antihumanist" effacement of the human subject. I finally grasped the significance of this move in conversations with John Law and in reading his recent work (Law 1993, e.g.). Barbara Herrnstein Smith also helped me by pointing out a "residual humanism" in a previous draft of this essay (the residue is still there: see below). That humanism is at the center of contention is made clear by Collins and Yearley (1992*a,* 1992*b*). Besides the further works cited below, my development of the posthumanist move owes a lot to Haraway (1991), Deleuze and Guattari (1987), and, more obliquely, to the writings of Michel Foucault. Foucault (1972) provides a general elaboration of the themes of temporal emergence and the displacement of the human subject, though how he connects these themes is not clear to me. I link them below in a discussion of agency, a topic on which Foucault displays a principled reluctance to speak. To make a connection between my analysis of scientific practice and Foucault's analysis of disciplinary mechanisms—see esp. Foucault (1979) and subsequent work in the analysis of the social sciences, management, and accountancy by Anthony Hopwood, Peter Miller, Ted O'Leary, and Nikolas Rose (Hopwood 1987; Miller 1992; Miller and O'Leary, in press; Rose 1990; Rose and Miller 1992)—one needs only to think of disciplinary apparatuses as machines for capturing, channeling, and framing human agency (see below).

understanding of the structure of scientific practice which, for reasons explained later, I call *the mangle.*

To begin to explain the mangle by means of some of the salient issues, it is convenient to start with the theme of posthumanism. Traditional sociology of science, like traditional sociology more generally, is humanist in that it identifies human scientists as the central seat of agency. Conversely, traditional sociology of science refuses to ascribe agency to the material world (the introductory paragraphs of Pickering [1984*a*] actually express this perspective rather well). Here I subscribe to the basic principle of the actor-network approach: I think that the most direct route toward a posthumanist analysis of practice is to acknowledge a role for nonhuman—or material, as I will say—agency in science.[6] Science and technology are contexts in which human agents conspicuously do not call all the shots. But thinking about material agency and its relation to human agency proves tricky, and I will follow the spirit rather than the letter of the actor-network approach, at least as it is presently articulated by Callon and Latour. To see where the difficulties lie, we can turn to a recent critique of actor-network theory by Harry Collins and Steven Yearley.

Collins and Yearley (1992*a*, 1992*b*) seek to defend the classically humanist orientation of traditional sociology of scientific knowledge, which accords priority to the human subject through an asymmetric distribution of agency—all to human beings, none to the material world. To do so, they construct a dilemma that, they claim, faces anyone who wants to attribute a role to material agency in science. As analysts, they say, we have just two alternatives. We can see scientists as producing *accounts* of material agency, in which case these accounts fall into the domain of scientific knowledge and should be analyzed sociologically as the products of human agents, or we can try to take material agency seriously—on its own terms as it were—but then we yield up our analytic authority to the scientists themselves: scientists, not sociologists, have the instruments and conceptual apparatus required to tell us how material agency really is. The upshot of this dilemma, therefore, seems to be that any sociologist with a shred of self-respect had better stick to humanist analysis of scientific accounting for material agency and had better not incorporate material agency per se into her interpretive schemes.[7] Callon and Latour do

[6] For recent important presentations of the actor-network position, see Callon (1991), Callon and Latour (1992), Latour (1987, 1988*a*), and Law (1993).

[7] The denial of material agency serves not just to defend humanist sociology but, oddly enough, to make an alliance with mainstream philosophies of science that are themselves otherwise savaged by SSK. Mainstream philosophy of science, too, seeks to keep material agency at arm's length, preferring to talk only of accounts of agency

not see things quite this way, and neither do I, but here our positions diverge.

Callon and Latour (1992) reject the prongs of Collins and Yearley's dilemma. They insist, rightly I think, that there are not just two alternatives in the treatment of nonhuman agency. And their position, if I have understood it correctly, is this.[8] We should not see nonhuman agency in the terms offered us by scientists or by humanist sociologists of scientific knowledge. Instead we should think *semiotically*. Semiotics teaches us how to think symmetrically about human and nonhuman agents. In texts, agents (actors, actants) are continually coming into being, fading away, moving around, changing places with one another, and so on. It is important that their status can easily make the transit between being real entities and social constructs, and back again. Semiotics thus offers us a way of avoiding the horns of Collins and Yearley's dilemma: the agencies we speak about are semiotic ones, not confined to the rigid categories that traditional thought imposes.

This is a clever and ingenious response, but it brings with it two different kinds of problem. One concerns human agency, and I return to it below. The other concerns material agency. One of the most attractive features of the actor-network approach is that its acknowledgment of material agency can help us to escape from the spell of representation. Traditional accounts of science take it for granted that the end of science is to produce representations of how the world really is; in contrast, admitting a role for material agency points to the fact that, in common with technology, science can also be seen as a realm of instruments, devices, machines, and substances that act, perform, and do things in the material world. Or so I am inclined to think: this essay is actually part of an attempt to understand science as a field of performative material devices (and to understand scientific representation in relation to those devices rather than in its usual splendid isolation).[9] From this per-

as items of the theoretical culture of science. There is an alternative tradition in contemporary philosophy, though, that insists that one cannot make sense of science without talking about nonhuman agency (or powers, tendencies, dispositions, or capacities): see Bhaskar (1975), Cartwright (1989), Chalmers (1992), Harré and Madden (1975), and Mellor (1974). I thank Peter Lipton for introducing me to this body of writing; it shades into studies of scientific practice in the work of such authors as Baird (1993), Baird and Nordmann (in press), Hacking (1983, 1992), and Rouse (1987).

[8] Let me say in advance that I do not feel too confident of my grasp of Callon and Latour's position (and neither, I suspect, do many of their critics and admirers). The argument that follows, and further arguments elsewhere in the text, might turn out to be hairsplitting. I hope that, even so, this essay can help to clarify matters, if only by marking some new routes across actor-network theory's terrain and by supplying a new example and analysis.

[9] See also Pickering (1989) and Pickering and Stephanides (1992). In philosophy of

spective the appeal to semiotics in the face of Collins and Yearley's dilemma looks like a kind of retreat, a return to the image of science-as-representation that one does not wish to make.[10] Fortunately, as I shall now explain briefly in anticipation of the empirical example to be discussed later, there is another way of steering our way around the humanists.

The trick is to link the posthumanist move to my other theme of temporal emergence. We can take material agency as seriously as traditional sociology has taken human agency, but we can also note that the former is *temporally emergent* in practice. The contours of material agency are never decisively known in advance, scientists continually have to explore them in their work, problems always arise and have to be solved in the development of, say, new machines. And such solutions—if they are found at all—take the form, at a minimum, of a kind of delicate material positioning or *tuning,* where I use "tuning" in the sense of tuning a radio set or car engine, with the caveat that the character of the "signal" is not known in advance in scientific research.[11] Thus, if we agree that

science, Hacking (1983) was perhaps the first widely recognized attempt to escape from the purely representational idiom. I should note that much recent writing on scientific practice actually bears upon the work of constructing representations in science (technology, computer science, etc.): Star (in press) provides access to the literature. I have no quarrel with this line of enquiry, but my focus is elsewhere.

[10] Semiotics cannot be the whole story about the actor-network understanding of nonhuman agency, and yet it has been a central theme since Latour and Woolgar's ([1979] 1986) emphasis upon scientific instruments as "inscription devices" and seems invariably to be invoked under pressure. Thus, in response to Collins and Yearley's argument, Callon and Latour (1992, p. 349, fig. 12.3) draw a diagram in which Collins and Yearley's dilemma is represented as a horizontal nature/society axis, to which Callon and Latour add a vertical axis on which they comment: "The vertical axis, however, is centred on the very activity of shifting out agencies—which is, by the way, the semiotic definition of an actant devoid of its logo- and anthropocentric connotations" (1992, p. 350). Likewise, in response to Schaffer's (1991) argument that Latour (1988*a*) depends on an illegitimate hylozoism—i.e., the imputation of agency to the nonhuman realm—Latour (1992) offers a reading of a single memoir by Pasteur. It seems clear that Latour's route to nonhuman agency in this instance is by means of texts in the most literal sense. (I have only an incomplete draft of Latour [1992], but I attended his seminar presentation of the paper, and conversation later seemed to confirm this judgment.) In a similar vein, Callon writes: "Sociology is simply an extension of the science of inscriptions. Now it should broaden its scope to include not only actors but the intermediaries through which they speak. . . . *The social can be read in the inscriptions that mark the intermediaries*" (1991, p. 140). The position that I take below is much closer to that developed earlier by John Law (in, e.g., Law 1987), where, without any detours through semiotics, he invokes natural forces as part of an actor-network account of the Portuguese maritime expansion.

[11] Fleck (1979) discusses the tuning (in my terms) of the Wassermann reaction as a test for syphilis. He notes that "during the initial experiments it produced barely 15–20 percent positive results in cases of confirmed syphilis" (1979, p. 72), but that,

we are interested in achieving a *real-time* understanding of scientific practice—and this is a fundamental stipulation of the actor-network approach that the remainder of this essay shares—the scientist is in no better a position than the sociologist when it comes to material agency. No one knows in advance the shape of future machines, but as sociologists we can track the process of establishing that shape without returning to the humanist position that only human agency is involved in it. Of course, after the fact, scientists often offer highly persuasive technical accounts of why the machinic field of science—the field of machines and their powers—has developed in specific ways. But for the purposes of real-time accounting, the substance of such retrospective accounts is one aspect of what needs to be analyzed; it would make no sense to bow to the scientists and project their retrospection backward in time as part of our explanation. This is my basic thought on how to think about the role of nonhuman agency in scientific practice.

