Muddling Through

Pursuing Science and Truths in the 21st Century

Mike Fortun and Herbert J. Bernstein

O U N T E R P O I N T

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Prologue

Just because we are finite beings, located, situated, embodied, we can, and can only, muddle through. . . . Scientists muddle through with staggering success. Only their success is rather different than they imagine. It depends not on any possibility of translating thought into action, but on the conjoining practices of a colluding community of common language speakers. Our task . . . is to make sense of the successes of science in terms of the particular linguistic and material conventions that scientists have forged for their own sorts of muddling through.

Evelyn Fox Keller[1]

For the scientists who work within them daily, and for the people who avidly tune in to their work through the media, the sciences are an endless source of exhilaration, insight, and invention. The sciences demand some of our best thinking and most dextrous manipulations. In terms that seem utterly dependable and authoritatively final, the sciences tell us about what's truly happening in the genes, cells, and organs of our bodies; about the evolution of life; about how our brains and minds really work, as well as about the brains and (perhaps) minds of computers; about the lawful and awe-ful realities of subatomic particles and vast cosmological stretches of time and space. The ingenuity of scientists connects to the intricacies of nature, and the resulting combination of the these forces feeds us, heals us, transports us, inspires us.

Yet few things are more unsettling than working in or observing the sciences today. Yesterday's truths are quickly forgotten, made obsolete by the truer truths just released. Medical breakthroughs soon show serious limitations or disturbing side effects. The next cosmic discovery will cost taxpayers a billion dollars more than the previous one. Many scientists with great ideas see those ideas go unsupported or undeveloped, and they encounter a public that is often uninterested, ill-informed, or hostile. Urgent controversies over health, behavior, and environment resist consensus and often seem only to splinter into bitter disagreements *among* scientists. Nature seems not only more and more complex but also opaque and even downright ornery. And so the problems pile up, and distrust and disillusionment set in.

These are contradictions, to say the least. This book will do its work not by resolving them, but by asking the questions that fall in between. Making sense of these contradictions is perhaps the hardest challenge for democracy in the twenty-first century. Of course we need to cultivate scientific literacy, but first we need to ask some basic questions about how the sciences are produced, applied, and understood.

The sciences of today create the worlds of tomorrow. They show their effects on our bodies, our conceptions of self, and our polity. Yet the sciences continue in large part to be viewed as a resource of absolutely certain, objective answers gleaned from a pure and exact "scientific method." This is, to put it bluntly, wrong, and all the really great (and some not so great) scientists know it. They know that the relationship between creative scientific inquiry and the requirements of a pluralist democratic society is anything but the straightforward application of supposedly neutral facts to social problems. They know that today we are living "after the fact," in a world in which science can no longer be regarded as the oracle of those cultural and material reforms necessary to a just society.

This world demands a kind of literacy that makes sense of the sciences not in terms of infinity and transcendence, but of finitude and location; not in terms of awesome translations from the real to the ideal, but of complex conjunctions and collusions among things, words, and deeds; not in terms of a book of nature discovered and decoded by a small group of experts, but of an ongoing essay written and spoken by many, in a shifting, generative language. In short, we have to engage with the sciences as the kind of activities they have always been, the *only* kind of activities they can be: muddling through.

Muddling Through is a book about the sciences in the late twentieth century and about the kind of sciences we need for the twenty-first. It is a book about how the sciences make sense of the world and provide sense to the world. Think of *Muddling Through* as the basic text for a different kind of science literacy project, a project to reimagine and then enact the sciences as operations of language and thought and as attempts, trials, *limited experiments* involving things, ideas, and just about everything in between.

This is also a book about politics (not policy) and culture—that is, about how the sciences are made through arduous and diverse political processes. This book is about how the sciences affect politics not only through technological invention but by generating the images and metaphors that we apply to every situation and phenomenon we encounter, and by providing the blueprints we use to make and legitimate crucial social decisions. The connections between the sciences and democratic pluralism need to be revitalized, through both new concepts and innovative social forms.

We use the plural "connections" deliberately to illustrate that the sciences and democracy must link up at many levels, from the policy panels in Washington and Brussels to the workings of curiosity and inquiry in each of us. Democratic society needs pluralism and participation not only in the application of science, but even more importantly in its production. In the words of François Jacob: "An age or a culture is characterized less by the extent of its knowledge than by the nature of the questions it puts forward."[3] We want *more* people asking *more*, and different, kinds of questions about what's "really" the case, and we want them in the laboratories, in the field, in the hospital wards, in the classrooms, and on the funding panels.

For this kind of social innovation to happen, and to happen in a way that helps produce good science rather than being simply prohibitive, people need an understanding of the sciences that is more complex than conventional accounts provide. Such accounts often hinge on the image and imagination of the hero, a "great scientist" like Einstein or Newton. But the notion of greatness making history, and as the proper basis for writing history, is passé. Still, there will be many places in this book where we will invoke the words of people from the high culture of science. In doing so we play a risky double game: using their authority and heroic stature not to undermine that authority or stature but to call them into question. Where, precisely, does the force of their ideas come from? Was it wrested from nature? Did it spring full-blown from their minds? Or are other things, other processes, and other people involved? Indeed, many of the scientists we discuss have often been most skilled at calling their own authority, as conventionally understood, into question. At their best, the sciences themselves put both the world, and their own processes of questioning that world, into the picture, the frame of inquiry. We also use recent historical and cultural explorations of some of the great figures of science—Copernicus, Galileo, Darwin—to work toward a more complex rendering of what the sciences actually are, and why they are often so successful, as well as sometimes risky and destructive.

Even so, the sciences are far more than what these noble figures represent. So many books today take that focus, purveying the conventional picture of science as an exalted mode of wondrous discovery, intellectual adventure, and abstract theorizing, undertaken by a chosen few. We choose to include stories about lesser-known scientists, with an emphasis on the sciences as practice, an activity that is socially complicated as well as intellectually complex. Our stories come not just from the realm of high culture and theory but from the laboratory, the courtroom, the toxic waste site, the popular television program.

These stories are more frequently included, and at greater length, than the reader may be used to finding in books of this type. We do this because it is necessary both to convey the complexity of the issues involved, the enormous amount of detail and the subtlety and difficulty of the questions, and to provide a better sense of just what kind of remarkable achievement the sciences in fact represent. We mix the old and the new, jumping across time, disciplines, and cultures. We range all over the scientific territory, displaying fragments that don't always assemble into an overall lesson, a moral, a sense of certainty, or some other comforting whole. We want these brief, detailed, but incomplete looks into particular episodes to spark skeptical interest and further inquiry. And while we certainly want to convince and persuade, we do not want to oversimplify. Above all, we want to upset faith in, and move far beyond, the usual accounts of the sciences.

The familiar story goes like this: The "scientific method" always starts from stable, given facts—observations, measurements in the form of numbers, isolated and purified substances that are part of an unchanging, solid reality. Logic then compels the assembling (a process carefully controlled by existing theory) of these indisputable pieces of the real world into a theory which literally re-presents that world: a perfect match that, when done correctly, admits no doubt. The theory is checked by the fact, by the real world, and underwritten by the rigor and purity of the scientific method. Together, hard fact and reliable method provide the sciences with a unique and powerful tool for self-correction that eliminates (in the long run) all forms of bias and error, yielding a neutral and objective progressive approach to equally neutral and objective final truths. Fact and method thus answer what we call the "really?" questions: Does this particular chemical really cause cancer? Is intelligence really genetic? Is the physical universe really made up of quarks and leptons, held together by bosons? Is biodiversity really important for the survival of the planet? Is homosexuality really rooted in biology? And so on. These answers, arrived at free from the disturbing influences of culture, society, language, or political power, can then be applied with the utmost assurance in the larger spheres of society and politics. Because of their faithful objectivity and scrupulous neutrality, the sciences can be a social resource—solving problems of health, hunger, and communication—precisely because they are immune to social or political influences. Their hard-won apparent neutrality, paradoxically, makes the sciences socially powerful.

So much for conventional accounts. We can no longer excuse the errors and simplifications on which such popular tales are based, no longer afford to have this view of the sciences circulate in the social and political realms. The stakes are too high.

If there is a single concept central to the errors, simplifications, and negative social effects of this conventional account of the sciences, it is purity. And against this concept's family of terms—purity, pure, purists—we will run our own family of terms: muddied, muddled, middled, messy, complex, hybridized, and many others. We are not particularly fond of such stark oppositions as pure/muddled, as the reader will see over the course of this book, but occasionally they come in handy. It is better to think about the sciences as muddled rather than pure; to imagine the borders between the sciences and the worlds of language, culture, and politics as muddied rather than clear and distinct; to know scientists as complex hybrid figures rather than rarefied heroes; to see the work of the sciences as a complicated interaction with a messy world, an exchange involving tools, words, things, and even more nebulous entities, rather than a methodical, pristine encounter between mind and nature.

Our point is *not* to drag the sciences through the mud, nor to dismiss the potential of science for social reform. Nothing we write here should obscure the passion we have, and the drive that the "great scientists" embody, to create truly embracing, challenging, and productive encounters with the world. Pursuing the sciences can be an amazing enterprise of rigorous thinking and subtle guessing, creative manufacturing and respectful listening, exercised will and inflicted surprise, hard work and even harder play. We need the sciences now more than ever, but we have to have them reimagined and re-formed—re-formed by attention to their own history, by overt attempts to enact them differently, and by new protocols for questioning and judging the sciences as they develop.

In these times when, as Avital Ronell phrases it, "America is being emptied of the desire to know,"[3] anyone who critically questions the sciences runs the heavy risk of being labeled "antiscience," a charge leveled as liberally as "anti-American" once was. If we have to make such silly generalizations, our preference is to say that we are simply "pro-inquiry." Each of us is a committed practitioner of the craft in which we were originally trained, yet we cross the lines as well. The physicist (Bernstein) appreciates the work of scholars in the fields of science studies, and considers it, at its best, to be just as robust, intellectually demanding, important, and valid—and just as fallible and culture-bound—as the sciences. The historian of science respects the work of scientists, knowing the power, creativity, efficacy, and legitimacy of the sciences—and their limitations and social embeddedness—through historical, philosophical, and cultural analysis. We believe that not only is such a dialogue between these endeavors possible, but that such collaboration and hybridization can produce both better science and better scholarship on the sciences, as well as more democratic social processes.

Expanding on Evelyn Fox Keller's imagery in the opening quote, we argue that the pursuit of both the sciences and of democracy is best imagined and enacted as "muddling through." Few things are more dangerous than unmuddled absolute faith in any answer or method, scientific or political. When it comes to the sciences, there are no simple answers like "just purify them," "just add values to them," "just keep them in their place," "just get rid of them," or even "just democratize them." They can't be pure, they already have values, they're everywhere, we can't get rid of them even if enough of us were stupid enough to want to, and democratizing them is an experiment, not an answer. We reject these and all similar imprecise, grand formulations: that the sciences disenchant the world; that they contribute to the loss of our souls; that they mechanize and reduce an organic, holistic cosmos; that they are essentially a violent way of knowing; that they destroy community; and so many more. Very high-minded, and very unhelpful—which is hardly surprising, since these expressions of the problem depend on concepts that are just as pure and idealized (souls, wholes, communities, and values) as their counterparts in the sciences which they aim to oppose.

Throughout this book we will show the way such paired sets of opposites recur both in and around the sciences. Dichotomies such as science/antiscience, mechanical/organic, fact/value (and countless others) structure the way we do and think about the sciences. While such polar oppositions are in some sense inescapable, new scientific literacies will depend on getting *in between* them. It is in the in-between, the muddled middle, where change happens, where creativity can be found, where the new emerges, where abundance dances—where the sciences *are* the sciences.

The in-between is the place scientists have in fact sought out and worked in for hundreds of years, and which they still seek today. While their own public representations of their work and the ways in which we hear about it in the media emphasize the seemingly miraculous, wondrous, and powerfully theoretical aspects of scientific inquiry, another part of the story that is usually (but not always) subsumed is the trial-and-error method: days, months, and years of what many scientists call "tinkering"—trying to get a piece of equipment to work properly, interpreting messy data, separating signal from noise, articulating new theory and explanations for new phenomena. Any decent scientist knows that results and explanations are always open to revision—indeed, *must* be revised if those results and explanations are to mean or work for anything. Answers are always only temporary, one-off, close but no cigar. They are guaranteed to make themselves obsolete by virtue of their own inescapable insufficiency.

The middle is also where uncertainty, risk, chance, and error can be found, and where one is therefore best advised to "muddle through." "Muddling through" is by no means a perfect principle—how could it be, centered as it is on imperfection?

The political scientist Charles Lindblom wrote an important article in 1959 titled "The Science of Muddling Through." Lindblom was working in a particular historical context in which operations research and systems theory were becoming quite powerful in organizational theory and in government policy. These disciplines claimed to be comprehensive and strictly rational, taking every possibility into account, optimizing material outcomes and maximizing efficiency while minimizing social conflict, providing the best solution to differences of values and goals. Lindblom called them "root methods," and while they were rarely practiced as precisely as they said they could be, and just as rarely yielded their intended results, these root methods, these total sciences were formalized as the best theory to be taught in professional schools and to use policy circles.

To this Lindblom opposed the "branch method," the "muddling through" practiced in the real world by administrators and policy makers, but hardly ever acknowledged as systematic and knowledge-based. "Muddling through" worked with incomplete information, lack of time and resources for full analysis of all factors and options, irreducible conflicts of values and political choices, the pressure of outside interests, the inertia of past decisions, and so on. Branch methods were better, in Lindblom's articulation, because they acknowledged finitude, the necessity and value of compromise, and they admitted that the lack of a system could itself be systematic in its own way. There was no sense of finality, an analysis accomplished and set in motion; you always had to go back again and again. Moreover, even when people claimed to be doing totally rational and systemic analysis, they were actually muddling through—they *had* to, given the complexity of the problems and systems with which they were dealing. Abstract root methods may have been the idealized theory, but muddling branch methods were the actual, empirical practice.[1]

We argue many of the same points for the sciences in general. Acknowledging that things are muddled seems to make it difficult if not impossible to render the kinds of judgments about the sciences that we say our society sorely needs. Judging muddles is difficult, as we'll see; it is *not* impossible. Still, there is another twist: imperial "muddling through," whether British or Austro-Hungarian, relied on the shared assumptions and prejudices of a gentry class whose education so set their thinking that improvised and muddled solutions—creative as they otherwise might be—nevertheless resulted in solutions that preserved class privilege. We have to be on the lookout for these kinds of effects, the way that the terms and frames of inquiry can sneak back up and surprise or contradict us.

We ask the reader to be patient, to let our argument emerge, in true muddling fashion, over the course of the book. While we will return again and again to the domain of the in-between, to the middle zone between opposed terms or viewpoints, to the place of compromise and negotiation, this does not mean that muddling through amounts simply to a desire for a happy medium, in political or intellectual terms. Instead, it entails a commitment to the "unhappy middle." Far from being indecisive, noncommittal, or blandly middle-of-the-road, muddling through is marked by perseverance: a dogged pursuit and relentless enactment of both pointed inquiry ("How do you know that?") and thoughtful social and political work ("Given *that*, let's try this . . . "). As a result, it does not imply muddleheadedness, but in fact the opposite. Through precise description of the complex ways that the work of our heads, our hearts, and our tongues intersects in the sciences, and how those intersections are in turn inextricably linked to events and movements in culture and politics, muddling through represents the only inquiry adequate to the monstrous, rapidly changing worlds of both nature and society. Recognizing its own limits, it suggests only actions that are cautious and responsible. And if it lacks unwavering belief in timeless principles, it makes up for it in vigilance, inventiveness, and democratic spirit.

