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Preface

One good thing about writing a book is that you get two tries at the introduction. Chapter 1 is a self-contained preview of the issues that concern me, how I propose to address them, and what I think my examples show. Here I take a more biographical route. It might help in reading the book to have an idea of how it arrived at its present shape.

I have been fascinated for a long time by knowledge. Since my schooldays, I have wondered about how knowledge relates to the world—about the problematic of realism, as philosophers call it. Having gained quite a bit of knowledge, eventually as an elementary-particle theorist, in 1976 I joined the Science Studies Unit at the University of Edinburgh, where I discovered that there were other people like me. The unit was one of the centers of a small community of people developing a sociology of scientific knowledge, and I tried as best I could to join in the absorbing but very difficult arguments that swirled around that topic. I worked through several case studies of my former discipline and finally wrote a book on it, *Constructing Quarks* (1984). By the time of that book, I had come to the conclusion that it was no use trying to think about knowledge in isolation. To understand why people believed what they did, it seemed one had to understand how specific items of knowledge fitted in with the practice of their producers and users. Indeed, *Constructing Quarks* tried to display the historical development of particle physics over a twenty-year period as instantiating a simple model of scientific practice. After the book, I was left with one nagging concern. The problem I started with had not been solved. I now felt clear enough on how knowledge related to people, but not on how it related to the world (except that it was implausible to imagine that scientific

knowledge literally corresponded to the inner constitution of nature). The problematic of realism was still with me.

The important development for me came when I spent 1986–87 as a member of the Institute for Advanced Study in Princeton. Shortly after my arrival, I set out to respond to an essay review of my book by Yves Gingras and S. S. Schweber (1986). They accused me, wrongly, of Duhem-Quine abuse—of mistakenly presupposing a set of arguments associated with the names of those philosophers. Thinking about this accusation led me to an appreciation of a fact that was already emphasized in constructivist sociology of science, namely that scientific culture is not a unitary, monolithic thing (say, a single big theory, as is typically taken for granted in Duhem-Quine-type arguments); that scientific culture is, in fact, an assemblage of multiple and heterogeneous elements. And I realized that I could, on this basis, at last say something about realism. An earlier study I had made of the history of quark-search experiments could be read as an analysis of knowledge production in terms of the difficult and uncertain work of making associations between heterogeneous—in this case, material and conceptual—elements of scientific culture. An essay followed, "Living in the Material World" (1989), in which I extended my previous account of the experimental work of the physicist Giacomo Morpurgo as an exemplification of a "pragmatic realist" perspective on knowledge, a perspective that deserved to be called realist precisely because it specified the nontrivial links that scientists fashion between representations and the world.

"Material World" was important to me for several reasons. First, as just mentioned, it addressed a problematic that had been with me for years. Second, thinking about cultural multiplicity and heterogeneity helped me to see what was original in the actor-network approach to science studies then being developed by Michel Callon, Bruno Latour, and John Law. I have continued to learn from the actor-network ever since. And third, on returning from Princeton to the University of Illinois, I realized that the essay could also be read as significantly extending the analysis of practice developed in *Constructing Quarks*. In the book, I had argued that scientific practice is centrally a process of modelling, but, looking back, I had failed to appreciate sufficiently the open-endedness of modelling. One can try to extend a given scientific culture in an indefinite number of ways. What the history of Morpurgo's experiments showed was that, to put it crudely, most of those ways do not work. When Morpurgo sought to extend the material and conceptual strata of his culture, the bits did not usually fit together. "Resist-

ances" continually arose in his work relative to the material-conceptual alignments he needed to achieve to produce facts. And, from the indefinite range of possibilities, certain specific modelling vectors were singled out in his practice precisely in that they did issue in such alignments. Practice as modelling, I thus realized, has an important real-time structure, with the contours of cultural extension being determined by the emergence in time of resistances, and by the success or failure of "accommodations" to resistance. This temporal structuring of practice as a dialectic of resistance and accommodation is, in the first instance, what I have come to call the mangle of practice. The mangle and its implications and ramifications in philosophy, social theory, and historiography are what this book is about.