Now for the second difficulty with the actor-network approach's semiotic move, the one that centers on human agency. Semiotics imposes an exact symmetry between the human and material realms. Semiotically, as the actor-network approach insists, there is no difference between human and nonhuman agents: human and nonhuman agency can be continuously transformed into one another. This, I think, is the sticking point for many people as far as the actor-network approach is concerned. Specifically, the sticking point is called *intentionality*. We humans differ from nonhumans precisely in that our actions have intentions behind them, whereas the performances (behaviors) of quarks, microbes, and machine tools do not. I think that this is right. I find that I cannot understand scientific practice without reference to the intentions of scientists, though I do not find it necessary to have insight into the intentions of things.[12] The key remark, for me, is that we humans live in time in a

---

after a period of collective development of the detailed performance of the reaction, the success rate rose to 70%–90%. Collins's (1992, chap. 3) account of the work of building a TEA laser can likewise be read as an ethnographic exemplification of the tuning of a material instrument. For more on the tuning of experimental devices in science, see Pickering (1984*b*, pp. 14, 20, 273–74, 409–10) and the works cited there. Note that it is implicit here that we are considering scientific practice as the work of extending, rather than reproducing, scientific culture—in the sense of building new machines and so on. I argue that material agency is temporally emergent in relation to practice so conceived. Whether material agency per se is temporally emergent is another matter. The relative reliability of certain machines—the fact that some magnets, cars, and TVs perform the same functions day after day, e.g.—indicates that some aspects of material agency evolve, at most, slowly on the time scale of human affairs. I thank Adrian Wilson for prompting me to think about this issue.

[12] I am grateful to Simon Schaffer and Steven Shapin for pressing me on the issue of intentionality. They insist that the concepts of agency and intentionality are bound

particular way. We construct goals that refer to presently nonexistent future states and then seek to bring them about. I can see no reason to suppose that DNA double helices or televisions organize their existence thus—why should they? So, with a thrill of transgression, in what follows I intend to commit the sin of breaking a symmetry: I will sketch out an analysis of human intentionality that has no material counterpart.[13]

Having said that, though, I need to qualify my faithlessness. A considerable degree of symmetry remains in my account of scientific practice. First, I will argue in the context of my empirical example that human agency is, just like material agency, temporally emergent. We can say more about the intentional structure of the former, but in the end it, too, simply emerges in the real time of practice.[14] Furthermore, I will argue

up together and that, therefore, one should not speak of material agency, but I do not think that this is correct. Thus, e.g., Harré and Madden (1975) are happy enough to speak of agency in nature without ever dreaming of imputing intentionality to it. Wise and Smith (1989, p. 419) quote William Whewell, writing in 1841 that "in many cases the work to be done may be performed by various agencies; by men, by horses, by water, by wind, by steam."

[13] Ashmore (1993) is a brave and amusing, but not very persuasive, attempt to attribute intentionality to a material agent, namely a "catflap." The other way to preserve a symmetry between nonhuman and human agency is, of course, to deny intentionality to humans. This, I think, is John Law's (1993) strategy in describing human agents as "network effects" "performed" by organizational narratives or "myths" (note the return from agency to textuality via another route). Actually, I think the human/nonhuman symmetry has to be broken somehow, however fastidious one tries to be about it. In any approach that seeks to maintain the symmetry by emphasizing semiotics and textuality (and this includes reflexivity as well as the actor-network approach) it seems necessary to admit that while texts might be written jointly by humans and nonhumans (the latter doing so as or through "inscription devices") they are only read by the former. It might also be noted that Latour often seems to have a pretty clear notion of human intentionality, too. While the early image in Latour and Woolgar (1986) of an "agonistic" war of all against all—a general intention to dominate in battle—can be carried over symmetrically to nonhuman agents or to networks as well (Callon and Latour 1981), I do not think that the same can be said of Latour's (1987, pp. 108–21) later discussion of "translating interests." This latter seems to me only to be applicable to intentional human agents acting on other intentional human agents. For a thoughtful review of sociological understandings of intentionality, see Lynch (1992*c*); for a survey of recent thinking in the social sciences on human and nonhuman agency, see the contributions to Ashmore (in press).

[14] Collins ([1985] 1992) is more or less alone in traditional sociology of science in emphasizing this point (see also my review, Pickering [1987]). Recognition of emergence in human agency aligns my position with symbolic-interactionist, ethnomethodological, and pragmatist sociologies more generally: Denzin (1992) surveys the history of symbolic interactionism up to the present, stressing its links with pragmatism and ethnomethodology; Lynch (1992*a*) is a good entry point for ethnomethodological studies of science;. for access to pragmatist studies of science and technology, see Star (1991, in press) and Fujimura (1992). Laurel Graham and John Law have long encour-

that the trajectories of emergence of human and material agency are constitutively enmeshed in practice by means of a dialectic of resistance and accommodation. Here I return to the fold: like the actor-network approach, my analysis of scientific practice is posthumanist not simply in its twinning of human with material agency but, more profoundly, in its insistence that material and human agencies are mutually and emergently productive of one another.

Now I can talk about the mangle. In a restricted sense, the dialectic of resistance and accommodation just mentioned is what I mean by the mangle of practice. "Mangle" here is a convenient and suggestive shorthand for the dialectic: for me, it conjures up the image of the unpredictable transformations worked upon whatever gets fed into the old-fashioned device of the same name used to squeeze the water out of the washing. "Mangle" can also be used as a verb: I want to say, for example, that the contours of material agency are mangled in practice, meaning emergently transformed and delineated in the dialectic of resistance and accommodation.[15] In a broader sense, though, I take the mangle to refer not just to this dialectic but to an overall image of practice that encompasses it—to the worldview, if you like, that sees science as just described, as an evolving field of human and material agencies reciprocally engaged in the play of resistance and accommodation. An exposition of the mangle in this broader sense is, then, my way of coming to grips with the themes of temporal emergence and posthumanism and their interrelation in the sociology of science.

To go any further, we need an empirical example to hang on to, and for the remainder of the essay I focus on the development of the bubble chamber as an instrument for experimental research in elementary-particle physics.[16] I begin by telling the history of the bubble chamber,

---

aged me to think about the relation between my studies of science and symbolic interactionism; I regret that I did not follow their suggestions earlier.

[15] If pressed too hard, the mangle metaphor quickly breaks down. A real mangle leaves the list of clothing unchanged—shirts in, shirts out—which is too conservative an image for the constructive aspect of scientific practice. "Mangling" also carries connotations of mutilation and dismemberment—"my teddy bear was terminally mangled in a traffic accident"—which carry one directly away from this constructive aspect. There is little to be done about this; I can think of no more appropriate word, one has simply to try take to the metaphor seriously enough but not too seriously. (Those of an acronymic turn of mind might substitute "the DRA"—the dialectic of resistance and accommodation—and "DRAed" for "the mangle" and "mangled" wherever they appear.) I thank, among others, Mike Lynch, Ted O'Leary, and Allan Megill for warning me of potential difficulties with mangle-talk and encouraging me to explain it more fully.

[16] Several reasons recommend this particular example. The choice to focus on a scientific instrument is indicated by my present concern with material agency, and this is

concentrating on the work of Donald Glaser, the chamber's inventor, exemplifying my key concepts of resistance and accommodation—the mangle, in its restricted sense—and emphasizing the goal-oriented nature of Glaser's practice. I then offer a commentary on this episode organized around my themes of agency and emergence. I suggest that we should see the chamber as a locus of nonhuman agency, and I argue that both its material contours and accounts of its character (scientific knowledge) were emergently produced in the real-time dialectic of resistance and accommodation—they were, as I put it, mangled. Turning to human agency, I analyze the intentional structure of Glaser's practice in terms of modeling and argue that Glaser's plans and goals were likewise emergently mangled. I further note that this mangling extended to the social contours of human agency. Not just the vectors of human agency are transformed in practice, then: the unit of analysis changes too. Finally, I compare my account of human agency with traditional humanist accounts of interests and constraints in a way that highlights the posthumanist intertwining of human and material agencies in the mangle.

BUILDING THE BUBBLE CHAMBER

I turn to the early history of the bubble chamber, an instrument that became the principal tool of experimental elementary-particle physics in the 1960s and 1970s, and I start with some basic technical background. A typical particle-physics experiment has three elements. First, there is a beam of particles—protons, electrons, or whatever. This beam can be derived from natural sources, such as the flux of cosmic rays that rains sporadically on the earth, or it can be artificially produced in a particle accelerator. Second, there is a target—a chunk of matter that the beam impinges on. Beam particles interact with atomic nuclei in the target, scattering—changing energy and direction—and often producing new particles. Third, there is a detector, which registers the passage of the scattered and produced particles in a form suitable for subsequent analysis.