How, then, do we use this book to turn muddling into muddling through, to steer through the bountiful and treacherous waters of the sciences today? Chapter by chapter, Section 1 introduces and elaborates on four navigational tactics that together define the method of muddling through. They are not only definitions, however; they are also responses to some hoary assumptions about the way science is thought about and conducted, namely, that facts are found, that theory and language mirror the world, and that science is a politically and culturally neutral tool.

First, *Facts are not found, but made*. The scientific method does not discover truth, it produces it. Chapter 1 thus focuses on *experimenting*: it is at this middle level that the muddle between facts and theories in the sciences is most easily located. We avoid unquestioned theoretical abstractions, grounding our inquiries instead in the realm of human activities, where flesh-and-blood people negotiate with cranky equipment, murky concepts, and an evasive "nature." One of the most important stories told here concerns the particular social function played by facts and the experimental production of facts in seventeenth-century England, the time and place in which the sciences first became truly experimental. All of the stories in this chapter help us see how facts are made, without being made up, and how facts should always be subject to extensive inquiry.

Second, *Theory and language refract the world, not reflect it*. Chapter 2 turns to *articulating*, focusing on the array of activities that produce what are conventionally referred to as scientific theories, as well as the broader narratives, world views, and interpretations that supplement their meanings. The notion of articulating steers us away from conceptions of theory as mirrorings of a world composed of atomistic facts, and toward an understanding of (many kinds of) theory whose status as "truth" depends less on faithful reflection of a preexistent world, than on the viability, strength, or robustness of a tangle of connections, or articulations. We employ at least two usages of the word "articulation": One, we argue that pursuing sciences requires a better understanding of how, and where, language works, that the sciences involve a struggle to articulate something that has never been said before, an attempt to put new things into new words (and new words into new things). Two, we use "articulation" to refer to the way the sciences are coupled and jointed: a vast connection of words, things, instruments, social trends, and funding sources. These articulations reach down into the level of experimenting and the experimental production of facts, across to other articulations from the same or other fields of thought, and up into the domain of culture and its narratives, and their embeddedness in social institutions. Our challenge is to demonstrate how the appeal and effectiveness of the sciences come not from their mirroring of nature, but from the density and peculiar strengths of these webbed linkages.

Which brings us to our response to the third assumption: *Science is never neutral, but always charged, moving in a field of cultural and political forces.* The sciences are not tools to be wielded for good or evil by the powers that be, but inquiry infrastructures composed not only of instruments, theories, and language but of larger institutions and their material and cultural resources. Thus, instead of reinforcing the usual distinctions between knowledge and power, reason and force, we introduce in Chapter 3 the analytic of *powering/knowing*. Using Galileo and Darwin as central examples, we show how the conventional ideals of knowledge and reason unsullied by baser considerations of power and resources do indeed require rethinking. But that doesn't mean simply that might makes right. Good science has always depended on a variety of power sources, and the sciences have always been an active, charged matrix remarkably sensitive to the pushes and pulls of seemingly distant ideas, institutions, people, culture, and, of course, capital. But the "charges" between the sciences and their historical and social contexts are contingent rather than determined; the affinities among the sciences, politics, and cultures are sometimes coarse and commanding, but just as often supple, subtle, delicate, and indirect. In any case, these contingent affinities are quite real; they shape and shade what we know, what we call truth, reason, nature, and justice.

If the purity and objectivity of the sciences were once guaranteed by their freedom from the corrupting influences of power and their faithful mirroring of a real world, what upholds and legitimates a system built out of experimenting, articulating, and powering? If the sciences are geared less toward faithful, objective representations of a primordial reality, and more toward the production of novel effects and entities, new social possibilities and unheard-of ideas, does this mean that anything goes? Can we construct facts or theories according to personal or political whim? We take up such knotty issues in chapter 4, and suggest that the demanding and difficult process of *judging* should be installed near the center of the complex webs spun through the sciences. We discuss notorious historical episodes such as the legitimation of eugenics in Germany and the United States, and Lysenkoism in the Soviet Union, and equally tangled current dilemmas posed by toxic torts and scientific fraud, to show how ever-present ambiguity and the volatile mix of the political and the scientific demand subtle, thoughtful, yet ultimately risky acts of judgment, every step of the way.

Thus, in Section I we are committed to muddling *up*: messing with conventional understandings of the sciences, blurring the boundaries between supposedly distinct things like facts and theories, hybridizing categories like "cultural" and "scientific," complexifying the figures of famous (and not-so-famous) scientists and the variety of forces which they drew upon and unleashed. Scientists (at least the really good ones) have always been adept at such muddling up. It has been crucial to their success, even if they weren't always aware of it, and we have much to learn from it. Scientists tweak their instruments and experimental setups to break up current theories. They imagine and articulate new ideas to order previously disorderly and nonsensical experimental results. They judge, they guess, they leap logical gaps, they combine rigor and risk, they cobble together money, time, people, ideas, and a host of other things. Section I explores these unmethodological methods, confirming that close inquiry into the practices of the sciences can help us understand the vital role they have to play in our future.

If Section I is about muddling up, Section II can be thought of as accounts of muddling *in*: getting one's hands dirty, running new experiments, creating new institutional resources, organizing communities. If Section I develops one meaning of "after the fact"—developing the tools and questions for living in a spacetime where facts are no longer as straight as we liked to think they were—then Section II interprets "after the fact" in another way: as the active pursuing of the sciences, chasing after them, *wanting* them.

Chapters 5 through 8 detail, respectively, efforts to clean up the military's toxic wastes at Westover Air Reserve Base in Massachusetts, an emergent illness that has been dubbed multiple chemical sensitivities (MCS), the articulations between health and "human nature" produced in the fields of molecular biology and biotechnology, and current work at the theoretical and experimental frontiers of quantum mechanics.

These chapters vary somewhat in voice and tone among themselves and from the rest of the book. The mixture of you-are-there reportage and more detached accounts reflects our differing degrees of direct, personal involvement in the stories told. But all of the accounts detail the critical tensions involved in work within the sciences and the many ways in which culture and the sciences both collude and collide. They show that the potential for pluralized democratic engagement with technical problems *does* exist, as does the urgent need for new ways to think about, and take, responsibility within the sciences.

After muddling up and muddling in, we reiterate some of the processes, promises, and problems of muddling *through* in Section III, an essay on the guiding principles and methods for the scientific literacies we hope to encourage.

Reimagining and reenacting the sciences in a democracy is a demanding project. We will all have to develop a stomach for contradictions and ambiguity and find ways to ask yet another question. Because every insight we gain will be accompanied by a certain blindness, we will have to keep experimenting—with new substances and machines, of course, but also with new habits of thought, new languages, new practices, and new colluding communities.

All of which is a fairly direct introduction to the content, questions, and themes of the book. But pursuing science is never so direct (for which we should be grateful). So let us proceed as the sciences proceed in their modes of inquiry and action—by indirection as well as direction, by the meander as well as the beeline, by uncontrollable excess conjoined with careful delimitation, by trial and error . . .

PART I

"...practicing a rationality..."

I think that the central issue of philosophy and critical thought since the eighteenth century has always been, still is, and will, I hope, remain the question: what is this Reason that we use? What are its historical effects? What are its limits, and what are its dangers? How can we exist as rational beings, fortunately committed to practicing a rationality that is unfortunately criss-crossed by intrinsic dangers? One should remain as close to this question as possible, keeping in mind that it is both central and extremely difficult to resolve. In addition, if it is extremely dangerous to say that Reason is the enemy that should be eliminated, it is just as dangerous to say that any critical questioning of this rationality risks sending us into irrationality.

—Michel Foucault

The effort really to see and really to represent is no idle business in face of the *constant* force that makes for muddlement. The great thing is indeed that the muddled state too is one of the very sharpest of the realities, that it also has color and form and character. . . .

—Henry James, *What Maisie Knew*

Experimenting

Invisible Lines of Force

The most primitive things in conventional accounts of the sciences are facts. Facts are supposed to be "brute": stubborn, unchangeable features of the world which serve as the building blocks of science and its theoretical representations. That the word "brute" is usually attached to facts signals their primal, even violent, nature: If you ignore the facts, you're going to get hurt.

When something is brute, it's beyond—or *beneath*—argument. Hence the persistence in the memory of many scientists, and in their diatribes against antirealists and social constructionists, of stories like the eighteenth-century English author and dictionary-maker Dr. Samuel Johnson (faithfully reported by the Johnson-fact-obsessed Boswell) kicking a stone, thus refuting idealist philosopher George Berkeley: *POW! Berkeley's talking trash, so what's the point of listening or debating?* Or the even more apocryphal story of Galileo leaving his Vatican trial, having agreed to recant his heliocentric teachings, and stomping his foot: *CLOMP! And yet it moves!* Or today, when a sociologist challenges the naive realism of scientists, the most frequent response that a scientist makes in defense: *You're a social constructionist? Try stepping out of an airplane: AAAAAIIIEEEEEEE. . . . . WHOMP!* Almost before it begins, the argument always ends with some kind of thud. The whole thing starts to look like the comic book version of science that it is, superheroes slugging it out with villains in confrontations that require only minimal and guttural texts in balloons.

Matters of fact—or questions of what facts are and do— demand much more subtle treatments. Surely there are ways to approach facts, ask questions about them, that fall between these polemical caricatures. To begin with, you could ask what facts are supposed to do according to conventional philosophy of science. That is: what are facts, ideally? Then you could go on to ask how (and why) facts have assumed that role historically. How do people in various situations and in different historical periods go about deciding what is or is not a fact? In other words: what are facts, in fact?

The most important question, however, is whether, to paraphrase Foucault, this is really the best place to start staying close to the questions of the sciences.

In fact, it isn't. As the title of this chapter indicates, the better (but by no means perfect or essential) place to start is with *experimenting*, which, for the moment, we take as the emblematic activity of the sciences. Starting an inquiry into the sciences with a focus on experimenting immediately puts you in the muddle of things, where people build, write, question and requestion, blunder and triumph, are surprised and disappointed. Experimenting is a fantastic, puzzling, productive, messy complex of practices—including the practice of theorizing. Experimenting involves encounters with a . . . world. (Later in this chapter, we'll introduce what we think is a better term—*reality*—but "world" will have to do for now.) Such encounters are indeed forceful, but hardly characterized by slapstick violence.

Still, "the fact" exerts a gravitational force that seems to draw all discussions of the sciences inexorably toward it. Conventional views of the sciences are all about grounding, and facts are the solid ground on which the building process starts. (That's why the foot and the ground, and all that kicking and stomping and falling, keep turning up in those stories defending realism's honor and virtue.) You have to start with the facts, and *only* with the facts. And so, as much as we would like to start this book elsewhere, we can't.

Earlier in this century, the dream of those philosophers and scientists associated with the Vienna Circle and its various brands of logical positivism, was that the sciences would be firmly grounded in these brute facts, once one delineated the strict rules by which facts could be built up into theory. Facts were to be the fundamental building blocks of the sciences, the only solid foundation on which to build knowledge and society. Despite decades of critique, this remains a widely accepted and widely deployed picture of science: Scientists make observations of an unchanging world, develop hypotheses on the basis of those observations, submit these to logical and empirical testing, to finally arrive at a mirroring, a literal re-presentation of the world.

But all that is so ideal as to be dull, and far too general and vague. No scientist actually works that way, even if they write textbooks or popular articles that say they do. It's much better to look at specific matters of fact in specific cases, to see what they do and how they do it. We now have ample evidence that the distinction between what the world is in fact, and our theoretical representations of it, is a quite muddled affair indeed. But before someone starts kicking a stone or thumping a table, the point of muddling this distinction is not to deny that something called "the real world" or "nature" matters, or that we are free to choose between relativized representations, none truer than any other. That also would be far too general and vague. The histories of the sciences, and their continual daily practice, are chock-full of unexpected encounters, strange new results, and daily confrontation with a material world that continually surprises and forces inquiry to begin again.

For the time being, we'll accept the conventional conception of facts as the solid basis that assists in the scientific project of representing a world. It's not that facts as building blocks aren't capable of providing solidity; they can provide a solid basis for scientific work, but always as part of an architectural strategy, and after a lot of skilled and sometimes not-so-skilled work. The reconstruction of our representations of the world is a neverending, unavoidable process. Looking at the interplay between solid facts and shifting strategies of representation allows us to ask a different set of questions: What muddled world-representation structures are nevertheless stable—at least temporarily, and for specific purposes? What makes them stable, or unstable? What will further experimenting accomplish?

Our views are close to those developed as part of the pragmatist tradition in philosophy, often distilled down to the "Big Three" figures of Charles Sanders Peirce, William James, and John Dewey. That tradition worked in a middle space between idealism and realism, trying to avoid the simple collapses to which philosophy was so prone, everything getting reduced to either a subject or an object, a foot or a stone. Words Dewey wrote in 1916, to introduce his provocatively titled *Essays in Experimental Logic*, could just as well describe our position:

> The position taken in these essays is frankly realistic in acknowledging that certain brute existences, detected or laid bare by thinking but in no way constituted out of thought or any mental process, set every problem for reflection and hence serve to test its otherwise merely speculative results. It is simply insisted that as a matter of fact these brute existences are equivalent neither to the objective content of the situations, technological or artistic or social, in which thinking originates, nor to the things to be known—of the objects of knowledge.[1]

Writing in an era of rapid industrialization, Dewey and other pragmatists often employed metaphors of production to make the conceptual distinctions they thought important. Dewey went on in this essay to compare brute existences to mineral rock or raw ore "in its undisturbed place in nature," a "brute datum" to "the metal undergoing extraction from raw ore for the sake of being wrought into a useful thing," and the object(s) of knowledge to the final "manufactured object." It is a scheme that depends not on two opposing terms (fact/theory, object/subject) but on *three* terms, with "brute datum" in the middle, between nature and human commerce. Existence, purpose, and knowledge would be another way to express this indissoluble triad. The most important characteristic of Dewey's kind of fact is that it was made (but not made up) to continue the productive process.

Simplistic charges of subjectivism were to Dewey a "depressing revelation" of the traps people were prone to falling into when talking about knowledge—idiotic traps involving stepping out of airplanes and such. "To stumble on a stone need not be a process of knowledge; to hit it with a hammer, to pour acid on it, to put pieces in the crucible, to subject things to heat and pressure to see if a similar stone can be made, *are* processes of knowledge."[2] The sciences, as a form of inquiry, depend on the third, middle term highlighted by Dewey. They also represent a kind of third term themselves: the sciences are neither subjective musings circling inside one's head, nor random collisions with a brutal world, but something in the middle—something that always involved *signs*:

> In every case, it is a matter of fixing some given physical existence as a sign of some other existences not given in the same way as is that which serves as a sign. These words of Mill might well be made the motto of every logic: "To draw inferences has been said to be the great business of life. Everyone has daily, hourly, and momentary need of ascertaining facts which he has not directly observed. . . . It is the only occupation in which the mind never ceases to be engaged." Such being the case, the indispensable condition of doing the business well is the careful determination of the sign-force of specific things in experience. And this condition can never be fulfilled as long as the thing is presented to us, so to say, in bulk.[3]

What follows in this chapter are some stories in which facts figure centrally, but not in bulk. Facts are not something from the bulk bins of raw nature, but the carefully packaged items neatly displayed on the supermarket shelves of the sciences. The following are stories *about* facts, less in the sense of giving direct definitions to this term, and more in the sense of trying to show what goes on about and around facts. They should illustrate how, if you consider facts not as inert things but as a "sign-force," you find yourself not just bumping into them, but being pushed or pulled in certain directions. Like one magnet approaching another, approaching the topic of fact can involve overwhelming attraction or repulsion, sudden reversals of polarity which turn things upside down or inside out, and the seemingly magical ordering of random filings from a far-flung territory into patterns showing the invisible lines of force.