Having arrived at the idea of the mangle, I began to wonder about its generality. The mangle operated, as I then understood it, at a level of detail not usually accessible to empirical study, but eventually I found and started working through the examples that follow here. From this work, it appeared that the mangle could go a long way, and thus, when I set off from Illinois to spend 1992–93 in the Department of History and Philosophy of Science in Cambridge, my ambition was to write a book about it. Then one last twist entered the story. Morpurgo's quark-search experiments had been the test case for the development of my ideas since I first saw how they could lead into the problematic of realism. For the book, I intended to extend my analysis of those experiments back into their earliest phase, and there I found myself on uncertain ground. I could not persuade myself of the story that I wanted to tell of Morpurgo's early struggles to get his apparatus to work. It was possible to talk about those struggles along lines that were becoming familiar—in terms of the making and breaking of associations between multiple and heterogeneous cultural elements—but something seemed to be missing. And that something, it appeared, was material agency. In building his apparatus, Morpurgo was trying to get the material world to do something for him, and this needed to be stated out loud. Having recognized that, my project reconstituted itself.

As discussed in the text, talk of material agency has always been suspect in the sociology of scientific knowledge, but not so in the actor-network approach. There, much is made of material agency and, further, of its symmetrical relations with human agency. It was clear, then, that if I wanted to talk about material agency I had better think what I wanted to say about human agency, too. In Cambridge, that led me in all sorts of directions: into considerations of the intentional structure of

human agency, of the scale of and relationships between social actors, of the disciplined nature of scientific work, and so on. At this late stage, I gained a new appreciation of writers as diverse as Michel Foucault and Michael Lynch and a heightened (but also critical) regard for the insights of the actor-network. And the book, in its turn, developed a vital interest in human and nonhuman agency, in how they temporally intertwine, and in how knowledge engages with them. I have thus ended up with a book that I never intended to write, in which an original preoccupation with knowledge has been subsumed into a wider preoccupation with human and nonhuman agency, and in which, as explained at the end, science itself appears as subsumed within a wider field of machinic production and destruction. I am no longer puzzled by how scientific knowledge relates to the world, nor by how it relates to scientific practice; now I feel the need to understand the disciplined, industrialized, and militarized, technoscientific world in which I have lived my life, and how it ever got to be this way.

Thus the history of the mangle. I hope it will help readers come to grips with the form and content of the chapters that follow.

It remains for me to acknowledge my many debts. As mentioned above, this book began in Princeton, at the Institute for Advanced Study. It also ended in Princeton, but at the university. In 1993–94, I completed and made final revisions to the manuscript as a fellow of the Shelby Cullom Davis Center in the Princeton History Department. In between, much of the work was done at the University of Illinois at Urbana-Champaign, and for their friendship, support, and critical acumen I thank my colleagues in the Sociology Department there, my students (especially those who took part in seminars where I stumbled through the earliest versions of arguments presented here), and, above all, the members of the informal seminar of the History, Philosophy, and Sociology of Science graduate program. The HPSS seminar was the visible manifestation of a social and intellectual community without which my work would have been impossible. Also at Urbana, participation in the Unit for Criticism and Interpretive Theory was my introduction to great swathes of contemporary thought, and the unit has to take its share of responsibility for the wilder ideas expressed in what follows. Two periods of support from the History and Philosophy of Science section of the National Science Foundation marked watersheds in my project. A grant in 1989–90 helped me find the time to escape from the detailed analysis of scientific practice and to start thinking about its implications, and a sec-

ond NSF grant enabled me to spend my sabbatical in Cambridge in 1992–93.

Now for the tricky part. More people than I deserve have helped me along the way to this book, reacting to talks, essays, chapters, entire manuscripts, sharing their ideas, yawning, telling me when I was talking rubbish, in correspondence, offices, seminar rooms, restaurants, homes, bars, and pubs. Increasingly over the past few years I have counted myself exceptionally fortunate that my thought and writing has been situated in a very rich and stimulating field of conversations. I therefore have no confidence whatsoever that I can generate a complete list of the individuals to whom I should express my gratitude. But still I should try, and I hope to be forgiven for what will no doubt prove to be appalling omissions. In footnotes to the text, I acknowledge specific debts, but here I offer my thanks in general to Susan Abrams, Brian Baigrie, Davis Baird, Barry Barnes, David Bloor, Geof Bowker, Nancy Cartwright, Soraya de Chadarevian, Harry Collins, Natalie Davis, Norman Denzin, Irving Elichirigoity, Paul Feyerabend, Owen Flanagan, Paul Forman, Peter Galison, Dilip Gaonkar, Gerry Geison, Yves Gingras, Laurel Graham, Ian Hacking, Mary Hesse, Joann Hoy, Piet Hut, Robert Alun Jones, Vera Ketelboeter, Yiannis Koutalos, Martin Krieger, Thomas Kuhn, Bruce Lambert, Michèle Lamont, Bruno Latour, John Law, Peter Lipton, Michael Lynch, Michael Mahoney, Peter Miller, Giacomo Morpurgo, Malcolm Nicolson, Ted O'Leary, Ronald Overmann, Trevor Pinch, Michael Power, Diederick Raven, Joseph Rouse, Simon Schaffer, Sam Schweber, Steven Shapin, Otto Sibum, Barbara Herrnstein Smith, Betty Smocovitis, Leigh Star, Adam Stephanides, Fred Suppe, Peter Trower, Stephen Turner, Adrian Wilson, Norton Wise, and Alison Wylie.