The instruments that we need to think about are cloud chambers and bubble chambers, and since their working principle is similar, a descrip-

a historically significant instrument, central to two Nobel Prizes (see below). As will also become evident, the history of the bubble chamber has an interesting social dimension that many similar histories lack. This social dimension is, however, not so rich or elaborate that it dominates the story, which is as it should be if the structure of the posthumanist displacement of the mangle is to be clearly expressed. Finally, there exists an excellent account of the history of the bubble chamber published by Peter Galison (1985) on which I can draw to establish my central points without myself telling the story in detail.

tion of the former will suffice for now. A cloud chamber is basically a tank full of vapor held under pressure, which doubles as both target and detector. Particle beams impinge on the vapor and interact and scatter there; when the pressure is released, the vapor begins to condense and small droplets of liquid form first as strings marking the trajectories of any charged particles that have recently passed through it. These "tracks" are photographed as permanent records of any particle interactions or "events" that occurred within the chamber. Now to history, where I follow Peter Galison's account (Galison 1985).

In the early 1950s, a problem was widely recognized in the physics of the so-called strange particles. These particles had been discovered in cosmic-ray experiments using cloud chambers, but it was proving very hard to accumulate data on them. Strange-particle events seemed to be very rare. At this point, Donald Glaser, then beginning his career at the University of Michigan, set himself a new goal. He wanted to construct some new kind of detector, like the cloud chambers that he had worked with as a graduate student but containing some denser working substance. His reasoning was simple: event rates are proportional to the mass of the target for a given beam intensity, so if he could work with a denser medium, he would stand more chance of finding the strange-particle events of interest. He began to investigate a range of techniques using liquids and solids that would, he hoped, register particle tracks like those produced in cloud chambers, but these failed, one after the other.[17] None of them produced anything like a particle track. These failures constituted, to introduce a key term, a sequence of *resistances* for Glaser, where by resistance I denote the occurrence of a block on the path to some goal. I should emphasize that I want to use "resistance" in just this sense of a practical obstacle, and I do not mean it to refer to whatever account scientists might offer of the source of such obstacles. More on such accounts later; first I want to describe Glaser's responses to such resistances as *accommodations:* in the face of each resistance he devised some other tentative approach toward his goal of a high-density detector that might, he hoped, circumvent the obstacles that he had already encountered. In the early trials, these accommodations took the simple form of moving from the exploration of one working substance and technique to the next. His practice took the form, then, of a dialectic of resistance and accommodation that shifted him through the space of all of the potential new detector arrangements that he could think of. This dialectic is what, in its restricted sense, I call *the mangle of practice*.

[17] Galison (1985, p. 317) mentions attempts to record polymerization reactions in liquids and to develop track-sensitive Geiger counters as well as "diffusion" cloud chambers (these last improving event rates by being continuously sensitive rather than by having higher density).

Glaser's practice reached a temporary resting place in 1952 when he built the first prototypes of a new detector that worked—the bubble chamber. Its operating principle was like that of the cloud chamber, but instead of being filled with a vapor it was filled with a superheated liquid held under pressure. When the pressure was released, boiling began and small bubbles (instead of droplets) formed along the tracks of particles and could be photographed. The main point to note here is that the liquid filling of the bubble chamber was much denser than the vapor used in cloud chambers, and thus the former held out the promise of the higher event rates that defined Glaser's goal.

Glaser made his work public in early 1953, and I want to concentrate on subsequent developments, since they have an interesting social dimension that earlier ones lack. After Glaser's public announcement of the chamber, several individuals and groups quickly set to work to develop the chamber into a practical instrument, most notably Glaser himself in Michigan, Luis Alvarez at Berkeley, and a group at the University of Chicago. (Glaser and Alvarez were awarded the Nobel Prize in physics for this work, in 1960 and 1968, respectively.) A point that I want to stress for future reference is that quite different goals were constructed around the bubble chamber at the three locations. In this respect, the work of Alvarez's group was the most impressive, establishing a basis for the big-science approach to particle-physics experiment that came increasingly to dominate the field in the 1960s. I will stay largely with Glaser, however, since his work exemplifies clearly and simply the emergent and posthumanist aspects of practice that I want to emphasize. I will make comparisons with Alvarez's work whenever it is useful.[18]

Prior to his invention of the bubble chamber, Glaser had trained and worked as a cosmic-ray physicist. He was used, that is, to doing particle physics using naturally occurring cosmic rays rather than beams artificially produced in accelerators. And, after the development of the prototypes, his goal became that of inserting his new detector into his existing specialty. Here another resistance was apparent. Since cosmic rays arrive at the surface of the earth erratically, there was little chance of detecting interesting cosmic-ray events by expanding a bubble chamber at random: the odds were high that nothing would be happening at the instant chosen. This problem was already familiar to physicists working with cloud chambers, and the established solution was to use a different kind of detector as a "trigger." A small electronic detector would be rigged up to register the passage of cosmic rays, and its output would be used to

[18] Galison (1985) gives an extensive account of Alvarez's bubble-chamber work; Pickering (1990) is an analysis of Alvarez's practice along the lines laid out here but lacking the terminology of the mangle.

initiate the expansion of the chamber. In this way, photographs would be taken only when there was a good chance of finding interesting events.

In the extension of his prototype chambers, therefore, Glaser adopted this triggering strategy—and failed. He found that when he wired his bubble chamber to an electronic trigger it did not produce any tracks. Once more a resistance had appeared on the path to his intended goal, and once more there followed a sequence of attempted accommodations. The material form of the bubble chamber was mangled in this process, as it was attached to a whole series of different triggering arrangements, ending with an attempt to trigger the chamber on the sonic plink that accompanied initial boiling.[19] None of these material transformations worked, and Glaser's next accommodation was more drastic.

Glaser's response to the continuing failure of his attempts to trigger his chamber on cosmic rays was, in fact, twofold. One line of response was to construct a conceptual account of the resistances that he had run into. He reasoned that he had failed because the time required for mechanical expansion of the chamber was greater than the lifetime of tracks within it; triggering, then, had to fail. This accommodation thus took the form of a mangling—an additive one, in this instance—of his *knowledge* about bubble chambers: he had learned something about them in his practice. And this knowledge hung together with his second line of accommodation, which was to revise his goal. He abandoned the attempt to use the bubble chamber in cosmic-ray physics and decided instead to put it to work in the accelerator laboratory. There, bunches of particles arrived at precisely timed intervals, so that one could expand the chamber by the clock and the problem of triggering would not arise.

I will discuss Glaser's work in accelerator physics in a moment, but first I want to emphasize that his departure from cosmic-ray physics served to bring out a social dimension of his practice. Glaser later put it this way: "There was a psychological side to this. I knew that large accelerators were going to be built and they were going to make gobs of strange particles. But I didn't want to join an army of people working at the big machines. . . . I decided that if I were clever enough I could invent something that could extract the information from cosmic rays and you could work in a nice peaceful environment rather than in the factory environment of big machines. . . . I wanted to save cosmic-ray physics" (Galison 1985, pp. 323–24).

The peace versus factory opposition in this quotation points to the tension between two distinct forms of work organization that can be

[19] Galison (1985, p. 324) states that after the failure of conventional triggering, "many attempts then followed," including adding carbon dioxide to the chamber to try to slow down the speed of bubble formation.

discerned in the physics of the early 1950s, "small science" and "big science." Small science was the traditional work style of experimental physics—an individualistic form of practice, needing only a low level of funding obtainable from local sources, requiring little in the way of collaboration, and promising quick returns on personal initiatives. Big science was the new work style and organizational form that had been born in the U.S. weapons laboratories of World War II and that made its presence strongly felt in the early 1950s in accelerator laboratories like E. O. Lawrence's at Berkeley, Luis Alvarez's base. Big science was done by teams of physicists and engineers, hierarchically organized; it was characterized by a high level of funding and the bureaucratic processes associated with that, a high degree of interdependence in obtaining access to accelerator beams and in the conduct of experimental research, and a relative lack of flexibility in its response to individual initiatives (for more nuanced historical discussions of big science, see Galison and Hevly [1992]). As the quotation makes clear, Glaser's attachment was to small science, an attachment that, as we shall see, he maintained even as he moved into accelerator-based physics, the home of big science.

With the triggering problem sidestepped, Glaser still faced one difficulty. His prototypes were very small devices, approximately one inch in linear dimensions. They demonstrated the possibility of detecting particle tracks, but in themselves they could not compete in data-production rates with other kinds of detectors already in use at accelerators. The key variable was again their mass as a target: Glaser's prototypes simply failed to put enough stuff in the path of the beam. The question was, then, how to scale up the bubble chamber. This was partly a question of its linear dimensions but also a question of the working fluid: the denser the fluid, the smaller a chamber of given mass could be. Glaser had initially, for convenience, used ether as his working substance. At Berkeley, Alvarez opted to work with liquid hydrogen since data taken on hydrogen were the most easily interpreted, and the low density of liquid hydrogen implied the construction of a relatively large chamber (eventually 72 inches long). This was the route that led directly into classic big science, the route that Glaser wanted to avoid, which he did by seeking to construct a liquid-xenon-filled chamber. Since xenon was much denser than hydrogen, he reasoned that a considerably smaller chamber than Alvarez's could be constructed that would still produce interesting physics. His goal with the xenon chamber was to find "one last 'unique niche that I could [fill] at Michigan without access to all this high technology and large engineering staffs' " (Glaser quoted in Galison 1985, p. 327).