Getting Centered

The Copernican Revolution is the paradigmatic example of a scientific revolution, when the old ways of seeing, knowing, and doing things with the natural world suddenly—or not so suddenly, as the case may be—change into new perceptions, conceptions, and activities. The Copernican Revolution, the story goes, established one of the most basic, entrenched, and universally accepted facts that has become practically a test of sanity: Everyone knows that the earth goes around the sun; you'd have to be crazy, or maybe just a skepticism-infected postmodern social constructionist, to think otherwise.

Since no event more marks Europe's emergence from the Dark Ages, when we are in effect told that religion enforced insanity, it's worthwhile looking closely at who Copernicus was, what he did and didn't do, and how exactly he did it.

Let's quickly get some standard misconceptions out of the way—misconceptions that nevertheless carry a lot of weight within scientific communities and the general public today. After a lot of work by a lot of historians and philosophers, dedicated to describing how and why this scientific revolution turned out the way it did, there is little doubt that:

1. Copernicus's heliocentric system was not *simpler* than the earth-centered, geocentric system inherited from Ptolemy; it was full of baroque mechanisms, some of which were more contrived and complicated than Ptolemy's. (Technically, it wasn't even heliocentric—Copernicus had to put the center of the universe at an abstract point *near* the center of the sun.)
2. For a long time, the Copernican system was no better at *predicting* astronomical events than the Ptolemaic.
3. Copernicus didn't build his system up *inductively* from the "facts" of observation; as Johannes Kepler would point out a bit later, both Copernican and Ptolemaic systems at times contradicted observations (which were not altogether reliable to begin with).
4. The Ptolemaic system was not *falsified*, i.e., demonstrated to be wrong, while the Copernican theory continually held up under trial.[4] Here things become a bit more complicated, but it seems clear that even if such disproof is considered essential, that kind of falsification didn't really occur until the early nineteenth century (with the introduction of stellar parallax observations in 1838). In which case you have a very long, and very muddy, revolution—not to mention a lot of explaining to do as to why all of the great scientists until then were totally, "rationally" committed to heliocentrism.[5]

These criteria of simplicity, increased capacity for prediction, reliance on induction rather than metaphysical hypotheses, and withstanding tests to prove it false, were for much of the twentieth century thought to be the hallmarks of rational progress in scientific theories. Little wonder, then, that when historians and philosophers showed that Copernicus and his heliocentric system didn't exhibit these characteristics, Thomas Kuhn would write in 1959 that "to astronomers the initial choice between Copernicus's system and Ptolemy's could only be a matter of taste, and matters of taste are the most difficult of all to define or debate."

But you needn't be a zealous science purist to feel that this phrase "matter of taste" could use a little more precision or specificity. Since Kuhn, historians and philosophers of science have become more adept at defining and debating what "matters of taste" are and how they operate. And by looking at some of their work, we can better understand how those matters of taste meld with matters of fact.

It's not that Copernican scholars have stopped looking at what historians of science sometimes call the "internal" complexities and achievements of sixteenth-century astronomy. (Particularly in the 1960s and 1970s, historians of science thought it was their job to distinguish between "internal" factors [mathematics, logic, etc.—the "real stuff"] that pushed the sciences to progress, and the "external" factors [cultural beliefs, social institutions, etc.] which could only hold science back or distort it. Now many of them know that the most interesting things happen in between these categories.) The conventionally scientific part of Copernicanism is still a salutary and inexhaustible topic, and you can find library shelves packed with volumes of articles crammed with detailed accounts of Copernicus's observational data, intricate geometric demonstrations, and other technical issues considered internal to the sciences, and exclusively constitutive of them. This aspect of the Copernican achievement remains compelling to historians and astronomers alike.

But not quite compelling enough. *Why* and *how* the Copernican Revolution happened, and why it happened so slowly as to make "revolution" a misnomer, remain nagging questions for many. Simple stories have an easy appeal—and the story, "the revolution happened because Copernicus got the facts right," is as simple as they come. But to really understand such a profound scientific and cultural change requires more than attention to the force of logic and factual evidence alone.

To begin complicating this story, we could ask whether Copernicus *really* believed that the sun was at (or near) the center of the universe, or just thought that this was a convenient and satisfying model. Moreover, whatever Copernicus really believed in the privacy of his own brain, how did his book instruct readers, then and now, to think about his system?

Copernicus's *De revolutionibus* was prefaced by a letter from his editor, Andreas Osiander, a letter that has generated four hundred years of controversy and much ill will toward editors in general. Osiander has been called everything from stupid to unethical, from an obstructionist and obsequious theologian and an enemy of science to a daring religious and scientific heretic. What has been at issue is the view which he put at the front of Copernicus's book, that "it is quite clear that the cause of the apparent unequal motions are completely and simply unknown to this art [astronomy]. And if any causes are devised by the imagination, as indeed very many are, they are not put forward to convince anyone that they are true, but merely to provide a correct basis for calculations." The insertion was anonymous, and thus ambiguous to readers.

Did Copernicus himself subscribe to such a view, that his system was just a better model for "saving the appearances" and possibly improving the calendar, but did not describe how the universe really was constructed? Almost certainly not. But such a view—that philosophy (and natural philosophy) could provide plausible, probable explanatory models but never the ultimate truth of God's causal mechanisms—was not unusual in either astronomy or philosophy more generally in this period. (This conceptual image of a nature which exceeds our capacity to represent it perfectly is one which will come up a number of times in the following pages, and we will be taking it quite seriously later.) Most scholars agree that in addition to misrepresenting Copernicus's actual convictions, Osiander's anonymous introductory letter served a strategic social function: it gave some protection to Copernicus's work in a time of profound religious and political upheaval. If this apparently anonymous introduction had not been included, all that was genuinely new and useful in Copernicus's book might have been almost entirely overlooked or immediately dismissed as simple heresy. The dissembling introduction at least forestalled and ameliorated that fate.

And in fact, for decades after its publication, most astronomers and other scholars used *De revolutionibus* for primarily the reason Osiander suggested: to make better calculations. The astronomer Praetorius used it to improve the Ptolemaic, earth-centered system, and did so quite effectively. The Danish nobleman-astronomer Tycho Brahe employed it similarly, devising a hybrid system which made excellent predictions, in which the earth remained motionless at the center of the universe where it had always been, with the sun going around it, while all the planets wheeled around the sun.[6]

While not exactly in the Middle Ages, Copernicus is nevertheless best thought of as "in the middle"—the middle between the Middle Ages and modernity. His attachment to the circle as the most virtuous of the geometric forms, a heavy hangover from the ancient Greeks, made him cling to baroque mechanisms like the orbit-on-orbit epicycle, the deferent, and the eccentric, and even more baroque combinations of these. Such devices made his system seem physically absurd to contemporaries not only in the Vatican, but in the scientific community as well. He held on tightly to the idea of crystalline heavenly spheres. He worked with old beliefs, sketchy data, and "irrational" commitments to both old and new disciplines of knowledge—and he did great work.

We will come back to Copernicus and his middled position in a later chapter. For now, we only wanted to show the dependence of a fact on a larger framework. To say: "It's a matter of fact that the earth goes around the sun," actually excludes the crucial parenthetical remark: "(Within the Copernican system of calculation, observation, and theorization) it's a matter of fact that the earth goes around the sun." This fact is no longer a fact within the modern theory of General Relativity, which allows us to specify *any* frame of reference for centering our factual measurements: earth, sun, center of the galaxy, or any arbitrary point in space. Within the current Einsteinian system of General Relativity, it's a matter of fact that the movements of earth and sun correspond to the curvatures of a four-dimensional construct called spacetime.

Nevertheless, there are stable relationships between the movements and observed positions of the planets, the sun, and surrounding stars—a "brute existence" or, in another of Dewey's terms, "original *res* of experience." But those relationships aren't at all meaningful or purposeful, and thus for Dewey—and for us—do not make a fact. They still must be refined into a "sign-force," which is what the Ptolemaic, Copernican, Tychoan, or Einsteinian system does. Within any of those systems, what was a jumble of observations and relationships gets packaged into a "useful thing." With it, you can build a Global Positioning System of satellites as an aid to navigation—a thoroughly Ptolemaic technology. You can predict the appearances and even the impacts of comets—the latter on other planets like Jupiter, we hope. Useful, reliable, even "mechanical" relationships and interactions? Definitely. Universal truth? An unnecessary and immodest, albeit reassuring, metaphysical claim.

Thus, ironically, the people and computers in the NASA control rooms worked within an earth-centered Tychoan system to launch inhabitants of the twentieth century to the moon, the first of the heavenly spheres. You could see this briefly in the hit movie *Apollo 13*, where the historically obsolete but pragmatically efficient view of the earth-as-calculating-center-of-the-cosmos flashed briefly across the screen. But such muddling within an outmoded theoretical framework was largely behind the scenes, while the main narrative centered on a different brand of muddling through. The earth-bound engineers and space-cast astronauts together did some quick-and-dirty calculations, and hastily rigged together components intended for other jobs into an air purifier that would get them home again. Such "kludge jobs" (see Chapter 2), whether cosmological or air-cleaning systems, can be remarkably effective, flexible, and heroic, even in the face of disaster.

Merging Beliefs and Facts to Make Experimental Science

You might think that, given a corpus of alchemical investigations and writings that was far more extensive than his famous work in natural philosophy and mathematics, we would present Isaac Newton as a study in muddling contradictions.

Instead, we take a brief look at the lesser-known but nevertheless important figure of Robert Boyle. Boyle's work in natural philosophy was of the kind we would now anachronistically refer to as "chemistry," the less glamorous status of which might go some way toward explaining why fewer people would recognize his name than would recognize Newton's. (Boyle's Law states that the volume of a gas increases proportionately as its pressure decreases at constant temperature. Most often written as part of the "gas law," pV = nRT, it describes the reliable relationship between the pressure (p), volume (V), and temperature (T) of a specific amount (n moles) of gas.) But Boyle, as one of the key figures in the founding of experimental science and of one of the earliest scientific institutions, England's Royal Society, deserves at least as much attention as Newton.

With *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, the historians of science Steven Shapin and Simon Schaffer have produced one of the most empirically and thematically rich and suggestive works on the origins of modern experimental science in seventeenth-century England. We can't consider all of their themes here in the detail they deserve—how science was both entertaining spectacle as well as an engine for facts; how experiments had to be publicly witnessed, and how new literary devices and genres for such witnessing were instituted (and still operate powerfully today); how the laboratory became a separate and privileged social space; how the problem of replication haunted all enactments of experiment; and many others. What we focus on here is their explication of how facts became centrally important to science, and how objectivity and truth came to be defined in terms of these facts.

Until the middle of the seventeenth century, "knowledge" and "science" were kept markedly distinct from matters of "opinion." The former adhered to the absolute certainty of "demonstrative sciences" like logic and geometry. Natural philosophers—whom it wouldn't be terribly wrong to think of as physical scientists—emulated these demonstrative sciences, and so produced "the kind of certainty that compelled absolute assent." But over the second half of the century, experimentalists like Robert Boyle and others associated with the newly established Royal Society came to redefine our expectations of science in terms of what was *probably* (that middling, gray area between knowledge and opinion) the case about nature. "Physical hypotheses were provisional and revisable; assent to them was not obligatory, as it was to mathematical demonstrations; and physical science was, to varying degrees, removed from the realm of the demonstrative. The probabilistic conception of physical knowledge was not regarded by its proponents as a regrettable retreat from more ambitious goals; it was celebrated as a wise rejection of a failed project. By the adoption of a probabilistic view of knowledge one could attain to an *appropriate* certainty and aim to secure *legitimate* assent to knowledge claims."[7]

It was the "matter of fact" that would undergird this new project of probable hypotheses, because the fact was what could provide the greatest degree of assurance, or moral certainty. Like Copernicus—or at least like Copernicus's preface-writer and, as we'll see in a later chapter, like Galileo—Boyle and the English experimentalists believed that God could produce similar effects through a variety of mechanisms.

How to get to a "matter of fact," then? According to Boyle, by aggregating individual *beliefs*. Here is an interesting muddle at the origins of experimental science: Solid, foundational matters of fact had themselves to be founded on the somewhat shakier matter of belief. To be more precise: The beliefs to be aggregated into facts would be those of individuals who could be trusted, whose eyes had been properly trained, and who would report faithfully on what they witnessed—in a word, gentlemen. Experimental science would have to be a noble pursuit.

(We're simplifying a bit here, and also excluding much interesting detail. One effect of the "gentlemen" argument at the time was to exclude those people, including women, associated with alchemy and other traditions of knowledge, keeping them out of the historical process of defining what the sciences would become. And things would get even more complicated later, as experimental science came to be celebrated as the best route out of the lower or middle classes, and into the more refined strata of society.)

Shapin and Schaffer point to what they call three "technologies" that allowed Boyle and others to produce matters of fact: a material technology of experimentation, in this case the air-pump; a literary technology that made "virtual witnessing" possible for a wider community that couldn't be squeezed into the Royal Society's demonstration chambers for every meeting; and a social technology that established certain conventions for how experimentalists should deal with each other and with each other's knowledge-claims. Each of these three technologies, or knowledge-producing tools, "embedded the others," and none could be said to be more fundamental than the others. The material technology of the air-pump required specific social organizations, such as the distinction between common mechanics, technicians, and demonstrators, and the gentlemen scientists, as well as the creation of a privileged laboratory space set apart from the rest of the world (social organizations which remain operative today). The literary technology of the new scientific report extolled the value of those social arrangements, embodied in its rhetoric the new social values of modesty and probabilism, and opened the new experimental findings to wider questioning or affirmation. Indeed, running the machine of the literary technology—i.e., reading—was in some sense equivalent to actually running the machinery of the experiment.

The air-pump, as historian of science A. Rupert Hall has put it, was "the cyclotron of the age." Relatively few of them existed, as they required the patronage of a state, royalty, or elite scientific society to be built. They were cranky, prone to malfunction and requiring constant tinkering and maintenance. This work was not always pretty and, as with all the sciences today, depended on a lot of craft knowledge and unseen, uncredited technicians. Although Boyle wanted a larger air-pump than the thirty-quart one he used, he reported that his nameless "glass-men" were at the limits of their abilities. As sealants and lubricants, Boyle had recipes (which he did not always write down) that included "sallad [sic] oil," "melted pitch, rosin, and wood-ashes," and for fixing small cracks, linen spread with a mixture of quicklime, cheese scrapings, and water ground to a paste "to have a strong and stinking smell."

The central problem against which all this effort was directed was the controversial Torricellian phenomenon. First performed in 1644, the phenomenon occurs when you take a long glass tube, closed at one end, and place it in a tub of mercury. The tube fills completely with mercury, and is then inverted so the open end remains in the tub and the closed end is at the top. You then see that the mercury no longer fills the tube, but leaves the top empty—or looking empty, anyway. We call it a barometer today, and regard it as a good measure of air pressure. For the seventeenth century, it might in retrospect be called a scandalometer, because it was a good measure of the scandalous variety of doctrines and opinions about what actually was, or what possibly could be, in that seemingly empty space at the top of the tube. Torricelli, Pascal, Roberval, and Descartes were just a few of the natural philosophers who weighed in on this controversy. The empty space at the top of the tube couldn't be isolated from other social and philosophical controversies; what was or wasn't at the top of the tube was a question inextricably bound up with disturbing differences between conservative Scholastics and radical empiricists, and even more disturbing associations among the concepts of "subtle matter" and spirit, and the practices of witchcraft. It was a seemingly empty space that sparked a seemingly endless series of weighty disputes.