From that list, I should single out two people. Since our days together at the Institute for Advanced Study, Barbara Herrnstein Smith has offered me consistently perceptive advice and incisive criticism. The trajectories of our research run along intersecting lines; I have learned much from her writings, and even more in conversations and arguments with her. The other person is Simon Schaffer. At the beginning, it was he who encouraged me to write "Living in the Material World." Almost at the end, Simon, as acting head of department, made possible my sabbatical in Cambridge and, together with Anita, helped make it such a pleasure. While I was there, he somehow found time to read and comment upon not one but two quite different drafts of this book. And more than that, he typically displayed a clearer grasp of where I was going and how to get there than I did. It was an education to have the chance of interacting

with him over an extended period of time. So, thanks, Simon and Barbara. Thinking of Cambridge, I also want to express my gratitude to my old friends Jim and Rhonda and Lee and Paula for their help above and beyond the claims of friendship in the hassles of transplanting me, Jane, and the children back to our native land. I recall remarking on several occasions, and only partly in jest, that they had saved our lives. Most of my writing in 1992–93 was done at Lee and Paula's old dining table, though Paul Waldmann's carpenter's bench deserves a mention, too.

Lastly, as ever I thank Jane F.—oh, for everything.

I think by writing, and my route to this book has been marked by a trail of essays, first stabs at stories and analyses that, for the reasons described above, especially my newfound concern with questions of agency, appear here and there in the text, transformed, redistributed, and accompanied by new material. The essays that have seen print, and the chapters to which they relate, are "Living in the Material World: On Realism and Experimental Practice," in D. Gooding, T. J. Pinch, and S. Schaffer, eds., *The Uses of Experiment: Studies of Experimentation in the Natural Sciences* (Cambridge: Cambridge University Press, 1989), pp. 275–97 (chap. 3); "Knowledge, Practice, and Mere Construction," *Social Studies of Science* 20 (1990): 682–729 (chaps. 6, 7); "Objectivity and the Mangle of Practice," *Annals of Scholarship* 8 (1991): 409–25 (chap. 6); "Constructing Quaternions: On the Analysis of Conceptual Practice," co-authored with Adam Stephanides, in Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992), pp. 139–67 (chap. 4); "Anti-Discipline or Narratives of Illusion," in E. Messer-Davidow, D. Shumway, and D. Sylvan, eds., *Knowledges: Historical and Critical Studies in Disciplinarity* (Charlottesville: University Press of Virginia, 1993), pp. 103–22 (chap. 7); "The Mangle of Practice: Agency and Emergence in the Sociology of Science," *American Journal of Sociology* 99 (1993): 559–89 (chaps. 1, 2); and "Beyond Constraint: The Temporality of Practice and the Historicity of Knowledge," in J. Buchwald, ed., *Scientific Practice: Theories and Stories of Physics* (Chicago: University of Chicago Press, 1995) (chaps. 2, 5, 6).

ONE

The Mangle of Practice

[T]here 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.*

Gilles Deleuze and Christine Parnet, *Dialogues*

This is a book about science that ventures into the worlds of mathematics, technology, and the workplace. It offers a general analysis of scientific practice, which I call the mangle, and some pointers as to how it might be extended toward an understanding of the reciprocal production of science, technology, and society (STS).[1] It is also a book about time and agency that addresses central questions in the philosophy, social theory, and historiography of science and beyond. This chapter lays out some basic features of my position; the rest of the book consists of examples and articulations.