Work proceeded on the xenon chamber, but when completed the chamber yet again failed to produce any tracks whatsoever. Once more a resistance had interposed itself between Glaser and his goal. This time, though, Glaser quickly found a way around it. At the suggestion of colleagues at the Los Alamos laboratory, Glaser and his collaborators tried adding a "quenching" agent, ethylene, to their chamber, and this accommodation was successful. Tracks appeared, and serious experimentation was underway by 1956. The success of adding ethylene additionally invited a reappraisal of the mechanism of bubble formation by charged particles, and the interpretive model that Glaser had worked with all along was abandoned in favor of the "heat spike" theory formalized by Frederick Seitz in 1958. This last sequence of resistance and accommodation in accelerator physics, then, mangled both the material and conceptual aspects of the culture of particle physics: a new material form of the chamber, the quenched xenon chamber, and new knowledge, a new understanding of the chamber's functioning, emerged together. And further, as I now want to describe, the social dimension of Glaser's practice was mangled in the xenon project too.

Glaser's research before the switch to accelerator physics had been typical small science. From June 1950 to November 1952—the period that saw the invention and early development of the bubble chamber—he worked in collaboration with a single graduate student, David Rahm, and was supported by the University of Michigan with a total of $2,000. At the end of 1952, the university increased its support to $3,000 per year. This is to be contrasted with the funding and manpower of the xenon-chamber project, where "part-time salaries for Glaser, Martin Perl (a new faculty member), a secretary, and four research assistants added to the salaries for a full-time postdoc and a full-time machinist-technician came to about $25,000. Equipment, supplies, and machining ran about the same" (Galison 1985, p. 327). While seeking to propagate the small-science work style, then, Glaser had clearly, in scaling up the chamber and switching to xenon, evolved something of a hybrid, by no means as solitary and independent as the classic form. But Glaser's xenon project should in turn be compared with Alvarez's liquid-hydrogen effort at Berkeley. Although the precise extent of Alvarez's empire has never, to my knowledge, been precisely mapped, Galison's account of the work at Berkeley mentions 11 collaborators—physicists, engineers, and graduate students—and it is probably enough to note that the project was eventually funded by the Atomic Energy Commission at $2.5 million and that Alvarez delegated to Don Gow, "a new role that is not common in physics laboratories, but is well known in military organizations; he became my 'chief of staff.' In this position, he coordinated the efforts of the physicists and engineers; he had full responsibility for the careful spending of our precious 2.5 million dollars, and he undertook to become an expert second to none in all the technical phases of the operation, from low temperature thermodynamics to safety engineering" (Alvarez [1987*b*, p. 259], quoted in part in Galison [1985, p. 334]). In comparison with Alvarez's program, then, which set the standard for bubble-chamber physics in the 1960s, the continued links between Glaser's work on the xenon chamber and small science remain evident.

Before I turn to a general discussion of this passage of practice, two last items of historical information can be included, both of which bear on the social dimensions of particle physics. Galison notes that "in 1960 Glaser moved to Berkeley to join the growing team of hydrogen-bubble-chamber workers. Shortly afterward, in large part because of his disaffection with the large team, he left physics for molecular biology," and, "indeed, by February 1967 Alvarez too had begun to devote almost all his time to other projects, principally his balloon work on cosmic rays" (1985, p. 353). There is a wonderful circularity here, with Alvarez regaining (something like) small science in the field that Glaser had failed to save with the bubble chamber.[20]

AGENCY, EMERGENCE, AND THE BUBBLE CHAMBER

In telling the story of the bubble chamber, I have introduced the key idea of the dialectic of resistance and accommodation that I call the mangle, and I have outlined the mangling of the material, the conceptual, and the social in Glaser's practice. Now I need to connect this story to my earlier remarks on agency and temporal emergence. I talk first about nonhuman or material agency and then about human agency. In both phases of the discussion the posthumanist intertwining of agency is evident, but in a third stage I seek to highlight the posthumanism of the mangle from a different angle in a comparison of my account of human agency with traditional humanist schemas.

Material Agency

The most obvious source of agency in my historical narrative is human: I found it necessary to refer several times to Glaser's plans and goals in order to make sense of the story. But my frequent references to the resistances that Glaser encountered en route (he hoped) to those goals should make it clear that he, as a human agent, was not in control of history, and the best way that I can find to think about such resistance is by symmetrizing the picture. As I suggested in the introduction, to understand what is going on in this example, we need to think of Glaser as struggling in his practice with nonhuman agency, somehow centered,

[20] For more on Alvarez's move into small science, see Alvarez (1987*a*).

in this instance, on the bubble chamber. Just how we should speak of its location is, however, another matter. In general, I can think of two ways to proceed.

On one hand, it seems reasonable to see the bubble chamber itself as an agent. Bubble chambers, when they work, produce tracks and photographs in a way that is not substantively attributable to any human agent. Scientists build and operate chambers, but neither *that* tracks appear in them nor the specific configuration of those tracks is in the hands of the chamber's human companions. On the other hand, one might want to see bubble chambers as not themselves agents but as devices, traps, for capturing (seducing, mobilizing) the agency of elementary particles. One might think of them as intermediaries that induce the particles to write.[21] I do not think that it matters which form of words one chooses. In the present instance, the latter seems possibly more appropriate, whereas if we were talking about technological artifacts like machine tools or gas turbines, the former might be, but all that my analysis requires is the idea that, in using bubble chambers, physicists are indeed dealing with some form of nonhuman agency.[22] Now I want to connect this idea to my theme of temporal emergence.

The key point that I want to emphasize is that the precise material configuration of nonhuman agency (and its precise character—just what it would do) was temporally emergent in the real time of Glaser's practice. It should be clear from the historical narrative that Glaser had no way of knowing in advance that most of his attempts to go beyond the cloud chamber would fail but that his prototype bubble chambers would succeed, or that most of his attempts to turn the bubble chamber into a practical experimental device would fail but that the quenched xenon chamber would succeed. In fact, *nothing* identifiably present when he embarked on these passages of practice determined the future evolution of the material configuration of the chamber: Glaser had to find out, in the real time of practice, what the contours of material agency might be.

[21] This is the actor-network idea of a scientific instrument as an inscription device that leads immediately into a semiotic analysis. Let me just emphasize that I am interested here in the process of getting the bubble chamber (or particles) to write, not in what chambers have written over the last 40 years.

[22] Phrases like "induce the particles to write" smack of correspondence realism about scientific knowledge, as if the real existence of elementary particles were being taken for granted, so I should make it clear that such correspondence is no part of my argument. That such particles are responsible for the tracks in cloud and bubble chambers is *the scientists'* way of accounting for events (and is formally on a par with Glaser's accounts of the functioning of his bubble chambers, discussed below). In principle, such accounting should itself be subjected to real-time accounting—but not here. Callon (1991) indicates some appropriate moves in thinking about the precise location of material agency.

This process of finding out is what I have conceptualized in terms of the dialectic of resistance and accommodation, and one can rephrase what has just been said by pointing to the brute emergence of resistance. There is no real-time explanation for the particular pattern of resistances that Glaser encountered in his attempts to go beyond the cloud chamber: in his practice, these resistances appeared as if by *chance*—they *just happened*. It just happened that when Glaser configured his instrument this way (or this, or this) it did not produce tracks, but when he configured it that way, it did. This is the strong sense of temporal emergence implicit in the mangle.[23]

Here it might be useful to return briefly to Collins and Yearley's dilemma concerning material agency. Their argument was that one has either to think about accounts of material agency as the products of human actors or about material agency itself along the lines of the scientists' accounts of it (thus ceding analysis of science to the scientists themselves). As I indicated earlier, the present analysis of material agency as temporally emergent evades both horns of this dilemma. It both recognizes material agency as that with which scientists struggle—that is, as prior to any scientific accounting—and denies that we have to fall in with such scientific accounting. Having said that, of course, attention needs to be paid to the fact that Glaser did produce accounts of the functioning of the bubble chamber—knowledge—as he went along. He explained the failure of triggering in terms of the response time of the bubble chamber, and the initial failure of the xenon chamber in terms of a revised understanding of the mechanism of bubble formation.[24] But, as I noted in the introduction, such accounts pose no problem for real-time analysis of practice—they should themselves be seen as part and parcel of the mangling process, as products of the dialectic of resistance and accommodation, at once retrospective glosses on emergent resistances and prospective elements of strategies of accommodation. Their substance can no more be understood in advance of practice than the material contours of nonhuman agency. Both material agency and articulated scientific accounts thereof are temporally emergent in the mangle.

[23] One can make a connection to early pragmatist philosophy here. Discussing the work of Charles Sanders Peirce, Cohen (1923, p. xix n. 11) explains that "Peirce's tychism is indebted to [Chauncey] Wright's doctrine of accidents and 'cosmic weather,' a doctrine which maintained against LaPlace that a mind knowing nature from moment to moment is bound to encounter genuine novelty in phenomena, which no amount of knowledge would enable us to foresee." The same doctrine is expressed in William James's (1907, 1909 [1978], p. 106) well-known sentiment that "experience, as we know, has ways of *boiling over,* and making us correct our present formulas."