Here's where Boyle thought his material-literary-social technology of experimental science could show its greatest value: it could stop, or at least sidestep, all these endless vituperative arguments that were so easily joined with religious, national, and political differences, and restrict the discussion (and therefore what would count as real knowledge) to matters of fact. In terms that we'll elaborate more on in Chapter 2: where others continually *articulated* the phenomenon at the top of the mercury column with other philosophical and religious beliefs or articulations, Boyle sought to disarticulate or disentangle questions and statements about nature from religious or political ones, and rearticulate those statements within the new material-literary-social technology of experimental science. Thus, in his writing Boyle decided to "speak so doubtingly, and use so often, *perhaps, it seems, it is not improbable,* and such other expressions, as argue a diffidence of the truth of the opinions I incline to, and that I should be so shy of laying down principles, and sometimes of so much as venturing at explications."[9] That decision led to an extremely powerful and persistent "literary technology": the often strained rhetoric of this contrived modesty (as well as what Boyle called his "prolixity") still characterizes scientific papers today.

This modesty was not a disadvantage or deferral, but an asset. Is there or isn't there a vacuum—whose side are you on?: Boyle would adroitly sidestep "so nice a question" and would not "dare to take upon me to determine so difficult a controversy." Doing so would "make the controversy about a vacuum rather a metaphysical, than a physiological question; which therefore we shall no longer debate here. . . ." As Shapin and Schaffer comment:

> The significance of this move must be stressed. Boyle was not "a vacuist" nor did he undertake his *New Experiments* to prove a vacuum. Neither was he "a plenist," and he mobilized powerful arguments against the mechanical and nonmechanical principles adduced by those who maintained that a vacuum was impossible. What he was endeavoring to create was a natural philosophical discourse in which such questions were inadmissible. The air-pump could not decide whether or not a "metaphysical" vacuum existed. This was not a failing of the pump; instead, it was one of its strengths. Experimental practices were to rule out of court those problems that bred dispute and divisiveness among philosophers, and they were to substitute those questions that could generate matters of fact upon which philosophers might agree.[9]

Unlike the alchemists, the experimentalists practiced their trade in public, in the assembly room of the Royal Society (although critics like Thomas Hobbes countered that the exclusive Royal Society hardly counted as "public"). There the experiments were witnessed and, in language drawn from the legal world, the results were *judged* by many observers of the trial. (We'll return to the inescapable inexactness of judging in the sciences in Chapter 4.) But Boyle, perhaps to maximize the number of people who could be recruited to the new experimentalist ways, showed a more inclusive attitude toward alchemy and the alchemists than did some of his contemporaries, such as Isaac Newton. Newton believed that alchemy and the new science should be kept as distinct practices and forms of knowledge, and so engaged in both while keeping his extensive alchemical pursuits secret. Newton criticized Boyle for simply publishing work that dealt (albeit critically) with the alchemical tradition. Working his new separation between matters of fact and the language of theory, Boyle's set of social conventions incorporated some (although not enormous) tolerance for social difference: "Let his opinions be never so false, his experiments being true, I am not obliged to believe the former, and am left at liberty to benefit myself by the latter."[10] Taken seriously, that's not only a laudable social convention for the sciences, but a productive one as well. It's an excellent legacy from this period which needs to be better preserved.

Boyle's sociotechnical innovation is easy to "naturalize": experimental practices were simply better at producing a better kind of knowledge, as subsequent history has shown. But as we'll see, just what "better" meant at that time, in that social context, was hotly debated. That's where the work of the new "social technology" was important, since it redefined conventions of proper discourse and what would count as knowledge. Boyle argued for and established new *social conventions* concerning how knowledge was or was not to be produced: disputes were to be about findings and not about the character of the investigator; ad hominem attacks were out. Indeed, the status of the individual investigator or philosopher, so prone to dogmatism or "enthusiasm," was supposed to shrink in comparison to the communal endeavor. And the community was obliged not to believe in each other's opinion, but only to assent to what they believed they saw in nature, apparent through the matters of fact produced by experiment.

Again, these are in many ways admirable ideals: public witnessing, the necessity of judging, and an agreement to at least *defer* social, political, and philosophical differences—to leave them aside and read only the natural world as it was presented through experiments. They can, with some cleaning up and rearticulation, be salvaged as goals which the sciences can and should pursue today.

But how was such a consensus, based only on witnessing, possible? Nature itself might be the basis for brute matters of fact, but did that mean that it could serve as the basis of knowledge? What makes some beliefs more believable than others? And just what does "judging" involve?

Richard Feynman's Half-Assed Atoms

After all that ancient history, it might be nice to shift registers to something and someone a little more recent, a little more familiar, and perhaps a little stranger at the same time.

Richard Feynman is surely one of the twentieth century's most famous physicists, and rightly so. From his contributions to the Manhattan Project (employing the ancient abacus for calculations of nuclear physics) to his work on the commission investigating the space shuttle *Challenger* disaster (running a tabletop experiment with ice water and O-ring in a congressional hearing room), he has worked unconventionally yet productively on science in the most public spheres. His invention of "Feynman diagrams" was enormously important to the theory and practice of particle physics. He wrote thoughtful and deservedly influential physics textbooks, and was also adept in the more popular vein of the *Surely You're Joking, Mr. Feynman!*, isn't-physics-amazing-and-beautiful-even-if-it-is-a-bit-strange genre. There's no question that in this culture he rates as a *Genius*, the word that science writer James Gleick picked for the title of his Feynman biography.

But when Feynman was not so concerned with tying up his own thinking and the wonders of physics into tidy publishable packages, did he see the world any differently? Did he think about his own thinking in physics as something that was even stranger than his books represented—stranger because less accessible, more muddled?

Interviewed by the physicist and historian of science Silvan S. Schweber, Feynman tried to articulate how he worked and thought through visualization. Reading this, we get some insight into how Feynman might have actually experienced the raw, brute world of things:

> I cannot explain what goes on in my mind clearly because I am actively confusing it and I cannot introspect and know what's happening. But visualization in some form or other is a vital part of my thinking and it isn't necessary I make a diagram like that. The diagram is really, in a certain sense, the picture that comes from trying to clarify visualization, which is a half-assed kind of vague [sic], mixed with symbols. It's very difficult to explain, because it is not clear. My atom, for example, when I think of an electron spin in an atom, I see an atom and I see a vector and a **Ψ** [psi] written somewhere, sort of, or mixed with it somehow, and an amplitude all mixed up with *x*s. It is impossible to differentiate the symbols from the thing; but it is very visual. It is hard to believe it, but I see these things not as mathematical expressions but a mixture of mathematical expression wrapped into and around, in a vague way, around the object. . . . What I really am trying to do is to bring birth and clarity, which is really a half-assedly thought out pictorial semi-vision thing. OK?[13]

Now you might say that we shouldn't take such statements very seriously; they're uttered in an interview, where the speaker doesn't have time to think carefully, or come up with the right words, let alone better syntax or grammar. But it's actually not far in spirit from what Feynman might have written in one of his books. Feynman's brilliance in physics is unquestioned. But you could also say that when asked where that brilliance stems from, or what goes into it, Feynman finds exactly the right words—a language of persistent muddling. He *really can't* "explain . . . clearly," he *really is* "actively confusing" his thought process, he *really does* rely on a "half-assed kind of vague" thing clumsily called visualization, before he sets down a diagram or equation on paper.

When Feynman is doing the work that made him famous, when he's doing physics, he is working with a world that is a mixture, a concatenation of mathematical symbols written over, or in, or *somewhere* around—*somehow*—an object like an atom. It is impossible to differentiate these things, extract the symbols from the things-in-themselves. Feynman's brain isn't capable of it, and neither are ours or anyone else's. It's hard to believe, because it so goes against our familiar way of thinking. You couldn't have asked Feynman, "Is electron spin really a fact?," because as soon as the words "electron spin" were out of your mouth, he'd already have had some "half-assed" but nevertheless quite powerful vision of a psi-function and all the attendant mathematics, together with an arrowlike angular momentum vector. Electron spin, or the fact that electrons have spin, doesn't exist (meaning, in the root sense of "exist," it doesn't *stand out*) without the supporting mathematics wrapped, vaguely, around it. Don't think of electron spin as a brute existence; think of it as a "sign-force." There's not first an atom, and then some idea of spin, and then the formulas for that spin-state: it's all at once, and it's not just in Feynman's head, it's out there, loose in the world. And it exists totally before, within, and constitutive of saying that electron spin is "real." He is—we are—it is—off and running, pushing and pulling, exerting peculiar sign-forces. The fact is always after the fact.

Sort of.

OK?

Charged Electron, Committed Physicist

Electron spin is one of those weird quantum properties, you might argue, but what about the charge of the electron? Surely there's nothing half-assed about that: reliably charged electrons ran through the computers that this manuscript was created on, powered the printing presses and other machines that produced this book, generated the light by which you're now reading it—and even helped to mediate the neural processes by which you gave some meaning to the preceding paragraph.

Another historical episode shows how a focus on experimenting can complicate even something as reliable as electron charge. It also starts to suggest how an emphasis on muddling actually forces us to new kinds of precision—both practically and conceptually, and in the pursuits of science and history. Eventually, we'll compare two well-scrutinized episodes from two different periods in the history of science, and two different disciplines. One story is about the Nobel prize–winning American physicist Robert Millikan's experimental establishment of the fundamental charge of the electron early in this century, and how that became a fact via what could be termed, ungenerously, as fudging the data. That's the story told here. The second story is a more recent controversy, and centers on the molecular biologist (and Nobel laureate) David Baltimore's involvement in a series of immunological experiments, which is elaborated in Chapter 4. Both cases involve the proper care and handling of "the facts;" both show how ambiguity presents opportunities as well as dangers, elicits and demands creativity even as it allows for selfdeception or fraud. In the end, precision and ambiguity end up in an uneasy coexistence, requiring a series of judgments.

Millikan's oil-drop experiment is famous within both science and the history of science, a fame secured in part by the analysis provided by the physicist and historian of physics Gerald Holton.[12] High-school students now try to replicate Millikan's efforts in their physics classes. What they learn is that even with much better equipment than Millikan had, and even knowing how the experiment is supposed to turn out (what the data are supposed to look like), it remains a maddeningly quirky, delicate, and difficult task.

The question facing Millikan and others in the opening decades of this century was whether the newest fundamental particle, the electron, had a precise unit of electric charge associated with it, or if the charge varied continuously from one electron to another. Other physicists had already established that the charge-to-mass ratio (e/m) of what they called "cathode rays" was constant, and that these cathode rays might be particles better called electrons. If the electrons all carried the same charge, they would also all have the same mass, and would qualify as "atoms" of electricity, in the original meaning of that word as an indivisible unit.

Working experimentally, Millikan set about his inquiries by constructing an apparatus consisting of two slightly separated, electrically charged horizontal plates enclosed in a box. He sprayed small droplets of oil between these plates, a process which gave most of the oil droplets some electric charge. By rapidly and carefully adjusting the voltage on the plates, while looking at the droplets through a small telescope, Millikan tried to get one droplet after another to hang perfectly suspended between the plates. A series of by no means trivial or easy observations, assumptions, and calculations allowed Millikan to judge the weight of each suspended droplet, and to then equate the force of gravity trying to pull it down with the electric force from the charged plates which kept it hanging, suspended in the telescope's sights. Knowing the voltage on the plates, knowing the weight of the drops, and knowing the force of gravity on them, Millikan could calculate the only remaining unknown in the equations: the charge on the droplet.

We write that Millikan was working experimentally, but that's not quite true. Yes, he had ingeniously dreamed up and built this apparatus, and disciplined his body to sit in front of it with one eye fixed on the telescope, hand twiddling the voltage dial and rapidly writing down observations. But if he had been working only experimentally, he would have ended up with what almost all the high school students today end up with: a data set that suggests that the charge on the oil droplets comes in precise units of one, two, three, or four electron charges, but doesn't quite *convince*, because the data set is muddled with observations and calculations that might also indicate a continuous range of charge values in between the neat set of integers, and especially some that seem to indicate a charge of one-half or one-third of the most common value.

Millikan was successful because he was working *theoretically* as well as experimentally. Millikan's notebooks and writings at the time reveal that he was *committed in advance* to the theory of unitary electron charge, and so knew which experimental outcomes to throw away. If he hadn't been so committed, he would have been awash in a sea of largely undifferentiated data (as was a competitor, the Austrian physicist Felix Ehrenhaft). Millikan was an excellent muddler. In the words of Peter Galison:

> Millikan supported atomic theory and in general a granular representation of nature. . . . By the strength of his convictions, he set aside some measurements not in accord with his atomistic hypothesis about electric charge. . . . [He] was faced with a choice where dogmatic pronouncements on "experimental method" would not help: to ignore all expectations leads to chaos, yet to adamantly follow prior beliefs can blind the experimenter to novelty.[13]

Millikan violated the prime directive that scientists are supposed to observe, harboring an irrational (in the sense of being before the fact) commitment to a certain theory. And his violation of that conventional expectation became the path to the fact. A kind of feeling for the apparatus was also part of doing good science for Millikan. He had an excellent sense of when the equipment he had built was working well and when it wasn't, when the complex and unquantifiable environmental conditions in the room were conducive to a good experimental run and when they weren't, when he had made good observations and when he hadn't. Being a good, precise experimentalist requires a much more imprecise ability, that of judging. (But when we turn to questions of judging and the case of David Baltimore in Chapter 4, we'll see that theoretical commitments in the face of experimental ambiguity, and a "feeling for the apparatus," don't always work out so well.)

Still, you might object, even if all that is true and Millikan didn't fit the conventional model of the orthodox scientist, he was able to do what he did because, in the end, nature *really* is that way, and the electron *really* does have a unitary charge of 1.602×10^{-19} coulombs. True—or shall we say, true enough. Thousands of physicists, electrical engineers, chemists, and other scientists make good use of that very stable fact every day. But not one of them could tell you what "charge" *really* is in the first place, what the name refers to—at least not without referring to an enormous series of other concepts, measuring devices, experimental effects, and disciplined procedures. As the eminent experimental British physicist who pioneered the study of electricity, Michael Faraday, wrote in 1839 about terms such as "ion," "electrode," "cathode" and others that he invented: "These terms being once well defined will, I hope, in their use enable me to avoid much periphrasis and ambiguity of expression. I do not mean to press them into service more frequently than required, for I am fully aware that names are one thing and science another."[14] Names and sciences don't comprise such easily separated spheres, but it is still good to be reminded that "the charge of the electron" is a shorthand term that helps us avoid the long, ambling, periphrastic description of a finely crafted network of technologies, techniques, equations, social infrastructure, and other names that together allow something called "the charge of the electron" to be produced—and produced reliably. If you really want to know why the charge of the electron is really 1.602×10^{-19} coulombs, you'd want to know the incredibly periphrastic history of electromagnetic theory and experiments, coupled with the technological and social history of electrical technologies.

Faraday, and later his compatriot James Clerk Maxwell, did brilliant theoretical and experimental work in electricity and electromagnetic theory. We tend to forget, and our science textbooks rarely reflect the fact, that they did this work in quite a different scientific culture from our own. One quality of that culture is hinted at in Faraday's statement above: these British physicists (the German physicists like Hertz and Helmholtz are quite another story) were always aware of that persistent gap between names and sciences, between our representations of a world and the world itself. As our earlier stories have suggested, this kind of modesty is an enduring philosophical strand in the sciences, and one well worth preserving. It's a modesty that redirects our attention to the middle space, to the terms and technologies that we put into that gap, stopgaps "pressed into service" to better manage both our ideas and our world.

But by the end of this chapter, we might say something else—something that tries to get around that most useful, natural, and totally outdated and inadequate stopgap phrase: "the world itself."