1.1 SCIENCE AS PRACTICE AND CULTURE

Science studies has been an exciting field over the past few decades, and one source of this excitement has been a continual expansion of conceptions of science as an object of study.[2] Until the late 1950s, it seemed

1. I hope, therefore, that "science" will be read hereafter as an umbrella term of a greater than usual extent.

2. The core fields of "science studies" are history, philosophy, and sociology of science but, as indicated in the previous note, I construe the term broadly to encompass both

enough to think of science as a body of knowledge, a collection of empirical and theoretical propositions about the world. This body of knowledge constituted a self-contained topic for the philosophy of science, for example, whose job it was to enquire into, and possibly to legislate upon, the formal relations between the propositions it contained. But the work of Norwood Russell Hanson (1958), Thomas Kuhn (1970) and Paul Feyerabend (1975) changed all that.[3] Especially Kuhn's persuasive periodization of the history of science into stretches of "normal science" separated by "revolutionary" gulfs challenged the self-containment of the science object, and opened the way for new waves of scholarship to wash over and reconceive it. Thus, since the 1970s, work on the sociology of scientific knowledge (SSK) has increasingly documented the importance of the human and the social in the production and use of scientific knowledge.[4] Social structure, social interests, human skills—all of these have come to be seen as constitutive of science, as integral to science in interesting and important ways. Further, though SSK's primary focus has been, as its name states, on knowledge and the social, empirical work in SSK has served also to foreground the material dimension of modern science—the omnipresence of machines, instruments, and experimental setups in scientific research. This dimension had long been ignored in mainstream history and philosophy of science, but here SSK has made contact with an alternative philosophical perspective powerfully articulated by Ian Hacking (1983), which seeks precisely to emphasize the machinic aspects of science. Finally, from the late 1970s to the present, scholars have evinced an increasing interest in the details of the day-to-day doing of science. This interest has served further to expand our conception of the science object by documenting its sheer multiplicity and heterogeneity. All of the dimensions of science

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contributions from other disciplines interested in science (anthropology, political science) and work that extends into the study of mathematics and STS.

3. Suppe (1977) surveys developments in philosophy of science consequent upon the work of these authors.

4. Canonical books in SSK include Barnes 1974, 1977, 1982; Bloor 1991, 1983; Collins 1992; and MacKenzie 1981b. See also Shapin (1982) for an excellent review of the history and sociology of science literature from an SSK perspective. I should explain why I single out SSK here, rather than the preexisting Mertonian approach to the sociology of science (for a comparative review of both, see Zuckerman 1988). Although the latter did expand our conception of science as an object of study by exploring its institutional structure, it did not envisage the detailed intertwining of the social with the technical in science that is a major concern in both SSK and this book. The Mertonian approach, as it is said, has been more a sociology of scientists than of science.

just mentioned—the conceptual, the social, the material—have to be seen as fragmented, disunified, scrappy.[5]

I can now sketch out the problematic of this book by, first, reexpressing what has just been said in terms of an expansion of our concept of scientific *culture*. Whereas one could once get away with thinking of scientific culture as simply a field of knowledge, in what follows I take "culture" in a broad sense, to denote the "made things" of science, in which I include skills and social relations, machines and instruments, as well as scientific facts and theories. And then I can state that my abiding concern is with scientific *practice,* understood as *the work of cultural extension.* My problematic thus includes the traditional one of understanding how new knowledge is produced in science, but goes beyond it in its interest in the transformation of the material and social dimensions of science, too.

Two points of clarification should immediately be entered. One is that in this book I seek a *real-time* understanding of practice. I want to understand the work of cultural extension in science as it happens in time. This is to be contrasted with retrospective approaches that look backward from some terminus of cultural extension and explain practice in terms of the substance of that terminus. The exemplary instance of the latter is what I call "the scientist's account" (Pickering 1984b), in which accepted scientific knowledge functions as an interpretive yardstick in reconstructing the history of its own production. I think that there are serious historiographic problems in such retrospective accounting for science (Pickering 1989b), but rather than rehearse them here, let me just note that my project is a different one and that, for my purposes, to indulge in retrospection would be circularly self-defeating and must be eschewed.

My second point of clarification takes us back to the recent history of science studies. It is probably true to say that many authors engaged in exploring the work of science once shared my interest in time—back

5. Several streams of work helped to constitute the doing of science as a topic for research in its own right. Within traditional history of science, Holmes (1974, 1981, 1985) has pursued the theme most tenaciously (an important recent study is Kohler 1994), but perhaps the most influential source has been the development of ethnographic studies of "laboratory life": Latour and Woolgar 1986; Knorr-Cetina 1981; Lynch 1985a. From another angle, the interest in scientific work appears as a continuation of the concerns of SSK: Pickering 1984b; Gooding 1990. Yet another source has been pragmatist studies of science (which take the work of Howard Becker and, especially, Anselm Strauss as their point of departure): see Star 1991b, 1992; and Fujimura 1992. For surveys of current perspectives, see Pickering 1992b; and Clarke and Fujimura 1992.