[24] As an antidote to correspondence realism, it is worth mentioning that the heat-spike model of bubble formation showed that Glaser's prior understanding, which he had relied on all along, was wrong (Galison 1985, p. 328).

One last point concerning material agency: I need to emphasize that the present discussion does not imply a technological determinist vision of science. The important remark in this connection is that the trajectory of emergence of material agency does not have its own pure and autonomous dynamics. Material agency does not, as it were, force itself upon scientists; there is, to put it another way, no such thing as a perfect tuning of machines dictated by material agency as a thing-in-itself; or, to put it yet another way, scientists never grasp the pure essence of material agency. Instead, material agency emerges by means of an inherently *impure* dynamics. The resistances that are central to the mangle are always situated within a space of human purposes, goals, and plans; the resistances that Glaser encountered in his practice only counted as such because he had some particular ends in view. Resistances, in this sense, are *liminal:* they exist on the boundaries, at the point of intersection, of the realms of human and nonhuman agency. They are irrevocably impure human/material hybrids, and this quality immediately entangles the emergence of material agency with human agency (without, in any sense, reducing the former to the latter). This entanglement is, so to speak, the far side of the posthumanism of the mangle: material agency is sucked into the human realm through the dialectic of resistance and accommodation. Now I turn to the converse proposition—that human agency is itself emergently reconfigured in its engagement with material agency.

Human Agency

Donald Glaser was certainly as much an agent in the development of the bubble chamber as the chamber itself: like the chamber, Glaser did things in the world that were constitutive of the historical pattern of events. Beyond this, though, as I indicated in the introduction, it seems that one can say more about Glaser's agency than the chamber's. In particular, human agency in this instance has an interesting temporal structure that material agency lacks. It seems unnecessary, at best, to think that a bubble chamber has any future end or purpose in view when it produces tracks upon expansion. In contrast, one cannot understand Glaser's practice without recognizing its orientation to future goals. Glaser did not assemble bits and pieces of apparatus in the laboratory just for its own sake; he had an end in view—the end of constructing and deploying some novel particle detector. Much of what Glaser did has to be understood as tentative steps toward that end. To get to grips with what is special about human agency, then—to break the perfect human/nonhuman symmetry of actor-network semiotics—one needs to think about the intentions, goals, purposes, or whatever of human action.

A first relevant observation is that goals have to be seen as temporally enduring relative to the details of the passages of practice that are oriented to them. A goal is a relatively fixed image of some future state of affairs at which temporally extended passages of practice aim. And from this observation, it would be a small step to the idea that the intentional structure of human agency has a temporally nonemergent quality. The body of literature in the sociology of scientific knowledge discussed in the following section can easily be read in this way. In general, though, I think that this line of thought is mistaken and that we should see the intentional structure of human agency as itself temporally emergent, albeit on a longer time scale than the details of practice.

To see what is at issue, we can begin by thinking about the formulation of goals. It is clear, I believe, that scientists do not formulate goals at random: the future states of affairs at which practice aims are constructed from present states in a process of *modeling*.[25] Glaser, for example, initially sought to manufacture new detectors modeled upon the cloud chamber—like the cloud chamber in certain respects, but transformed in others. Later, he (and others) sought to construct useful bubble chambers modeled on Glaser's own prototypes. Models are, I think, constitutive of scientific practice, in the sense that it is impossible to imagine Glaser embarking on the path that led to the bubble chamber without the example of the cloud chamber before him. This centrality of modeling to goal formation situates human agency with respect to the cultural field in which it operates—the field of existing detectors, in the present example. And such cultural situatedness immediately implies a degree of both temporal emergence and posthumanist intertwining in the intentional structure of human agency. Concerning the former, I argued earlier that, for example, the precise material configuration and properties of Glaser's prototype chambers were temporally emergent, and the present discussion connects that emergent form to the goals of Glaser's subsequent practice. The goals of scientific practice must be at least as emergent as the models on which they are based. The posthumanist aspect of those goals follows equally directly. Though Glaser formulated the goals of his practice as a classically human agent, the field of existing detectors in

[25] I take the idea of modeling from traditional discussions in the history and philosophy of science of the role of metaphor and analogy in theory development. I prefer to speak of modeling since I want to apply the idea to the material culture of science, while metaphor and analogy are usually taken as having textual referents. Modeling, in Kuhnian terms, is developing an exemplar. For access to the relevant literature, see, e.g., Barnes (1982), Bloor (1991), Gooding (1990), Hesse (1966), Knorr-Cetina (1981), Kuhn (1970), and Pickering (1981, 1984*b*). There is now also a growing cognitive-science literature on the role of "mental models" in science and elsewhere (see Gentner and Stevens 1983; Rouse and Morris 1986; and Gorman 1992).

which he formulated those goals was a field of material agency. There is, then, a temporal and posthumanist interplay here between the emergence of material agency and the construction of human goals.

More needs to be said on the nature of modeling in scientific practice, but before that I want to bring the social nature of human agency into the discussion. So far I have talked about modeling as it was constitutive of the technical goals of Glaser's practice, but the analysis is also applicable to the social means that Glaser envisaged en route to those goals. Thus I think that it makes sense to see the small-science pattern of work organization in cosmic-ray physics as functioning as a model in Glaser's practice: he imagined the work of constructing and using new detectors as proceeding along small-science lines. The significance of these remarks should become clearer shortly; for the moment, let me just note that the conception of modeling makes it possible to think about the emergence of both material agency and the social contours of human agency along the same lines.

My next observation is that modeling is an *open-ended* process with no determinate destination: a given model does not prescribe the form of its own extension.[26] Glaser tentatively imagined and sought to construct a whole range of different kinds of detectors—all modeled, in one way or another, on the basic form of the cloud chamber—before eventually succeeding with the bubble chamber. Likewise Glaser, Alvarez, and the Chicago group sought to develop Glaser's prototypes along quite different axes, and Glaser himself, as we have seen, developed the model in many different ways—first the several versions of the triggered chamber for cosmic-ray physics and then the variants of the xenon chamber. Modeling, then, is the link between existing culture and the future states that are the goals of scientific practice, but the link is not a causal or mechanical one: the choice of any particular model opens up an indefinite space of different goals. And the question therefore arises of why particular scientists fix on particular goals within this space. Here I have no principled suggestions. One can speak of scientific creativity, or one can say that the formulation of goals just happens: it just happened, for example, that Glaser set himself the goal of going beyond the cloud chamber, that along the way he hit on the idea that led him to the bubble chamber, and so on. Certainly nothing identifiably present in advance determined the intentional structure of his practice. Again, then, we run into a role for chance and brute temporal emergence in scientific practice, and we need, therefore, to think about the intentional structure of scientific prac-

[26] Barnes (1982) gives a very clear exposition of this point. On my analysis, the openness of modeling is a necessary counterpoint in the realm of human agency to the emergence of material agency.

tice as being emergent in at least two senses: practical goals are constructed in a temporally emergent cultural field, and their detailed substance is itself emergently constructed in that field.[27] But more remains to be said. An orientation to future goals serves to distinguish human from material agency, but does not exhaust the former. We need to think about what goal-oriented human practice looks like in its temporal extension.

Most of Glaser's practice did not involve the formulation of goals; it consisted rather in material attempts to achieve such goals. He knew the kind of detectors that he wanted to construct and spent most of his time trying to make them. The latter consideration, of course, returns us to the mangle, to the dialectic of resistance and accommodation in the engagement with material agency. And the point that I now need to stress is that accommodation amounts, to a greater or lesser extent, to revision of plans and goals, to a revision of the intentional structure of human agency. When Glaser gave up the attempt to trigger his chamber, for example, at the same time he gave up the idea of inserting his new detector into cosmic-ray physics and relocated his goal to the accelerator laboratory. Goals, then, while relatively enduring through time, have themselves to be seen as subject to mangling in practice.[28] This observation, in turn, brings us to the third and last sense in which I want to describe intentionality as temporally emergent: the transformation of goals in practice has to be understood in terms of contingently formulated accommodations to temporally emergent resistance. And it points, yet

[27] No doubt more can be said about goal construction in science. Elsewhere (Pickering 1981, 1984*b*), I have argued that expertise is a key variable to consider in the dynamics of scientific practice: one can think of expertise as among the resources that scientists can deploy in pursuit of ends and, hence, as structuring the particular ends that given scientists choose to pursue. But two points about expertise need to be recognized: it is itself open-ended, being deployable in an indefinite range of future projects, and it is itself emergent—expertise comes with practice (and in a posthumanist fashion if that practice engages with material agency). The appeal to expertise does not, therefore, yield a determinate account of goal formation. In Pickering (1990) I approached the problem from another angle, trying to analyze the goals that Alvarez formulated around the bubble chamber in terms of the intersection of modeling vectors and the piling up of cultural resources. Again, I think that this illuminates the processes of goal formation and elaboration, but it does not efface the elements of contingency and temporal emergence present in it.