Ludwik Fleck and the Signal that Resists

Ludwik Fleck conducted noteworthy and sometimes remarkable work in the fields of bacteriology, serology, medical practice, and sociology and philosophy of science. Fleck was a man at a number of crossroads, subjected to a variety of forced migrations, and dedicated to scientific work in what were frequently less than ideal circumstances.

Fleck was born, educated, and began his biomedical career in Lvov, Poland. Beginning in 1928, he headed the bacteriological laboratory of the Social Sick Fund in Lvov, until anti-Jewish measures forced his dismissal in 1935. He continued his medical and research practice in private, where in addition to working on streptococci he devised a method to improve the Wassermann test for syphilis. When the Soviets took over Lvov in 1939, he became director of the City Microbiological Laboratory, head of the microbiology department of the State Bacteriological Institute, and held a number of other positions as well. Then when the Germans occupied Lvov in 1941, Fleck became director of the bacteriological laboratory of the Jewish Hospital. Prompted by the severe typhus epidemic that gripped the Jewish ghetto, Fleck muddled through with "primitive means" to use the urine of typhus patients in rapid diagnostic and vaccination procedures. His methods were so effective, in fact, that when the Germans arrested him in 1942, he was forced to put his skills to work producing the vaccine for German troops.

Defiant truth-telling was not the most effective survival strategy at the time. Asked by the Germans if his vaccine would be suitable for Aryans, Fleck responded: "Of course, provided that the vaccine is prepared from the urine of Aryans and not of Jews." Fleck and his staff were taken first to Auschwitz, where they were attached to the camp's hospital and forced to produce vaccine; Fleck was later taken to Buchenwald, where he continued this work. The Nazis killed Fleck's two sisters and their families in Poland, before he was freed when the U.S. army entered Buchenwald in April 1945. He resumed his scientific work in Lublin, holding a number of prestigious positions and receiving numerous awards, including election to the Polish Academy of Science in 1954. Fleck died in 1961, in Israel.

In addition to this extraordinary life and career, another of Fleck's enduring legacies is his book, *The Genesis and Development of a Scientific Fact*. Published in Switzerland in 1935 (as a Jew, he couldn't publish in Germany), the extent to which this book was read and discussed in Europe is only now beginning to become clear. Its greatest acknowledged effect would seem to come much later: at the very least, Fleck's book reassured Thomas Kuhn that many of his own ideas about paradigms and revolutions in the sciences were not crazy.

The book's seemingly paradoxical title struck Kuhn when, as he reports, Harvard president and (briefly) U.S. High Commissioner to Germany James Conant related "with glee" the remarks of a German associate: "How can such a book be? A fact is a fact. It has neither genesis nor development."[11] But the book in fact is, and it details the historical shifts in what was believed to really cause syphilis, and how that condition came to be diagnosed and defined.

Besides his work on syphilis (which is best read in his own words), another, briefer episode related in Fleck's book also conveys his views on scientific research, experimentation, and the status of "facts."

Fleck relates a story about how he and an assistant grew a streptococcus from the urine of a female patient. "Its unusually rapid and profuse growth attracted our attention, as did pigment formation, which is very rare with streptococci."

Fleck had never seen such colors in bacteria and "remembered only vaguely having read about them." Wanting to find out more about these germs, he planned a series of experiments—and then didn't do them. Instead—as he recalled and recounted after the fact—"the project turned largely into a study of variability." Such a thing undoubtedly happens all the time in science: An investigator starts out interested in one set of questions and phenomena, and ends up doing something completely different. But what doesn't usually happen is that an investigator asks the crucial reflexive question that Fleck then asked: How could this have happened?

Because he inquired about his own process of inquiry, wondering how it came to be that he did something he didn't plan to do, Fleck becomes one of our archetypal muddling, reflexive, admirable scientists. He had been studying the literature on variability in bacteria, and how they were classified according to various species, and because he knew that Streptococcus often reminded laboratory scientists of Staphylococcus, he also remembered something about staph colonies having different colors. So he suggested to his colleague that she determine if their strep colonies could be split into lighter and denser colonies. They could: There were a large number of "ordinary yellowish, transparent colonies," and there were a very few "small, white, and more opaque ones."

Now they really began trying to patch things and explanations together, unsure whether they could "even claim with any certainty and assurance that a real problem existed at all." Those first differences that they had noticed—between yellowish and white bacterial colonies—"all became unstable in subsequent generations." But that became a new difference to work with—the difference between the offspring of the few special colonies and that of the majority. That difference even seemed to increase as they kept creating new generations of the germs, which Fleck attributed to the "partly subconscious selection of the most divergent colonies." But again, "all attempts to formulate this difference had to be dropped right after the next inoculations. . . ." They continued working.

At this point, many scientists would have reconstructed their narrative into the "Eureka!" genre, where the truth suddenly becomes miraculously and immediately clear. But Fleck saw it differently: ". . . at last, after we had gained comprehensive experience, a formulation crystallized." The emphasis here is on experience rather than the flash of intuitive confirmation—the vague, "partly subconscious" yet powerful cumulative effects of habit, work, acquaintance via immersion, and half-remembered scientific literature. Of course, sudden insight is often an important characteristic of the sciences, but the frequent emphasis on insight in the sciences can easily lapse into a kind of mysticism.

For Fleck, the truth doesn't appear, the mind of the deity isn't revealed, the scientist's mind doesn't seize the wondrous fact, but a *formulation crystallizes* out of this viscous gel of experience. It wasn't the difference in color that was important, as they had first thought. It wasn't the difference in intensity of color in subsequent generations that was important. It now appeared that all the colonies had the same color, but differed in structure. And unlike the color, those differential structures—later called the "smooth" (type G) and the "curly" (type L)—"could be perpetuated through transfers." They had—discovered? made? crystallized?—a fact.

Fleck explained why he thought it important to subject the reader to this kind of account (which we have simplified immensely), one that followed the vicissitudes of the actual experimental encounters:

> It shows (1) the material offering itself by accident; (2) the psychological mood determining the direction of the investigation; (3) the associations motivated by collective psychology, that is, professional habits; (4) the irreproducible "initial" observation, which cannot be clearly seen in retrospect, constituting *a chaos*; (5) the slow and laborious revelation and awareness of "what one actually sees" or *the gaining of experience*; (6) that what has been revealed and concisely summarized in a scientific statement is an artificial structure, related but only genetically so, both to the original intention and to the substance of the "first" observation. The original observation need not even belong to the same class as that of the facts it led toward.[16]

Fleck tried to anticipate how many "theoreticians," trying to get this complex mess of process tamed into the usual ordered accounts of science, might come up with alternative formulations. They could say that Fleck and his colleague's initial observation, "devoid of any assumptions," was something like "Today one hundred large, yellowish, transparent and two smaller, lighter, more opaque colonies have appeared on the agar plate." But that seemingly raw statement of brute facts, Fleck points out, "anticipates a difference between the colonies, which could actually be established only at a later stage of a long series of experiments." Recall that the first observation was colors, shortly after muddled with the memory of some literature on color in staph colonies. All right, the theoreticians might respond: you would have to investigate all the colonies, and all their properties, and then you could see how there were one hundred of this first sort and two of the second sort. Well, writes Fleck, "it's altogether pointless to speak of *all* the characteristics" of something like bacteria, because you always begin from some combination of habit, professional experience, or some motivation that's not entirely conscious. A good scientist always begins, at least in some measure, after the fact. Why delude yourself into thinking the process is or should be random, arbitrary, or completely inclusive? "Observation without assumption," writes Fleck, "which psychologically is nonsense and logically a game, can therefore be dismissed."

> But two types of observation, with variations along a transitional scale, appear definitely worth investigating: (1) *the vague initial visual perception*, and (2) *the developed direct visual perception of a form*. . . .
>
> Direct perception of form requires being experienced in the relevant field of thought. The ability directly to perceive meaning, form, and self-contained unity is acquired only after much experience, perhaps with preliminary training. At the same time, of course, we lose the ability to see something that contradicts this form. . . . Visual perception of form therefore becomes a definite function of thought style. The concept of being experienced, with its hidden irrationality, acquires fundamental epistemological importance. . . .
>
> By contrast, the vague, initial visual perception is unstyled. Confused partial themes in various styles are chaotically thrown together. Contradictory moods have a random influence upon undirected vision. . . . Nothing is factual or fixed. Things can be seen almost arbitrarily in this light or that. There is neither support, nor constraint, nor resistance and there is no "firm ground of facts."[13]

This beautiful passage is well worth lingering over. Unstyled, undirected, or purposeless perception—the kind of raw empiricism that we saw Boyle apparently aiming for—can't provide facts or solidity. Meaning requires discipline; all perception and thought, in order for it to count as science, has to be styled. And that means living with contradictions and trade-offs. Direct perception of form or meaning is only possible through the indirection of being trained in a "thought-style." And while you gain enormously from being so styled—indeed, it's a prerequisite for doing science—you lose at least some of your ability to see other, new things. The sciences drive a hard bargain.

Fleck was a scientist who knew that conventional ideas about facts and what happened in the processes called experimenting had to be reworked, rethought, and reinvented. And like scientists who continually invent new instruments and procedures, and then experiment with them, the analyst of the sciences has to invent new concepts like "thought-style" and "thought-collective"—and then see if they work or not, and for what ends. Fleck invented "thought-style" to draw our attention away from the nonsensical idea of observation without assumption, and the associated idea of brute facts drawn from an independent reality, and focus that attention instead on what some scholars now call the elements and processes of social construction: how habits of thought, long-standing disciplinary and cultural assumptions, personal history, professional education and training, and even moods and chance all work together to stylize perception and theorizing.

A thought-style is similar to what Kuhn would later call a paradigm; both terms have been ungenerously equated with "fashion." That's simply a very bad translation (often deliberately so). You don't change thought-styles like an old shirt or skirt; they are embedded, as we'll see in more detail over the next few chapters, in words, practices, funding patterns, our science curricula, and in many other things and relationships. They change slowly, and not simply through individual will or commercial promotion. Thought-styles (and there are usually more than one operating within a given field of science at any one time) are created and transmitted by thought-collectives, which isn't as totalitarian as it might sound. Narrowly construed, Fleck used thought-collective to refer to the scientific subcommunity that developed and tested new ideas, trained its newer members to see certain forms and ask certain types of questions, and acted in a variety of other ways to control the arbitrariness and chaos of unstylized reality.

Fleck, ruminating on his experience as a creative, successful muddler in the sciences, gives future scientists a very valuable gift. He gives to them, and to the rest of us as well, some training and stylizations to help us perceive the sciences and give meaning to them in a new way. He gives us a way to stop thinking about facts themselves, and to start thinking about "how a *fact* arises. *At first there is a signal of resistance in the chaotic initial thinking, then a definite thought constraint, and finally a form to be directly perceived.* A fact always occurs in the context of a history of thought and is always the result of a definite thought style. . . . The *fact* thus represents a *stylized signal of resistance in thinking*. Because the thought style is carried by the thought collective, this 'fact' can be designated in brief as the *signal of resistance by the thought collective*. . . . The fact thus defined as a 'signal of resistance by the thought collective' contains the entire scale of possible kinds of ascertainment, from a child's cry of pain after he has bumped into something hard, to a sick person's hallucinations, to the complex system of science."[16]

While it might seem that Fleck was something of an idealist, locating that resistance in thinking itself, a careful reading of that passage just cited shows that to be a poor interpretation. Fleck rearticulates the fact as "the signal of resistance by the thought collective." By mentioning the child's cry he connects the idea of a scientific thought-collective back to the naive realist's cartoon violence of feet, stones, and bodies falling out of airplanes. He even shows how preverbal utterances, truly brute cries of pain, have to be seen in social context: the cry is a signal to parents!

Scientists meet all kinds of resistance; they bump up against things all the time. That's what makes the sciences challenging, exhilarating, creative, and useful. But it's a mistake to think of reality, the material world, as either the first or only signals of resistance. Fleck and numerous scholars since him help us see how habit, tools, moods, money, professional training, metaphors, and other things that aggregate into a thought-style and thought-collective (fairly imprecise terms which we will try to specify in more detail later) have what we could call very *material* effects.[17]

When he says that there is no "firm ground of facts," Fleck isn't pulling the rug from under our feet. He's redirecting our attention again, because we've been looking for solidity in the wrong place—or rather, we've been looking for it in just *one* place: the facts. The solidity of facts, like the truth of scientific theories that we will be discussing in more detail over the next few chapters, isn't simply a consequence of their being "grounded in reality." Fleck directs our attention to the "signal that resists," a signal (or Dewey's sign-force) that comes not just from nature or reality, but from . . . where?

The short, imprecise answer is: from all over the place. A better answer comes in the next, final section of this chapter: from realitty.

Experimenting with Some Physicists . . . and Realitty

Later in the book we undertake a more extended exposition of some history of and current research in quantum physics. But it would be rather strange not to mention something about quantum physics in a chapter devoted to experimenting, since this particular branch of physics is most responsible for blurring the boundaries between experimental instruments, the acts of measuring and observing, the spinning of theory, and perhaps even the line between mind and reality.

Ever since the physicist Max Planck introduced the concept of the quantum into physics in 1900, these once distinct and stable categories have destabilized more and more. Planck (and many other physicists since) felt that the trembling extended beyond the boundaries of physics itself, and took on vast cultural significance. "We are living in a very singular moment of history," Planck wrote in his 1932 popular book, *Where Is Science Going?*—"a moment of crisis" that showed itself "not only in the actual state of public affairs but also in the general attitude towards fundamental values in personal and social life." There was a vague but growing unease in the world that Planck associated with "skeptical attack," a spirit that might have been appropriate to religion's "doctrinal and moral" systems, and even to art, but now the skeptic had "invaded the temple of science." Even within physics, the holiest of holy domains, "the spirit of confusion and contradiction has begun to be active." Established principles of causality, the independence of external reality from measurement and thought, were under suspicion from within, from physicists themselves.

Indeed, Planck was in no small part responsible for this situation. His invention in 1900 of the quantization of energy as a rather desperate means of reconciling disagreeable implications of classical theory with some reliable data on thermal radiation was the small event that snowballed into the quantum revolution that now disturbed him.

Where some believed this might mark the beginning of a "great renaissance," Planck's sympathies were more in line with those who saw the "tidings of a downfall to which our civilization is fatally destined." The "logical coherence" of science had once served as protection against this skeptical "contagion." Now even great scientists like Planck knew that it could no longer serve as the "firm foundation," the "rock of truth" on which we could "take our stand and feel sure that it is unassailable and that it will hold firm against the storm of skepticism raging around it."[20]

That's a rather fearsome, and fearful, vision, and one that is still felt and expressed by many scientists today. Concern about the pernicious cultural influence of postmodernists, social constructionists, and other skeptical types has almost reached the level of hysteria in some portions of the scientific community. It's something that has changed little between Planck's time, and the "Science Wars" of our day: elder statesmen of science extolling a quasireligious defense of the secure, solid, unassailable ground of scientific truth as the bedrock of civilization, feeling besieged by the skeptical and possibly immoral forces of unbelief and contradiction.[21]

Meanwhile, things in the quantum world seem only to have gotten more puzzling and entangled, and quantum physicists have been forced over and over again to acknowledge the unavoidable collusions that go on between their own experiments, observations, theories, and instruments, and the real outer world. Physicist John Wheeler has been one of the most original and audacious quantum theorists of the post–World War II era. (He also helped develop the A-bomb and the hydrogen bomb.) Recently, he has distilled most of his work into the pithy phrases "No question? No answer!" and "It from bit."