in the early 1980s, say. But as I have been writing this book, it has dawned on me that a kind of purification has taken place. Much of the most interesting work now being done is not concerned with practice as I have just defined it, but takes the form of atemporal cultural mappings and theoretical reflections thereon. My present interest in the temporality of cultural extension leaves me, I think, in a minority as far as current initiatives in science studies are concerned.[6] I say this not in a spirit of critique of what I call the cultural-studies approach—in fact, I draw extensively upon its findings as the book goes on, and I argue in section 7.2 that it is complementary to my own approach—but to make clear the tendency of this book. And in this connection, I think it will help at this stage to note an ambiguity in the word "practice," an ambiguity that explains why it took me, at least, so long to realize that my project had diverged from others.

One sense of "practice" is the generic one around which all that follows is organized—practice as the work of cultural extension and transformation in time. The other sense of "practice" relates to specific, repeatable sequences of activities on which scientists rely in their daily work—things like the "plasmid prep" in molecular biology, discussed by Jordan and Lynch (1992). Unlike my generic sense of "practice," this one has a plural form; one can talk, for instance, of a distinct set of practices as characteristic of a given science or laboratory. And—this is the important point—*practices,* on my definition, *fall into the sphere of culture,* and the study of practices in their own right falls into the domain of what I just called cultural studies. In contrast, I am interested in practices not so much in themselves but inasmuch as they are among the resources for scientific practice and are transformed (or transformable) in practice, alongside all of the other components of scientific culture. I return to the practice/practices distinction in section 1.3, but for now the simple message is that thinking about science bifurcates in the

6. I cannot offer statistical evidence on this point, and it would not affect my arguments in the rest of the book if I were wrong. But still, my reading of the latest works of the science-studies authors I most admire tells me that they are largely engaged in projects different from my own. As an example of this, when I look back on the book I recently edited, *Science as Practice and Culture* (1992), I see now, as I did not then, that only Gooding's essay there shares my consistent interest in cultural transformation in time. The actor-network approach to science studies discussed in section 1.3 has been very important to my own thinking about the temporality of practice, but has largely been developed and appropriated by others in a cultural-studies mode, perhaps because many of its key terms—"centers of calculation," "obligatory passage points" as well as "network" itself—are terms of art in cultural mapping.

act of deciding whether "practice" has a plural or not. In this book, I take the latter fork.

1.2 REPRESENTATION AND PERFORMATIVITY

> Satisfied that the sequence of men led to nothing and that the sequence of their society could lead no further, while the mere sequence of time was artificial, and the sequence of thought was chaos, he turned at last to the sequence of force; and thus it happened that, after ten years' pursuit, he found himself lying in the Gallery of Machines at the Great Exposition of 1900, his historical neck broken by the sudden irruption of forces totally new.
>
> Henry Adams, *The Education of Henry Adams*

Before I get into the details of my understanding of scientific practice, I need to talk about the metaphysics that informs it. In particular, I want to contrast what I call the representational and performative idioms for thinking about science. The representational idiom casts science as, above all, an activity that seeks to represent nature, to produce knowledge that maps, mirrors, or corresponds to how the world really is. In so doing, it precipitates a characteristic set of fears about the adequacy of scientific representation that constitute the familiar philosophical problematics of realism and objectivity (which I discuss in chapter 6). Of course, within the traditionally restricted vision of science-as-knowledge, the representational idiom is more or less obligatory—what else can one ask of knowledge other than whether it corresponds to its object?–but it has continued strongly to inflect our understandings of science even to the present. It has been taken to a new pitch of intensity, for example, in the reflexive approach to science studies developed in the works of Michael Mulkay (1985), Steve Woolgar (1988b, 1988c), and Malcolm Ashmore (1989). Reflexivity turns the usual fears concerning the adequacy of representation—the "methodological horrors," as Woolgar calls them—back upon science studies itself, but never doubts that the point of science and science studies is indeed representation.[7]

Within an expanded conception of scientific culture, however—one

7. Woolgar (1988a, 1992) makes clear the connection between the reflexive turn in science and that taking place more generally in the human sciences, especially in anthropology. The grip of the representational idiom is exemplified in Lynch's essay "Representation Is Overrated" (1993a). Lynch's conclusion is that we need to study representation differently, not that we should escape from the representational idiom.