[28] If one thinks through strategies of accommodation in detail, some ambiguity between means and ends becomes evident in scientific practice. Thus it seems reasonable to see Glaser's move from cosmic-ray to accelerator-based physics as a shift in goal, while his moves through the space of possible triggering arrangements seem better described as the exploration of various possible means to an unvarying end. This ambiguity does not, however, undermine the present argument: both means and ends are bound up in human intentionality. Suchman's (1987) work on "plans and situated actions" is relevant and informative here.

again, to the posthumanism of the mangle. As I remarked when discussing material agency, resistance emerges at the intersection of human and material agency and, as the present argument suggests, serves to transform the former in one and the same process as it delineates the latter. Just as the mangle, then, pulls material agency onto the terrain of human agency, so it materially structures human agency. Just as the evolution of material agency lacks its own pure dynamics, so too does the evolution of human agency.

One last thread remains to be picked up in this stage of the discussion. In the previous section I emphasized the temporal emergence of the material configuration of nonhuman agency, and now I want to symmetrize my analysis by making a similar point about the contours of human agency. I have so far been talking about the intentional structure of Glaser's agency, but now it is time to note that the identity of "Donald Glaser," as he figured in my narrative as the bearer and executor of intentions, varied with time. At the beginning of the narrative, "Glaser" denoted an almost classic microactor—a single human individual (though, even in his early work at Michigan, Glaser was assisted by a graduate student)—whereas in the xenon-chamber project, as we have seen, "Glaser" had become something of a macroactor, denoting a team of no less than nine people. Here, again, I want to emphasize the temporal emergence of this transformation of the social contours of human agency. No one could have foreseen in advance that this transformation would come about; no identifiable feature of Glaser's initial situation determined it. Glaser did not intend it at all. Instead, Glaser's small-science model, as I expressed it above, was itself open-endedly mangled in practice.[29] And again I want to emphasize the posthumanist decentering of this mangling. The social evolution of Glaser's work style was itself constitutively the product of maneuvers in the field of material agency. Most strikingly, perhaps, it seems clear that Glaser's practice would have remained much more individualistic if he had succeeded in triggering his chamber on cosmic rays; likewise, the nine-person team

[29] Another way to put this point is to note that while Glaser's interest in small science was clear enough, he was not limited in his practice by any closed definition of it. In effect, he had to find out what would count as small science in the course of his project—or, equivalently, to find out what he was willing to tolerate as close enough to his basic conception of small science. Elsewhere (Pickering 1990) I make a similar point concerning Alvarez's finding out just what big science could amount to in bubble-chamber physics. As noted at the end of the historical narrative, both physicists eventually decided that the social organization of bubble-chamber work had become intolerable and left the field, but that this was an emergent upshot of practice is especially clear in the case of Alvarez, who had deliberately set out to construct the big-science form of life that eventually repulsed him (Alvarez 1987*a*).

would have collapsed if the xenon-chamber project had failed. Here, as before, then, one needs to think of the impure dialectic of resistance and accommodation between human and material agencies to comprehend the mangling of the social contours and embodiment of the former.[30] The social aspects of Glaser's practice did not evolve in accordance with any pure social dynamics; no purely sociological explanation can suffice to explain Glaser's transformation toward the status of a macroactor.

Interests, Constraints, and the Mangle

To complete my presentation of the mangle, it might be useful to thematize its emergent posthumanism from a different angle, so I close with a brief comparison of my account of human agency with traditional humanist accounts. The latter fall into two classes. One class—encompassing, for example, pragmatist and symbolic-interactionist approaches—explicitly recognizes the emergence of human agency and differs from my account only in its human-centeredness.[31] I do not need to discuss it further here. The other class, though, is often cast in nonemergent terms, and this is the class that I need to focus upon. It can itself be subdivided into two rather different understandings of human agency. On one hand, following Marx and Weber, human agency can be characterized positively in terms of, say, the *interests* of individuals and groups. On the other, following Durkheim, it can be characterized negatively, in terms of *constraints* on human action. Both of these lines of thought have been articulated within the sociology of scientific knowledge: the latter by David Bloor (1983), for example, the former in the writings of Barry Barnes (1977, 1982), Steven Shapin (1979, 1982, 1988), and Donald Mackenzie (1981*b*). And it is clear that my account of human agency has resonances with both. Most obviously, my insistence that scientific practice has to be understood as goal-oriented aligns my analysis with the interest model—at least if one is willing to concede that interests are open-ended and subject to emergent redefinition in practice. The question arises, however, of just how interests are transformed in practice. In humanist analyses of science, at least, that question has never been clearly answered, effectively enforcing a nonemergent understanding by default.[32] The answer that I have offered is couched in terms of

[30] The emergent "coproduction" of social structure and material agency is a central theme in the actor-network approach: for some exemplifications, see Callon (1987) and Latour (1983, 1987).

[31] On pragmatist and symbolic-interactionist understandings of human agency, in science and more generally, see the works cited above in n. 14.

[32] To make their point about "social construction," most studies in the sociology of scientific knowledge focus on instances where interests arguably remain constant

the dialectic of resistance and accommodation—the mangle—and here my appeal to resistance as an explanatory category clearly puts me somewhere near the terrain of the second accounting scheme, the one that understands human agency in terms of constraint. A clarification of the difference between my conception of resistance and traditional notions of constraint can, though, serve to bring to the foreground what is novel about the mangle.[33]

The point is this. In the humanist schema, constraint has two characteristic aspects. First, it is located *within* the distinctively human realm. It consists, say, in a set of social (or epistemic) norms, derived in some sense from social structure. And second, constraint is *nonemergent,* at least on the time scale of human practice. Constraints are continuously present in culture, even when not actively operative. The language of constraint is the language of the prison: constraints are always there, just like the walls of the prison, even though we only bump into them occasionally (and can learn not to bump into them at all).[34] My usage of

---

through practice. In critical theory, Smith (1988, p. 32) spells out an emergent and posthumanist understanding of the intentional structure of human action: "What we speak of as a subject's 'needs,' 'interests,' and 'purposes' are not only always changing, but they are also not altogether independent of or prior to the entities that satisfy or implement them; that is, entities also produce the needs and interests they satisfy and evoke the purposes they implement. Moreover, because our purposes are continuously transformed and redirected by the objects we produce in the very process of implementing them, and because of the very complex interrelations among human needs, technological production, and cultural practices, there is a continuous process of mutual modification between our desires and our universe." Giddens (1979, p. 181) expresses a similar thought in social theory: "To be aware of one's interests, therefore, is more than to be aware of a want or wants: it is to know how one can set about trying to realise them. . . . Interests presume wants, but the concept of interest concerns not the wants as such, but the possible modes of their realisation in given sets of circumstances. . . . Interests imply potential courses of action, in contingent social and material circumstances." In a similar vein, see the quote from Deleuze and Guattari (1987) at the beginning of this essay. For a fascinating discussion of "interest" and related concepts in early German social theory, see Turner (1991).

[33] I thank Michael Lynch and others who, in professing to see no difference between resistance and constraint, forced me to think this issue through.

[34] Thus Giddens (1984, p. 174) conceptualizes the whole Durkheimian tradition like this: "The structural properties of social systems . . . are like the walls of a room from which an individual cannot escape but inside which he or she is able to move around at whim." In contrast to this picture, Giddens's structuration theory "is based on the proposition that structure is always both enabling and constraining" (1984, p. 169). Hence, the previous quotation continues, "Structuration replaces this view [of complete freedom within a room] with one which holds that structure is implicated in that very 'freedom of action' which is treated as a residual and unexplicated category in the various forms of 'structural sociology.' " The residual, nonemergent and unexplicated walls are still there, though. (Giddens later abandons metaphor for something close to tautology: "What, then, of structural constraint? . . . It is best described as *placing limits upon the range of options open to an actor*" [1984, pp.

resistance has neither of these qualities. As I have emphasized, in the real-time analysis of practice, one has to see resistance as genuinely emergent in time, as a block arising in practice to this or that passage of goal-oriented practice. Thus, though resistance and constraint have an evident conceptual affinity, they are, as it were, perpendicular to one another in time: constraint is *synchronic,* antedating practice and enduring through it, while resistance is *diachronic,* constitutively indexed by time.[35] Furthermore, while constraint resides in a distinctively human

176–77].) For a poetic variation on the theme, see Ginzburg (1980, p. xxi): "In the eyes of his fellows, Menocchio was a man somewhat different from others. But this distinctiveness had very definite limits. As with language, culture offers to the individual a horizon of latent possibilities—a flexible and invisible cage in which he can exercise his own conditional liberty"—an invisible rubber prison with very definite limits. As Shapin (1988, n. 14) notes, even Latour is not immune from this style of nonemergent thinking, as, for instance, in his idea that "interests are elastic, but like rubber, there is a point where they break or spring back" (Latour 1987, pp. 112–13). In science studies, Galison (1987, 1988, 1994) offers an analysis of scientific practice based on a posthumanist but still nonemergent notion of constraint; for a critique, see Pickering (1994). I had thought that I was more or less alone in my critical sensitivity to nonemergent prison metaphors in social theory until I came across the following passage (Edwards, Ashmore, and Potter 1992, p. 13): "The problem with the idea of objective limits [or constraints] on textual readings, or on descriptions of physical events, is that it is impossible to say in advance of discussion *what exactly they are,* outside of the circularity of taking the author's word for it, or appealing as Eco does to what other readers will find 'preposterous.' But that is unfortunate too, since what people in general find preposterous is patently a matter of social judgement and consensus, and no more a guarantee of truth or reality than it is when later judgements declare everyone to have got it all wrong."