> Otherwise put, every *it*—every particle, every field of force, even the spacetime continuum itself—derives its function, its meaning, its very existence entirely, even if in some contexts indirectly—from the apparatus-elicited answers to yes-or-no questions, binary choices, *bits*.
>
> It from bit symbolizes the idea that every item of the physical world has at bottom—at very deep bottom, in most instances—an immaterial source and explanation; that which we call reality arises in the last analysis from the posing of yes–no questions and the registering of equipment evoked responses; in short, that all things physical are information-theoretic in origin and this is a *participatory universe*.[22]

"This is a participatory universe." It's a bold statement—even an overstatement, of the kind that has made quantum physics so prone to vastly overextended and overinterpreted conclusions about how uncertainty, indeterminacy, or the interpenetration of subject and object become definitive, all-embracing cosmic principles that illuminate everything from holistic medicine to enlightened business management strategies to the failures of justice in televised trials. (And if a feminist theorist of science were to write something similar, she or he would be met with much harsher criticism than Wheeler receives.) But Wheeler offers this formulation as a "tentative idea or working hypothesis" that might very well be wrong, but as "the policy of the engine inventor, John Kris, reassures us, 'Start her up and see why she don't go!'"

Moreover, a close reading of the passage above shows that Wheeler is careful to leave certain qualifications in place: the immateriality at the bottom of the material world may lie so deep as to be not worth asking after. The interactions between questions and things, instruments and objects, in some contexts other than the subatomic, quantum context may be so indirect as to be irrelevant. We have to try to be very modest and precise here: Photons are not immune systems, and the equipment for evoking a response from a subatomic particle is not the equipment for evoking a measurement of groundwater contaminants. The strange interactions in the muddled space between bit and it, question and answer, virtual and real, immaterial theory and material fact, are going to be different in different scientific pursuits and contexts. A recurring demand in this book will be to track the specific interactions that happen in the wildly varied situations in which the sciences are actively pursued.

Nevertheless, Wheeler's concept of a participatory universe drawn from quantum physics is an excellent working hypothesis to try to incorporate in all of the sciences, and in all of our thinking and experimenting with the sciences. The sciences have to start with the knowledge that there is always some path, however indirect, by which theory and fact participate with each other. All of our scientific pursuits have to somehow acknowledge that the real and the virtual partake of each other, that the most factual and solid signal that resists is always informed by seemingly immaterial thought-styles. That's a much better way to pursue the sciences than with our current conventional notions of solid, solitary facts, securely walled off from or unmuddled with our changing concepts and cultures.

To start seeing why, let's return to the world of Max Planck and his book *Where Is Science Going?* We've already seen how Planck was deeply concerned about the cracks in the foundations of science and reason, and the implications which these cracks of skepticism and "irrationalism" held for the wider culture. Trying to shore up those foundations, Planck set out two theorems that together formed what he called the "cardinal hinge" on which the physical sciences turn. They are theorems that many scientists today would agree with, and which form our common-sense, conventional view of the sciences: "(1) *There is a real outer world which exists independently of our act of knowing*, and (2) *The real outer world is not directly knowable*." [Planck's italics]

Planck, who had just been lamenting the spirit of contradiction abroad in the current culture, admitted that these theorems were themselves mutually contradictory—that is, he could not rationally justify their conjunction. "And this fact," he continued, "discloses the presence of an irrational or mystic element which adheres to the physical sciences as to every other branch of human knowledge. The knowable realities of nature cannot be exhaustively discovered by any branch of science. . . . We must accept this as a hard and fast irrefutable fact. And we cannot remove this fact by trying to fall back upon a basis which would restrict the scope of science from the very start merely to the description of sensory experiences. The aim of science is something more. It is an incessant struggle towards a goal which can never be reached. Because the goal is of its very nature unattainable. It is something that is essentially metaphysical. . . ."[23]

Somewhere along the line, Planck argued, the scientist has to jump the wall or make the leap, and introduce a "metaphysical hypothesis":

> [S]cience demands also the believing spirit. Anybody who has been seriously engaged in scientific work of any kind realizes that over the entrance to the gates of the temple of science are written the words: *Ye must have faith*. It is a quality which the scientist cannot dispense with. . . . The reasoning faculties alone will not help him forward a step, for no order can emerge from that chaos of elements unless there is the constructive quality of mind which builds up the order. . . . Again and again the imaginary plan on which one attempts to build up that order breaks down and then we must try another. This imaginative vision and faith in the ultimate success are indispensable. The pure rationalist has no place here.[24]

This conventional view of the sciences hinges on a series of contradictions, or juxtaposed oppositions: physics/metaphysics, reason/imagination, science/faith, and even laboratory/temple. While the public, common-sense view of the sciences almost exclusively privileges the term in front of the slash—putting its best face forward—in fact, the opposing term is always present. It *has* to be there, even if it generally has to be swept under the rug for public occasions. There's certainly nothing wrong with contradictions per se; they're a part of any system, and indeed, the sciences wouldn't be possible or nearly so generative without the engine of contradiction. But when contradiction is denied or swept under the rug, it becomes much harder to get a sense of the peculiar effects of that particular contradiction. More importantly, it becomes almost impossible to imagine another system of thinking that might work just as well, although harboring its own contradictions.

Planck's radically bifurcated world is utterly dependent on opposed terms like imagination and reason, faith and science—and fact and theory. Planck asks us to have faith in what cannot possibly make sense, that science approaches the truth ever closer, enacting a hyperbolic nonencounter with the absolutely separate—ever nearing reality, but never touching it. That can't make sense and *has to* be an article of faith, because if "nature" or "truth" is indeed radically separate in the sense that Planck postulates, then it's impossible to know if you are approaching it more closely or not. As kids, many of us have played the game where someone hides an object, and then gives verbal cues to someone else seeking it: *you're getting warmer, warmer, colder, ooohhhh you're burning up!* But for that game to be played successfully, someone has to know where the object is hidden. The game needs a God, in other words, who can be outside the system looking in. In order to judge distance and progressive approach, you have to know the final destination—which Planck has just said is humanly unknowable.

Furthermore, if it's true that scientists have to have this kind of faith, it also true that they can't put too much faith in faith. Passionate adherence to a set of theoretical beliefs is a recipe for bad science . . . sometimes. Because at the same time, scientists have to be skeptical about being consistently skeptical, since that is also likely to land them in trouble. Deciding when to have faith, and when to skeptically attack results and theories is an expert, subtle art or craft. There are no hard-and-fast rules to follow in this world of contradictions and experimenting, and the best scientists—like Copernicus, Millikan, Fleck, and even Planck himself—will be the ones who can muddle through.

But what makes us think we really need Planck's kind of faith anyway? Why is it that our culture, by and large, encourages and even demands that we believe that the sciences progressively approach and approximate a full and final truth? Why are we so often presented with the stark alternatives: a world of rock-solid, timeless facts-or completely adrift in a raging sea?

One possibility is that the sciences then gain the same exalted and authoritative status as religion has had, as Planck's use of the word "temple" should make clear. There are many ways in which the sciences, and our cultural attitudes toward them, are indeed like religion. But it would be a mistake to reduce and equate the sciences to religion, and to say that reason and the sciences are simply another form of faith. The world is much more complicated than that. Part of that complexity involves the fact that progressivism in politics has traditionally been connected to this progressivism in the sciences; eliminate (even if only in our epistemology) the real world which is approached progressively, and you eliminate any basis for making judgments not only about what is scientifically "better," but also morally or politically "better." We will question this linkage more closely in Chapter 4.

Our point here is more modest: It's *possible* to pursue another kind of thought-style regarding the sciences, the work of experimenting, and the status of facts. This other thought-style —"muddling through" is one way of putting it—is coherent, rational, productive, reliable, and critical of established truths, including its own. This way of pursuing science is no more (and no less) marked by its own internal contradictions than the conventional view, described succinctly in Planck's two axioms. The opening glimpses of that thought-style provided here by Fleck, Dewey, and others will be used as building blocks for the following chapters.

And in the end, the ways in which Planck and Fleck or Dewey view the nature of the world and the nature of the sciences might be very similar, or at least compatible. Each seems to be saying that "nature" or "reality" remains inaccessible, and that what we have access to is only a kind of imperfect simulation of it, in the form of what Fleck calls our "stylized knowledge," or what Planck calls our "constructive quality of mind."

And there is certainly plenty of room for common ground among these worlds, a middle space or interzone where the differences might be better tolerated or even suspended. Diplomatic negotiations in the science wars are not only possible, they're necessary, and they could be quite productive. Planck's image of the sciences as an "incessant struggle" toward an unattainable goal is very close in spirit to what we are calling "pursuing sciences." Throw in his notion of an inexhaustible outer world that can't be known directly, and we can find even more room for discussion. An inexhaustible reality, a nature so excessive that it can provide multiple changing answers to our multiple, changing, questioning experiments, may not be so far from a world in which facts are made and nature is produced—or what Donna Haraway, borrowing from Native American traditions, calls a "trickster nature": a shape-shifting, provocative world which can never quite be pinned down. There should be *some* room to work with here in the middle.

And it's in the middle, at the hinge of contradictions, where muddling through happens. Linger over *Coastline Measure*, the Mark Tansey painting which appears on the cover of this book. Planck's either/or choice—either the firm, factual rocks of solid truth, or the raging storm of skepticism—becomes a both/and here. Pursuing sciences isn't a matter of approaching an outside world ever more closely, but rather the difficult, slippery, often risky and usually admirable work at the limit, on the border of solid rock and stormy sea. When the surveying team returns next year, the coastline will still be a disjunctive juncture of stability and change, but its precise contours will have shifted into a new pattern whose measure must be taken again. Immersed in their work, concentrating on keeping their footing in this treacherous territory, they may lack the privileged perspective of the distant observer who can see the suggestion of an oddly repeating, fractal structure that penetrates to the limits of their world.

Is this what bedrock is—a layered, iterated pattern made solid by its own cross-referenced iterations?

Do the stories we've told above cohere into some kind of theory of facts? Not exactly. You wouldn't necessarily say that Tansey's painting is a theory of coastlines. But just as Tansey provides an image that prompts a series of speculations about rocky shores and the immense forces shaping them, we've tried to provide a collage of fact-images whose structure is meant to initiate a new set of responses to the question: what are the sciences *grounded* in?

In other words, if you think this chapter is supposed to answer one of our "really?" questions—"Are facts really discovered via experimenting, in a separate reality out there, or are they really manufactured, social constructions?"—you're going to be disappointed. That's probably what a good philosopher would call an undecidable question. At the very least, judging by the number of books already written on the subject, it's the kind of question that promises to engage philosophers and many others for a long time to come.

It's not the most productive question. What we're more interested in, and what we hope will be more productive in the long run, is inventing a new word that can help keep us close to the question without resolving it, to paraphrase Foucault—a word for further experiments on experimenting.

Why does it seem so naturally, intuitively true that, as Planck says, "there is a real outer world which exists independently of our act of knowing," but which our acts of knowing called the sciences can uncover and approach (but never meet)? Part of the answer certainly has to do with the experiences that scientists have every day in their laboratories or at their desks, where it seems to them undeniable that nature compels their hands and minds in powerful ways. Another reason has to do with the dominant culture of the sciences in which we've all grown up, including the way that the sciences are almost always taught in our schools. But another part of the answer lies still deeper, and has to do with language.

The ways in which most of us (including Planck) think about what the sciences really are and how they really work are threaded together with what language is and how it works. It's no accident that Planck's opposition between subjective act of knowing and objective world resonates so strongly with the subject-(verb)-object structure of language. Alternative styles for thinking about the sciences don't find nearly so comfortable a fit, and so lack that natural feeling of common sense: facts aren't discovered, but produced?! By what subject? Out of what object? *The signal that resists?!* Language cannot simply be overthrown or overcome.

Language can, however, be played, and the first step might be a new word. Throughout this chapter we've referred to this *something* that has been variously termed "a material world," "the world itself," or just "the world." "Reality" is the key word here. The word "reality" signifies the irrefutable thing, or the signified, that makes Johnson's toe hurt, makes Millikan's experiment eventually come out correctly, and flattens the social constructionist's body at the end of the airplane experiment. It's a word full of finality, for the brute reality of brute facts.

Can we come up with an alternative signifier? The thing, the signified that we're trying to point to here is what will emerge over the course of the next few chapters as "a muddle, or complex assemblage, of material, social, cultural, linguistic, technical and other forces—although those things are just our provisional names, too—that constitute what is most frequently called reality." *That's* the thing that makes pursuing the sciences so hard, so useful, so important, and so problematic.

That's the thing that we have to learn to recognize as the ground on which the sciences are pursued, and on which they depend—ground in the sense of the violent conjunction of rock/sea.

But that phrase in quotation marks, as you can see, is a clumsy and long construction that you would soon tire of seeing in print. "The signal of resistance by the thought collective" is certainly not without its problems, either—not the least of which is that it tends to alienate scientists who aren't already partly sympathetic. We need another term for "reality" that will work on our coastline, in the middle between realists and constructivists. It should be close to "reality," since it's an important word that we wouldn't want to get rid of all together. But the new word should be something which breaks "reality" up at the same time, just a bit. (And all we need is just a bit, since reality as a concept depends on absolute purity, zero leakage between facts and theories, things and words, objects and practices, land and sea.) So instead of reality, from now on we'll be writing (about) realitty. When you speak it, you can't hear the difference. We can go around talking about realitty, just as all the other good scientists do. You can only see the difference in writing, where it introduces a difference into the word-concept of reality in two ways. One is quite literal: realitty is an anagram of "alterity," a word which signifies difference, otherness in general, in all its multiple forms. Alterity is "the state of being other or different; diversity, otherness." Like reality, then, realitty is a kind of alien presence that can mock our wills and desires.

The second difference incorporated in reali*t*ty is an injection of time, always represented in scientific equations by the lowercase italic *t*. Reali*t*ty is what you get when you throw time into the equations of reality. If reality always stays the same, but we believe that we approach it ever more closely, reali*t*ty always changes, and it's no longer a question of nearness and approach, but of successive experimental practices and their successive (and successful) thought-styles.

The sciences today are better seen as a matter of re-producing realitty so that it can be worked on and experimented with, not simply re-presenting reality so that it can be thought about and understood. Why is science better seen that way? Why open a book with such stories? Or as Dewey asked eighty years ago: "Since instrumentalism admits that the table is really 'there,' why make such a fuss about whether it is there as a means or as an object of knowledge?"[25]

Dewey formulated a number of responses to that question, but the primary one was that the conventional view of science and the facts "commits us to a view that change is in some sense unreal, since ultimate and primary entities, being simple, do not permit of change."[26] Seeing entities like facts and things as complex rather than simple, as mediated and manufactured rather than ultimate and primary, at least holds open the possibility of change. Fleck's world is not divided in two; he doesn't need or want us to believe in a separate or future world of total truth that can be approached. He only wants us to pay attention to *this* world, to watch how we stylize our observations and thoughts, to think about how facts emerge via experimentation, without making the metaphysical leap to believing that those experiments and those facts are leading us to some fuller realization.

This doesn't mean that everything is relative, or that "all that is solid melts into air." Notice how that famous aphorism skips over the middle term between solid and gas, liquid. Look again at *Coastline Measure*—it's not about the evaporation of solidity; it's an image of solidity *and* liquidity *and* muddling through in their midst, all at once.

This doesn't mean that it would be easy to make a world without, say, electron spin. Seeing electron spin as the complicated sign-force of realitty that Feynman visualizes, gives it no less stability than seeing it as a simple brute existence in reality. But it does allow us to see that as mathematics slowly changes, as instruments become different, as the ensemble of physical theories in which "spin" is an important term continues to evolve, a new fact is someday going to emerge to replace electron spin, just as quantum-mechanical electrons have replaced the miniature billiard balls of classical physics. It means that realitty will change, because that's what realitty does best.

Moreover, shifting the focus of our thought-style from reality to realitty might better allow people to ask such questions as: If we're told that a definite low level of benzene in our drinking water in fact poses no risk to health, what realitty does such a statement depend on, or belong to? In what realitty is a statement like "intelligence is genetic" a factual, rational one—and are there any other realitties that make as much if not more sense?