[35] A conversation with Michael Power helped me to see the mangle as a temporally rotated version of traditional accounts of human agency. I hope pointing out that resistance performs in my analysis a role similar to that of constraint in traditional accounts makes it clear that my emphasis on emergence does not amount to the idiotic version of "anything goes" often mistakenly imputed to Paul Feyerabend. Comments from, among others, Irving Elichirigoity, Peter Galison, and Paul Forman have encouraged me to make this explicit. I should also make it explicit that I have no objection to the notion of "constraint" as an actors' category. Certainly, actors often do construe their situations and develop their practice in terms of articulated notions of constraint. I suggest, however, that such accounts need to be analyzed as constructed in practice and themselves subject to mangling—just like any other item of knowledge. They should not be treated, as is often done, as somehow structuring and thus explaining the flow of practice from without. Constraints are as emergent as anything else. Thus, to give one example, in his early work, Glaser found that the interior of a bubble chamber had to be extremely clean if tracks were to be formed. This became an element of bubble-chamber lore and can readily be understood as a constraint on chamber development. Interestingly, though, this lore came under pressure, especially at Berkeley, where it was evident that the big chambers that Alvarez had in mind would necessarily be "dirty" ones (in a technical sense—having metal-to-glass joints). At this point A. J. Schwemin, one of the Berkeley technicians, just ignored the constraint and went ahead, building a relatively large dirty chamber,

realm, resistance, as I have stressed, exists only in the crosscutting of the realms of human and material agency. Resistance (and accommodation) is at the heart of the struggle between the human and material realms in which each is interactively restructured with respect to the other—in which, as in our example, material agency, scientific knowledge, and human agency and its social contours are all reconfigured at once. Coupled with the rotation in time just mentioned, this displacement—from constraint as a characteristic of human agency to resistances on the boundary of human and material agency—serves to define the emergent posthumanist decentering implicit in the mangle.[36]

REFERENCES

Alvarez, Luis. 1987*a*. *Alvarez: Adventures of a Physicist*. New York: Basic Books.

———. 1987*b*. "Recent Developments in Particle Physics." Pp. 110–53 in *Discovering Alvarez: Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues,* edited by W. Peter Trower. Chicago: University of Chicago Press.

Ashmore, Malcolm. 1993. "Behaviour Modification of a Catflap: A Contribution to the Sociology of Things." In *Kennis en Methode*. In press.

———, ed. In press. *Humans and Other Agents: Studies of Agency and Its Attribution in Contemporary Social Science*. Special Issue of *American Behavioral Scientist*.

Baird, Davis. 1993. "Analytical Chemistry and the 'Big' Scientific Instrumentation Revolution." *Annals of Science* 50:267–90.

Baird, Davis, and Alfred Nordmann. In press. "Facts-Well-Put." *British Journal for the Philosophy of Science*.

Barnes, Barry. 1977. *Interests and the Growth of Knowledge*. London: Routledge & Kegan Paul.

———. 1981. "On the 'Hows' and 'Whys' of Cultural Change (Response to Woolgar)." *Social Studies of Science* 11:481–98.

———. 1982. *T. S. Kuhn and Social Science*. London: Macmillan.

which proved to work quite satisfactorily. This constraint was discontinuously mangled—it disappeared—in material practice (Alvarez 1987*b*, pp. 118–19).

[36] Note that one can arrive at notions of emergence within traditional humanist accounts of human agency through the introduction of feedback. Interests and constraints might be seen as subject to conscious or unconscious revision in response to practical experience. This line of thought has not been well developed in science studies, but within sociology as a discipline it is familiar in, e.g., the guise of Anthony Giddens's (1984) structuration theory, where the idea is that social structure both constrains (and enables) action and is itself subject to reflexive monitoring and modification in the practice of individual agents. Law (1993) and Law and Bijker (1992) borrow from structuration theory in the sociology of science and technology; the latter construes the entire range of studies collected in Bijker and Law (1992) as emergent. For a valuable history of feedback thought in the social sciences, see Richardson (1991). I thank Bruce Lambert for encouraging me to think about structuration theory in relation to studies of scientific practice. Of course, no amount of feedback can transform the asymmetric attribution of agency that is definitive of traditional humanist approaches.

Bhaskar, Roy. 1975. *A Realist Theory of Science*. Leeds: Leeds Books.
Bijker, Wiebe, and John Law, eds. 1992. *Shaping Technology/Building Society: Studies in Sociotechnical Change*. Cambridge, Mass.: MIT Press.
Bloor, David. 1983. *Wittgenstein: A Social Theory of Knowledge*. London: Macmillan.
———. 1991. *Knowledge and Social Imagery,* 2d ed. Chicago: University of Chicago Press.
———. 1992. "Left and Right Wittgensteinians." Pp. 266–82 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.
Callon, Michel. 1987. "Society in the Making: The Study of Technology as a Tool for Sociological Analysis." Pp. 83–103 in *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology,* edited by W. Bijker, T. Hughes, and T. Pinch. Cambridge, Mass.: MIT Press.
———. 1991. "Techno-Economic Networks and Irreversibility." Pp. 132–61 in *A Sociology of Monsters? Essays on Power, Technology and Domination: Sociological Review Monograph,* vol. 38. Edited by John Law. London: Routledge.
Callon, Michel, and Bruno Latour. 1981. "Unscrewing the Big Leviathan, or How Do Actors Macrostructure Reality?" Pp. 277–303 in *Advances in Social Theory and Methodology: Toward an Integration of Micro- and Macro-Sociologies,* edited by Karin Knorr-Cetina and Aaron Cicourel. Boston: Routledge & Kegan Paul.
———. 1992. "Don't Throw the Baby Out with the Bath School! A Reply to Collins and Yearley." Pp. 343–68 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.
Cartwright, Nancy. 1989. *Nature's Capacities and Their Measurement*. Oxford: Clarendon Press.
Chalmers, Alan. 1992. "Is a Law Reasonable to a Hume?" *Cogito* (Winter), 125–29.
Cohen, Morris. 1923. Introduction to *Chance, Love and Logic: Philosophical Essays,* by Charles S. Peirce. Edited by Morris R. Cohen. New York: Harcourt, Brace.
Collins, Harry M. 1992. *Changing Order: Replication and Induction in Scientific Practice,* 2d ed. Chicago: University of Chicago Press.
Collins, Harry M., and Steven Yearley. 1992*a*. "Epistemological Chicken." Pp. 301–26 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.
———. 1992*b*. "Journey into Space." Pp. 369–89 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.
Deleuze, Gilles, and Felix Guattari. 1987. *A Thousand Plateaus: Capitalism and Schizophrenia*. Minneapolis: University of Minnesota Press.
Deleuze, Gilles, and Christine Parnet. 1987. *Dialogues*. New York: Columbia University Press.
Denzin, Norman. 1992. *Symbolic Interactionism and Cultural Studies: The Politics of Interpretation*. Oxford: Blackwell.
Edwards, Derek, Malcolm Ashmore, and Jonathan Potter. 1992. "Death and Furniture: The Rhetoric, Politics and Theology of Bottom Line Arguments against Relativism." Paper presented at the fifteenth Discourse and Reflexivity Workshop, Sheffield, September.
Fleck, Ludwik. (1935) 1979. *Genesis and Development of a Scientific Fact*. Chicago: University of Chicago Press.
Foucault, Michel. 1972. *The Archaeology of Knowledge*. New York: Pantheon.
———. 1979. *Discipline and Punish: The Birth of the Prison*. New York: Vintage Books.
Fujimura, Joan. 1992. "Crafting Science: Standardized Packages, Boundary Objects, and 'Translation.' " Pp. 168–211 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

Galison, Peter. 1985. "Bubble Chambers and the Experimental Workplace." Pp. 309–73 in *Observation, Experiment, and Hypothesis in Modern Physical Science,* edited by Peter Achinstein and Owen Hannaway. Cambridge, Mass.: MIT Press.

———. 1987. *How Experiments End.* Chicago: University of Chicago Press.

———. 1988. "Multiple Constraints, Simultaneous Solutions." Pp. 157–63 in *PSA 1990: Proceedings for the 1990 Biennial Meeting of the Philosophy of Science Association,* vol. 2. Edited by Arthur Fine and Mickey Forbes. East Lansing, Mich.: Philosophy of Science Association.

———. 1994. "Context and Constraint." In *Theories of Practice/Stories of Practice,* edited by Jed Buchwald. Chicago: University of Chicago Press.

Galison, Peter, and Bruce Hevly, eds. 1992. *Big Science: The Growth of Large-Scale Research.* Stanford, Calif.: Stanford University Press.

Gentner, D., and A. L. Stevens. 1983. *Mental Models.* Hillsdale, N.J.: Erlbaum.

Giddens, Anthony. 1979. *Central Problems in Social Theory.* London: Macmillan.

———. 1984. *The Constitution of Society: Outline of the Theory of Structuration.* Berkeley: University of California Press.

Ginzburg, Carlo. 1980. *The Cheese and the Worms: The Cosmos of a Sixteenth-Century Miller.* New York and London: Penguin.

Gooding, David. 1990. *Experiment and the Making of Meaning.* Dordrecht, Boston, and London: Kluwer Academic.

Gorman, Michael. 1992. *Simulating Science: Heuristics and Mental Models in Technoscientific Thinking.* Bloomington: Indiana University Press.

Hacking, Ian. 1983. *Representing and Intervening.* Cambridge: Cambridge University Press.

———. 1992. "The Self-Vindication of the Laboratory Sciences." Pp. 29–64 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

Haraway, Donna. 1991. *Simians, Cyborgs, and Women: The Reinvention of Nature.* London: Free Association Books.

Harré, Rom, and E. H. Madden. 1975. *Causal Powers: A Theory of Natural Necessity.* Oxford: Blackwell.

Hesse, Mary. 1966. *Models and Analogies in Science.* Notre Dame, Ind.: University of Notre Dame Press.

Hopwood, Anthony. 1987. "The Archaeology of Accounting Systems." *Accounting, Organizations and Society* 12:207–34.

James, William. (1907, 1909) 1978. *Pragmatism and the Meaning of Truth.* Cambridge, Mass.: Harvard University Press.

Knorr-Cetina, Karin. 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science.* Oxford and New York: Pergamon.

Kuhn, Thomas. 1970. *The Structure of Scientific Revolutions,* 2d ed. Chicago: University of Chicago Press.

Latour, Bruno. 1983. "Give Me a Laboratory and I Will Raise the World." Pp. 141–70 in *Science Observed: Perspectives on the Social Study of Science,* edited by Karin Knorr-Cetina and Michael Mulkay. Beverly Hills, Calif.: Sage.

———. 1987. *Science in Action: How to Follow Scientists and Engineers through Society.* Cambridge, Mass.: Harvard University Press.

———. 1988*a*. *The Pasteurization of France.* Cambridge, Mass.: Harvard University Press.

———. 1988*b*. "The Politics of Explanation: An Alternative." Pp. 155–76 in *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge,* edited by Steve Woolgar. Beverly Hills, Calif., and London: Sage.

———. 1992. "A 'Matter' of Life and Death—or Should We Avoid Hylozoism?" Paper presented at the Department of History and Philosophy of Science, University of Cambridge, October 29.

Latour, Bruno, and Steve Woolgar. 1986. *Laboratory Life: The Construction of Scientific Facts,* 2d ed. Princeton, N.J.: Princeton University Press.

Law, John. 1987. "Technology and Heterogeneous Engineering: The Case of Portuguese Expansion." Pp. 111–34 in *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology,* edited by W. Bijker, T. Hughes, and T. Pinch. Cambridge, Mass.: MIT Press.

———, ed. 1991. *A Sociology of Monsters? Essays on Power, Technology and Domination.* Sociological Review Monograph, vol. 38. London: Routledge.

———. 1993. *Modernity, Myth and Materialism.* Oxford: Blackwell.

Law, John, and Wiebe Bijker. 1992. "Postscript: Technology, Stability, and Social Theory." Pp. 290–308 in *Shaping Technology/Building Society: Studies in Sociotechnical Change,* edited by Wiebe Bijker and John Law. Cambridge, Mass.: MIT Press.

Lynch, Michael. 1985. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory.* London: Routledge & Kegan Paul.

———. 1992*a*. "Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science." Pp. 215–65 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

———. 1992*b*. "From the 'Will to Theory' to the Discursive Collage: Reply to Bloor." Pp. 283–306 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

———. 1992*c*. "Springs of Action or Vocabularies of Motive." Paper presented at workshop on Vocation, Work and Culture in Early Modern England, sponsored by the Achievement Project: Intellectual and Material Culture in Modern Europe, Oxford, December 10–12.

MacKenzie, Donald. 1981*a*. "Interests, Positivism and History." *Social Studies of Science* 11:498–504.

———. 1981*b*. *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge.* Edinburgh: Edinburgh University Press.

Mellor, D. Hugh. 1974. "In Defence of Dispositions." *Philosophical Review* 83:157–81.

Miller, Peter. 1992. "Accounting and Objectivity: The Invention of Calculating Selves and Calculable Spaces." *Annals of Scholarship* 9:61–86.

Miller, Peter, and Ted O'Leary. In press. "Accounting, 'Economic Citizenship' and the Spatial Reordering of Manufacture." In *Accounting, Organizations and Society.*

Pickering, Andrew. 1981. "The Role of Interests in High-Energy Physics: The Choice between Charm and Colour." Pp. 107–38 in *The Social Process of Scientific Investigation. Sociology of the Sciences,* vol. 4. Edited by Karin D. Knorr, Roger Krohn, and Richard D. Whitley. Dordrecht: Reidel.

———. 1984*a*. "Against Putting the Phenomena First: The Discovery of the Weak Neutral Current." *Studies in History and Philosophy of Science* 15:85–117.

———. 1984*b*. *Constructing Quarks: A Sociological History of Particle Physics.* Chicago: University of Chicago Press.

———. 1987. "Forms of Life: Science, Contingency and Harry Collins." *British Journal for History of Science* 20:213–21.

———. 1989. "Living in the Material World: On Realism and Experimental Practice." Pp. 275–97 in *The Uses of Experiment: Studies of Experimentation in the Natural Sciences,* edited by David Gooding, Trevor J. Pinch, and Simon Schaffer. Cambridge: Cambridge University Press.

———. 1990. "Openness and Closure: On the Goals of Scientific Practice." Pp. 215–39 in *Experimental Inquiries: Historical, Philosophical and Social Studies of Experimentation in Science,* edited by Homer Le Grand. Dordrecht: Kluwer.

———. 1994. "Beyond Constraint: The Temporality of Practice and the Historicity of

Knowledge." In *Theories of Practice/Stories of Practice,* edited by Jed Buchwald. Chicago: University of Chicago Press.

Pickering, Andrew, and Adam Stephanides. 1992. "Constructing Quaternions: On the Analysis of Conceptual Practice." Pp. 139–67 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

Pinch, Trevor, and Wiebe Bijker. 1984. "The Social Construction of Facts and Artefacts: Or How the Sociology of Science and the Sociology of Technology Might Benefit Each Other." *Social Studies of Science* 14:399–441.

Richardson, G. R. 1991. *Feedback Thought in Social Science and Systems Theory.* Philadelphia: University of Pennsylvania Press.

Rose, Nikolas. 1990. *Governing the Soul: The Shaping of the Private Self.* New York: Routledge.

Rose, Nikolas, and Peter Miller. 1992. "Political Power beyond the State: Problematics of Government." *British Journal of Sociology* 43:173–205.

Rouse, Joseph. 1987. *Knowledge and Power: Toward a Political Philosophy of Science.* Ithaca, N.Y.: Cornell University Press.

Rouse, W. B., and N. M. Morris. 1986. "On Looking into the Black Box: Prospects and Limits in the Search for Mental Models." *Psychological Bulletin* 100:349–63.

Schaffer, Simon. 1991. "The Eighteenth Brumaire of Bruno Latour." *Studies in History and Philosophy of Science* 22:174–92.

Shapin, Steven. 1979. "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes." Pp. 139–78 in *On the Margins of Science: The Social Construction of Rejected Knowledge.* Sociological Review Monograph, vol. 27. Edited by Roy Wallis. Keele: University of Keele.

———. 1982. "History of Science and Its Sociological Reconstructions." *History of Science* 20:157–211.

———. 1988. "Following Scientists Around." *Social Studies of Science* 18:533–50.

Smith, Barbara H. 1988. *Contingencies of Value: Alternative Perspectives for Critical Theory.* Cambridge, Mass.: Harvard University Press.

Star, Leigh. 1991. "The Sociology of the Invisible: The Primacy of Work in the Writings of Anselm Strauss." Pp. 265–83 in *Social Organization and Social Process: Essays in Honor of Anselm Strauss,* edited by David R. Maines. Hawthorne, N.Y.: Aldine de Gruyter.

———. In press. "The Trojan Door: Organizations, Work, and the 'Open Black Box.' " *Systems/Practice.*

Suchman, Lucy. 1987. *Plans and Situated Actions: The Problem of Human-Machine Communication.* Cambridge: Cambridge University Press.

Traweek, Sharon. 1992. "Border Crossings: Narrative Strategies in Science Studies and among Physicists in Tsukuba Science City, Japan." Pp. 429–65 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

Turner, Stephen. 1991. "Two Theorists of Action: Ihering and Weber." *Analyse & Kritik* 13:46–60.

Wise, Norton, and Crosbie Smith. 1989–90. "Work and Waste." *History of Science* 27:263–301, 391–449; 28:221–61.

Woolgar, Steve. 1981*a*. "Critique and Criticism: Two Readings of Ethnomethodology." *Social Studies of Science* 11:504–14.

———. 1981*b*. "Interests and Explanation in the Social Study of Science." *Social Studies of Science* 11:365–94.

———. 1992. "Some Remarks about Positionism: A Reply to Collins and Yearley." Pp. 327–42 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

Zuckerman, Harriet. 1988. "The Sociology of Science." Pp. 511–74 in *Handbook of Sociology,* edited by Neil J. Smelser. Beverly Hills, Calif.: Sage.