It's not a perfect invention, this word-concept. It doesn't do everything we want it to do, and it will no doubt do too many disturbing things for others. The coastline of the sciences is a tumultuous, uncertain place. Does a rocky realitty indeed harbor a kind of truth, serve as a reliable fact? To paraphrase John Wheeler again, there's only one way to tell: start it up, and see if it doesn't go.

Our stories about famous scientists and their scientific achievements are better than the conventional accounts because ours are grounded in a complex realitty instead of paltry reality. The sciences themselves are better when they are situated in realitty. Realitty is neither discovered nor constructed. It is pursued and performed; it pursues the scientist. Realitty is never perfect nor direct, but always indirect—which is pretty damn good. Realitty is completely Other, inhuman, overwhelming, inexhaustible Nature; it is nothing without us. Realitty resists utterly; it yields supplely. Realitty is the beginning and end of experimenting.

Articulating

Sacramental Swan

Gregory Bateson was . . . what *was* Gregory Bateson? An anthropologist studying human culture? An ethologist observing animal behavior? A philosopher playing with ideas? A theoretical biologist enmeshed in the social and intellectual feedback loops of cybernetics?[1]

It's hard to say exactly what Gregory Bateson was, which is why we begin this chapter with him. The questions we'll be staying close to in this chapter have to do with the problem of *saying things exactly*. If it's true, as Max Planck argued, that we can never have *direct* knowledge of the "real outer world," does that mean that all of our indirect knowledge is, in some sense, inexact as well? Can the sciences—at least some of which are referred to, sometimes, as "the exact sciences"—ever say something exactly? If they can, what language do they use? Are the answers they give in response to "really?" questions spoken in mathematics? Symbolic logic? The same language that we use for novels, poetry, road signs, and conversation? Or some combination of all of these? And a less obvious but equally important question: when the sciences make these kinds of speeches about what's really real, on what platform do they stand?

When Richard Feynman tried to articulate how he visualized the world when he was doing physics, and in particular, how he "saw" electron spin, we could say that he was speaking metaphorically. Should all statements about reality, then, be taken as metaphors? That is, is the real really a metaphor? That would be a very unmetaphorical way of asking the question and would demand something like an equally unmetaphorical yes or no answer. Maybe there's another, less exact and less direct way to pursue the questions of exactness, language, and the sciences.

Consider Bateson trying to explain to his daughter what it means to speak in metaphors, in terms of something being sort of something else. They start their dialogue—in fact, "sort of" a dialogue, since Bateson wrote it himself, for his book

Steps to an Ecology of Mind—by looking at how a ballet dancer can sometimes be a sort of swan, and then move to the question of transubstantiation:

> *Father:* And then there is that other sort of relationship which is emphatically *not* "sort of." Many men have gone to the stake for the proposition that the bread and wine are *not* "sort of" the body and blood. . . . If we could say clearly what is meant by the proposition "the bread and wine is *not* 'sort of' the body and blood," then we should know more about what we mean when we say either that the swan is "sort of" human or that the ballet is a sacrament.
>
> *Daughter:* Well, how do you tell the difference?
>
> *F:* Which difference?
>
> *D:* Between a sacrament and a metaphor.
>
> *F:* . . . Well—I think it's sort of a secret.
>
> *D:* Do you mean you won't tell me?
>
> *F:* No—it's not that sort of secret. It's not something that one must not tell. It's something that one *cannot* tell.
>
> *D:* What do you mean? Why not?
>
> *F:* Let us suppose I asked the dancer, "Miss X, tell me, that dance which you perform—is it for you a sacrament or a mere metaphor?" And let us imagine that I can make this question intelligible. She will perhaps put me off by saying, "You saw it—it is for you to decide, if you want to, whether or not it is sacramental for you." Or she might say, "Sometimes it is and sometimes it isn't." Or "How was I, last night?" But in any case she can have no direct control over the matter.
>
> . . . I'll start again. The swan figure is not a real swan but a pretend swan. It is also a pretend-not human being. It is also "really" a young lady wearing a white dress. And a real swan would resemble a young lady in certain ways.
>
> *D:* But which of these is sacramental?
>
> *F:* Oh Lord, here we go again. I can only say this: that it is not one of these statements but their combination which constitutes a sacrament. The "pretend" and the "pretend-not" and the "really" somehow get fused together into a single meaning.
>
> *D:* But we ought to keep them separate.
>
> *F:* Yes. That is what the logicians and the scientists try to do. But they do not create ballets that way—nor sacraments.[3]

A good muddling scientist, when asked if she is saying something "really" true about nature, or just something "sort of" true, will know that she can't say. (She also knows that she has to say *something*, after she rolls her eyes, invokes the deity, and prepares herself to start over again—and then again. It's what Bateson would have called a double bind: knowing that she can't say what is asked of her, and knowing that she must say it.) She can't say it's *either* real *or* metaphorical because that kind of distinction is a secret, sort of. It's not that the scientist knows the difference between the real and the metaphorical, but won't say; nor is it the case that the scientist *doesn't* know, and so can't say the distinction. It's more like she knows, but can't say because she doesn't know that she knows.

Or, to put it in terms taken from Ludwig Wittgenstein, who thought long and hard about what it means to try to say something exactly: The statement that would speak the distinction between the real and the metaphoric cannot be part of the system of real and metaphoric statements; it is outside that system and hence unspeakable. You can try to make such distinctions and, oddly enough, it's important to try. But at the same time, you do not make art or sacraments that way. And you don't make the sciences that way, either.

Transparency

We invented the word realitty to stay close to the question "real or constructed?" rather than to resolve it—to keep our eyes on the collusions between things and thoughts, thoughts and experiments, and facts and theories. Now we move on to do some experimenting with this latter term, theory.

We know now that the ideal picture of building up (by either logical deduction or induction) shining, illuminating theory out of dull, dirty, brute facts just doesn't capture the messiness or richness of "the scientific method." Even the most curmudgeonly philosophers of science, such as Sir Karl Popper, know that theorizing is never simply a matter of induction from empirical facts, but is a more elusive and less perfectly rational process that might even involve the imagination.

Can we say what theory is exactly, then? Starting with the dictionary meanings, we quickly run into trouble, trying to reconcile various definitions ranging from "a coherent group of general propositions" to "guess or conjecture." Wherever it fits in this spectrum, it looks as though theory is located in another world, and applied to this one: *Sounds good in theory, but will it work in practice?* A theory is "abstract," mind-stuff, not of this material world. Theory is just a set of statements, diagrams and equations in a manuscript, words on a page representing ideas in the air.

The word's roots in the Greek *theorein*, meaning "to view," are no less riddled with different interpretive possibilities. But that older definition at least brings into view the idea that our habitual ways of thinking about "theory" are tied up with direct visual perception—images of illumination, enlightenment, eyes, and less directly, mind. It's hard to even think of the words "theory" and "theorizing" without getting the image of someone's head with a light bulb over it. Theory is part of an ideal realm that is as seemingly insubstantial as light itself.

Implicit within these conceptual habits of theory as immaterial light is a family of ideas tied to the notion of transparency. Theories in the sciences are like lenses, which enable us to focus on the real world or on truth, without distortion. Like light, the real and the true pass directly to our eyes via scientific theories. The privileged access to the real world that we grant to the sciences is presumed to be, must be, unmediated. That's exactly why we privilege them: the sciences give us direct access to the real, with none of the coloration, diffraction or other distorting effects of technologies, subjectivities, or cultural or other interpretive factors. No shadow should fall between the eye and the thing, between the idea and the actuality.

But why? What is the danger of mediation that it evokes such horror? Because it starts with the idealization of transparency, mediation in the conventional view can only be conceived negatively: to be mediated, to be in the middle is to be fallen, to deny or to have been denied the pure illumination of objects. (The subtle force of religious culture is at work here in this part of the sciences.) When the positive ideal is the absolute transparency of the "scientific perspective," a mediated and limited perspective can only be about distortion, the intrusion of error, the presence of pollution, the insertion of ideology or political force between us and it. This middle space must be excluded because only bad things can happen there; the middle can only be impure and contaminated. Ideally, a scientific instrument like a telescope or particle detector, or a scientific theory like evolution by natural selection, should fade away and allow things or facts to speak for themselves.

Let's be clear about the productive qualities of such an ideal. It always has prompted scientists to do their best work, making them obsessed with the proper functioning of their instruments. A good scientist is dedicated to getting instruments to "work properly" and produce "good data." That is the exhilarating, frustrating, demanding, and admirable work that goes on every day in hundreds of laboratories, observatories, and field sites around the globe. Millikan would have never defined the charge of the electron if he had not painstakingly tinkered with his experimental apparatus, checked and rechecked observations made with it, patiently gone over calculations. Spare and unreliable as Copernicus's observational data were, they nevertheless demanded that he refine his mathematical and geometrical tools and assumptions. Similarly, chromatic aberration and other effects of poorly crafted lenses were problems that plagued observation of cosmological and microscopical reality for hundreds of years—and that we learned to solve. You didn't have to be an expert in optics to see the difference in those before-and-after photographs from the repair of the Hubble Space Telescope as they made their triumphal appearance in the media. Somehow we know the difference between a bad photo of the edge of the known universe, and a good one.

But why do we think that, tens of millions of dollars and a space shuttle mission later, an enormous array of photodetectors, computer software and hardware, and thousands of other hard and soft components suddenly turned transparent and allowed us to see exactly, directly what the edge of the universe *really* looks like? Why do we think that we were seeing directly, for the first time, a part of reality that we had never seen before? Could we instead be satisfied with knowing that we had coaxed into our world a part of reality that hadn't been here before? And just what, for heaven's sake, could that mean?

Articulating "Articulating"

To stay close to these questions, we need a few more alternative words. *Articulate*, *articulation*, and *articulating* might be made to carry a different set of associations from those of "theory" and "theorizing." Articulate has a number of meanings, as both adjective and verb, and the ensemble of these meanings gives us a better set of working concepts for reformulating how we use theory, and how we think about what it is.

First are those meanings that revolve around language. To be articulate is to be "endowed with the power of speech," or to be capable of "expressing oneself easily in clear and effective language." To articulate is to give words to, to try to express, describe, or invent something that wasn't previously a part of language or thought. If theory is the mental capture and representation of an illuminated world "out there," then an articulation is something one speaks rather than sees, that is expressed rather than mirrored. An articulation is something put as adequately as possible into words, rather than something seen in its true nature.

Then there are the anatomical or structural senses of "articulate"—disparate articles jointed into a larger, segmented structure. Imagine a lobster, or a Rube Goldberg contraption. It involves hinges where disparate elements turn on each other, finding new leverage points, creating new angles and forces. If theory is a mental construct, then an articulation is corporeal, an organic or mechanical (or some hybrid, in-between form of these) structure in which different parts meet and rub against each other. There are tensions and frictions, squeaky wheels, generated heat, noisy crashes and slips—yet there is an overall ability to accomplish or produce something important to us.

To think in terms of the articulations of science rather than scientific theories, of articulating rather than theorizing, would restore questions about the importance of language to the domains of the sciences, and questions about our ability to say things exactly. Thinking in terms of articulations would also help us pursue sciences in which theory is less a set of ideas that somehow mirror the world, and more an assemblage or contraption of different parts, humanly designed within the world to do certain kinds of work.

Theories in the sciences are lobsterlike entities made out of disparate, heterogenous elements, including numbers, relationships, machined metal, purified enzymes, hazy concepts, precise and imprecise metaphors, and many other things and nonthings. The sciences are "science" not because they reflect or transparently transmit the world, but because their many articulated connections enable them to crawl around, clutching and poking, within that world

These are rather abstract articulations of what articulation involves. The rest of this chapter fleshes out this starting structure, adding example upon example to show why it is better to think of the sciences as involving mediated articulations, rather than immediate, transparent theories. If we can learn to keep the terms of mediation in view and in question, to track down all the things that are being articulated, even down to the very tail of the lobster, then we will be better equipped for the kinds of critical inquiry into the sciences that democratic society now sorely needs.

Pens, Swords, Tongues, and Politics

"Controversye Is a Civill Warr with the Pen which pulls out the sorde soone afterwards."
Earl of Newcastle to King Charles II

In the first chapter we saw how the new experimentalists of seventeenth-century England were creating a new discipline and form of scientific knowledge, which they called natural philosophy. Here we take up this topic again, extending our view to encompass the broader set of articulations within which experimentation was embedded. Why was experimentation persuasive? What was particularly valuable about it, and the kinds of knowledge which it yielded? Can experimenting be valuable now for the same reasons?

When Charles II was restored to the English throne in 1660, overt civil war might have ended, but dissension and controversial beliefs of all manner remained pervasive threats to civil order. Anything having to do with the beliefs or knowledge claims underlying politics, religion, law, or the natural world was a matter of vital social concern. The Uniformity Act against dissent in religion resulted in the formal expulsion from their posts in 1662 of hundreds of ministers from Presbyterian, Anabaptist, and other sects. Laws were enacted to enforce fealty oaths to the king, and against "sedition in print." The number of licensed printers was cut from sixty to twenty, and placed under the authority of the Archbishop of Canterbury and the Bishop of London. In the words of one licensing official, "[T]he spirit of hypocrisy, scandal, malice, error and illusion that achieved the late rebellion was reigning still."[3] The new "coffee-houses" were placed under surveillance as potentially dangerous meeting places of sectarian groups.

Thus when Charles II granted to the newly founded Royal Society the right of assembly, the right to correspond with foreign members, and the right to publish without censorship (not to mention things like the right to dissect the corpses of prisoners), it's understandable that he would want certain assurances in exchange. The Royal Society intended, in the words of Robert Hooke, pioneer microscopist and drafter of the Royal Society's charter, "to improve the knowledge of natural things, and all useful Arts, Manufactures, Mechanics, Practices, Engynes and Inventions by Experiment"—*and* it would do so without "meddling with Divinity, Metaphysics, Moralls, Politicks, Grammar, Rhetoric, or Logick."[4]

A divide was forming, with knowledge (arrived at through experiment) on one side and power (in interestingly diverse forms) on the other. (It's a divide to which we will return in the next chapter.) Yet even though such a division was indeed emerging, its cross-border traffic was not so much nonexistent as very consciously managed—stylized, even. Thomas Sprat's *History of the Royal Society* compared the new houses of experiment to a theater, where disputes could be safely staged: "There we behold an unusual sight to the *English Nation*, that men of disagreeing parties, and ways of life, have forgotten to hate, and have met in the unanimous advancement of the same *Works*. . . . [I]t gives us room to differe, without animosity; and permits us, to raise contrary imaginations upon it, without any danger of a *Civil War*."[5] Experimental knowledge was literally a type of enactment, designed to perform the kinds of acts on the stage of knowledge that should be emulated in the larger house of power, even while insisting that the two spheres were separate. Differing "contrary imaginations"—different theories, in other words, or different articulations—could be managed by unlinking or disarticulating them from the experimental production of "unanimous" facts. Check your politics at the door and witness the production of consensus inside; not only would you produce the best possible knowledge, but you would enact the social ideal for managing—indeed, eliminating—controversy.

Agreeing to exclude controversial discussions or analyses of "Divinity, Metaphysics, Moralls, and Politicks" is one kind of deal, perhaps even an understandable one in light of the violently contentious times. But what threat to order, civil or otherwise, was perceived in "Grammar, Rhetoric, or Logick"? What was it about language that led Sprat to remark in his history that "[t]he Truth will be obtain'd between [men]; which may be as much promoted by the *contention* of hands, and eyes; as it is commonly injur'd by those of *Tongues*"?[6]

Enter Thomas Hobbes.[7] Hobbes shared some of the experimentalists' values. He was convinced that the production of knowledge could be entrusted only to gentlemen, and that those gentlemen should avoid "contumelious language." Only cool heads and proper manners could prevail, for they were necessary for the exercise of judgment (the topic of Chapter 4):

> It cannot be expected that there should be much science of any kind in a man that wanteth judgment; nor judgment in a man that knoweth not the manners due to a public disputation in writing; wherein the scope of either party ought to be no other than the examination and manifestation of the truth.[8]

But unlike Robert Boyle and other members of the Royal Society, truth for Hobbes could not be manifested by experimenting. Hobbes argued that by avoiding issues and questions of language and logic, or reason, and focusing only on experimental facts supposedly unencumbered by metaphysics, Boyle and the Royal Society could never arrive at real knowledge (and therefore, real intellectual and social order). It wasn't a matter of getting rid of metaphysical language, contended Hobbes, or putting it off-stage, but of finding the *proper* metaphysical language. That rival interpretations of an experimental phenomenon such as "the vacuum" were possible proved the necessity of such a proper language to Hobbes, if something like true knowledge was ever going to be had.

The model of such a proper language for Hobbes was geometry. Why geometry? In part because it was, according to Hobbes, "the only science that it hath pleased God hitherto to bestow on mankind." But more important than its divine origin was the fact that geometry provided irrefutable results and unquestionable truth. "All men by nature reason alike, and well," Hobbes argued in his *Leviathan*, "when they have good principles." Geometry was a better staging of how social controversy was to be managed than experiments were because it showed that the only way to *reach* agreement was to *start* in agreement on fundamental principles, and reason from there. Moreover—and here's where Hobbes's argument takes an interesting twist—at the solid foundation of geometry were definitions of words. "Settling the signification . . . of words" and "plac[ing] them at the beginning of reckoning" was Hobbe's first methodological principle. From there, "right reason" took over, an entirely straightforward affair of "adding and subtracting, of the consequences of general names agreed upon for the *marking* and *signifying* of our thoughts." Experimental facts were to Hobbes "nothing else, but sense and memory." They couldn't serve as the basis of knowledge, let alone as the basis for resolving social disputes. In other words, Hobbes knew that experimenting had to be *articulated*, joined to language *via* language.

Of course, Hobbes had a rather authoritarian view of language, as of most everything else. But our goal here is not to side with either Hobbes or the experimentalists, but to carve out some working principles somewhere in between. Seventeenth-century England, and the rise of the experimental sciences within it, is a complicated and contradictory space, and does not lend itself to simple conclusions and good-guy, bad-guy lessons. On the one hand, we can admire how the experimentalists eschewed discussion of "true causes" in favor of probable explanations. As we emphasize in Chapters 1 and 3, there is something in this aesthetic of knowledge that is well worth preserving: its (potential) sense of modesty, finitude, and tolerance of difference. History has certainly shown that experimenting can indeed be a wonderful source of new things and new ideas, and can break up old habits of knowledge and power. However imperfect or uneven, experimental science is an immensely valuable antiauthoritarian resource in many situations. (And in other situations, not.)

On the other hand, we can hold our noses and take an important lesson from the fairly odious Hobbes: Language matters. Language is essential to reason, and can't be gotten rid of so easily with a few new machines. Somewhere along the line—no matter how long that line is—every experiment, every mathematical equation, every pure numerical value will have to find its way into words. But rather than share Hobbes's faith that finding the one proper language and reason will guarantee knowledge, we only insist that attention be paid to the varieties of language articulations in the sciences, and their effects. If the question for Hobbes was what constituted proper language, the question for us is how languages participate in making knowledges proper. That unavoidably awkward-sounding question sits solidly in the muddled middle between Boyle and Hobbes, and is one to which, in various ways, the remainder of this chapter stays close.

Copernicus Revisited

In the previous chapter we began a more complicated story of what Copernicus was trying to do with his book, *De revolutionibus*, and how he thought he could do it. Was he merely trying to "save the appearances," to simply provide a possible, working model that would explain the apparent movement of the planets? Did he see directly to the truth of empirical planetary motions, his eyes and mind unclouded by philosophical or religious doctrines? Did he see himself in opposition to the Church and religious dogma, instantiating a new order of truth?

What historian of science Robert Westman calls the Vulgar Triumphalist version of Copernicus no longer holds up among professional historians, although you can still find it in science textbooks, popular magazines and newspapers, and the popular writings of scientists today.

In our culture, Copernicus is identified as a scientist allied with the truth, a man who defied the priests and their traditional, false beliefs. This is what we learned and continue to teach our children in school. We'd like to offer a different story, a version of history both better grounded in the facts, and better able to help us understand the sciences in productive ways. In this story Copernicus stands firmly in between the opposed terms of scientist and priest, and science and tradition; if anything, this portrayal should make him more admirable, not less.

Copernicus lived his entire life as a church official, a canon in charge of such things as local military defenses and health care. This position entitled him to collect rent from the local peasants. As was usually the case during the late Renaissance, he obtained this office and the kind of upward mobility it afforded through family connections. He further upped his income when his uncle, the bishop of Varmia, appointed him to the position of scholaster at the Church of the Holy Cross in Breslau. Had Copernicus chosen to do so, he might have made a career move into the papal bureaucracy in Rome.

He chose a more middle path, however, pursuing astronomy and mathematics while maintaining his church offices and seeking further patronage from the larger church system. Here's where the history of science, religion, and humanism come together in an interesting and productive intersection.

Historical analyses of the University of Cracow have articulated the profound intellectual and cultural shifts taking place in the early sixteenth century—larger changes of which Copernicus was one small part. There, at one of the greatest of the medieval universities, Copernicus not only reaped the benefits of one of the best faculties in mathematics and astronomy but was also exposed to the new "critical humanistic attitude which was transforming older cultural and educational values."[9] The new humanists of the late fifteenth and sixteenth centuries—lovers of language and experts in Roman and Greek poetry, philosophy, and moral and civic literature—placed themselves in service (often as diplomatic emissaries) to the papal court. Copernicus, then, didn't come out of nowhere, the lone hero of science who single-handedly provided the material to overthrow a repressive tradition. He was one of many who attended the universities at Cracow and elsewhere, and who would collectively transform the entire cultural and intellectual climate. And Copernicus obtained a whole bag full of tools and skills to help in this transformation—"science" was just one part of it.

Copernicus also had a network. Pope Clement VII's secretary, Johann Albrecht Widmanstetter, was one of these humanists, who explained Copernicus's theories to the pope and his associates. In exchange for these services, Clement gave Widmanstetter a gift as well—not a religious treatise, but a Greek manuscript of scientific essays. This gift illustrates the need to resist easy oppositions between "church" and "science" at this time. Another cardinal whom Widmanstetter served, Nicholas Schönberg, wrote to Copernicus in 1536 and asked him to send a copy of his manuscript to Rome. In a strategic move, Copernicus inserted this letter into *De revolutionibus* between the title page and his preface to the pope, in effect allowing Cardinal Schönberg to announce Copernicus's "new account of the World": "In it you teach that the earth moves," wrote Schönberg in his letter, "that the sun occupies the lowest, and thus the middle, place in the universe."[19] Contrary to what the "preface" of his editor Osiander might suggest, then, it seems that Copernicus did indeed want to say that the earth *really* moves and the sun *really* was at the "middle" of the universe—although he let the cardinal say it for him first. (At the time philosophers and other professional classifiers considered astronomy itself to be a "middle science," sandwiched between physics and mathematics, and thus somewhat less pure and lower in status than either of those pursuits.) Far from acting the defiant hero against the church, Copernicus was angling for papal acceptance and patronage.

But only somewhat. The pope at the time, Paul III, came from a wealthy noble family, had gone to school at the University of Pisa, and was known as a poet, a widely read scholar, and an aficionado of astronomy. (He also helped initiate the Roman Inquisition.) Copernicus could have easily placed himself in service to him; one astronomer who made favorable astrological predictions for Paul III (remember, astronomy and astrology were entangled at this time) was amply rewarded. Perhaps because he wanted to associate himself more closely with the church bureaucracy's humanist contingent, which didn't think highly of astrology, Copernicus did not follow this example.

Instead, his preface to *De revolutionibus* was an impressive concatenation of the sophisticated rhetorical devices (irony, confession, antithesis) and tropes (knowledge as a solitary voyage, an aesthetic of cosmic belongingness) then offered by humanist literature. The preface was meant to *persuade*, to subtly convince the pope as well as other readers of the veracity of a difficult and controversial argument—an argument that did not, and could not, proceed on the basis of evidence or reason alone. Copernicus embedded references to Horace in his text, a favorite author of the pope and other humanist scholars. He made subtle allusions in favor of the pope and against Martin Luther.

Even this more complicated version of Copernicus and his strategies is somewhat simplified. We recommend a reading of the full article by Robert Westman from which we've drawn this material. Westman concludes that Copernicus

> aimed to solicit reform sentiment from among those in the church who . . . valued the mixed mathematical disciplines but saw them as needing renewal through a return to a purified ancient tradition. . . . His reformist rhetoric was not stridently polemical; it was gently Horatian and

> Erasmian: an end to controversy among astronomers; an internal cadre of humanist mathematicians to reform church teaching on the heavens by providing true principles from which planetary order and calendrical accuracy could be restored, the entire enterprise to be legitimated by papal authority and by appeal to a broad range of ancient, pagan sources. The approach evokes Erasmus's broad reconciliation of Christian and pagan letters in a *philosophia Christi*—a life of lay piety modeled on the true life of Christ. . . .
>
> In *De revolutionibus*, Copernicus sought to bring the individual parts of the universe into concordance with a sun that he described to his ecclesiastical audience in the most classical, pagan images, not as the generative or emanative force of the Neoplatonists but rather as a properly placed lamp or lantern, an eye, a mind, an enthroned king, a visible god. His choice of language and imagery pointed the church away from mediative, astrological influences and instead returned it *ad fontes*, to an ancient truth: the primitive order of the creation.[11]

Copernicus *articulated*, in other words. He didn't simply theorize a new cosmology, he joined together old observations, traditional notions of order and symmetry, new humanist rhetoric and new physical-mathematical concepts, the pious life of a believer, and appeals to papal patronage and protections. It is of course still possible to say, as science purists would, that these are "outside" forces, incidental to the "real" science and mathematics that make up the bulk of *De revolutionibus*. But incidental to whom? They certainly weren't incidental to Copernicus, and we don't think they should be incidental to us. The Copernican achievement was a package deal, and even if his rhetorical strategies didn't mess up or bias the physical-mathematical system, neither are they superfluous. We need to understand rhetoric, the historical context, and the cultural and political scene to make better sense of the way the whole system of "truth" operates: what gives it its force, what makes it persuasive, and what makes it work.

Like scientists, historians are always returning to bump their heads against existing materials over and over, and each time they bring with them new interpretive skills, new data, new perspectives. If it was once difficult to craft a historical Copernicus who was anything other than the heroic, oppositional, pure scientist, we're now able to cast him differently, as expert muddler: an adept mixer of math and rhetoric, church reform and disciplinary reform, new truths and old, reform and tradition, change and conservatism, the radically new and the absolutely primitive. Like all scientists (and historians), Copernicus had to articulate a kind of sweeping narrative within which his science made sense. It was not the narrative of "vulgar triumphalism" so popular today among science purists, but of subtle, binding connections between religious, cultural, and scientific ideals which, taken together, would stabilize and authorize a social order that was both old and new.

Clarity as Illusion: It's Reigning DNA

Copernicus articulated a broad, overarching narrative as part of his work, but this is not the only way in which language is vital to the sciences. Language also works within the sciences at microlevels. Here we take another approach to the questions about saying things exactly and plainly—that is, without metaphor—that we started to explore with Bateson's dialogue.

The direct truths of the sciences, revealed through seemingly transparent technologies and methods, are supposed to be literal and self-evident, as plain as the nose on your face. This is why today, for scientists like Richard Dawkins who write popular books, the word "plain" has a place of particular honor and importance. "My own books have been both popularizations of material already familiar to scientists and original contributions to the field which have changed the way scientists think," Dawkins has told an interviewer, "albeit they haven't appeared in scientific journals or been languaged up with incomprehensible jargon. They've been written in terms that any intelligent person can understand."[12] The implication here is that science doesn't have to be hard or abstruse; not only can it be translated into plain language—or maybe "languaged down" would be more comprehensible—but translated such that the terms preserve their original, unequivocal meaning for *any* (intelligent) person.

But plain language says both more and less than what it seems to say, which is why it merits skepticism and close scrutiny. Asked by the same interviewer about Dawkins's work, computer scientist W. Daniel Hillis commented: "My only complaint about Dawkins is that he explains his ideas too clearly. People who read his books often walk away with an illusion of things being much simpler than they actually are. . . ."[13] Clarity, then, can be a form of illusion, and often comes at the price of necessary complexity. One of the complex things that Dawkins most frequently covers up—indeed, it is a covering up that amounts to perhaps his most significant contribution to the sciences—is the organism.

In his book *The Blind Watchmaker*, Dawkins spins an image that comes close to summing up his scientific viewpoint:

> It is raining DNA outside. . . . Up and down the canal, as far as my binoculars can reach, the water is white with floating cottony flecks. . . . The cotton wool is made mostly of cellulose, and it dwarfs the tiny capsule that contains the DNA, the genetic information. . . . It is the DNA that matters. The whole performance, cotton wool, catkins, tree and all, is in

> aid of one thing and one thing only, the spreading of DNA. This is not a metaphor, it is the plain truth. It couldn't be any plainer if it were raining floppy disks.[12]

This is quite the vision, a plain vision (assisted by presumably transparent binoculars) that sees behind the illusions where less well-trained eyes might linger. But literary theorist Richard Doyle sees something else going on in this passage. Doyle analyzes the ways in which language works (and doesn't work) within the life sciences today, and contends that "this vision hides as much as it reveals . . . the 'program' or floppy disk of DNA is not itself sufficient for life. The 'fluff' that Dawkins disparages in the name of 'plain truth' is more than a mere husk or tool; in its movement and 'performance,' it literally makes life possible. In a sense, it is nonsensical, or at least certainly not 'the plain truth,' to speak of the spread of DNA without remembering the spread of organisms."[13] One might just as sensibly say, "It is the 'fluff' that matters. All DNA coding and transcription is in aid of one thing and one thing only, the spreading of 'fluff.'"

So Dawkins says less than he should, but also more. In the name of plain speech and transparent truth, Dawkins musters a whole series of metaphors. He "systematically deploys metaphors while refusing them," as Doyle puts it. That contradictory impulse results from the fact that language within the sciences, if it is to be at all productive of thought, is inescapably metaphorical.

In this and other examples from his book *On Beyond Living*, Doyle gives us another language with which to articulate how the life sciences work, via both the "hardware" of experimental technologies *and* the "software" of language. Drawing on such computer metaphors is indicative of the strategies and necessities of both Doyle's project and the projects of the life sciences today: symbiotic, hybridizing, recombinant endeavors. Imagine a software program without the machinery of a computer—it's not very interesting or effective. Now imagine a DNA program without cellular machinery—it's not very interesting or effective. And now imagine the sciences without language and its metaphors—ditto. Finally, imagine a book about the life sciences that didn't make use of metaphors and other linguistic tools from other disciplines and social domains.

Doyle's book is not "plain speech," but a precise and demanding discourse appropriate to the complexity of the object it is analyzing: the life sciences and, indeed, "life" itself. It respects the fruitful and unavoidable contradictions inherent within the languages, practices, and theories of life scientists. Doyle is not exposing molecular biologists or sociobiologists as frauds; he is not "anti-life sciences." He is bringing his expertise in language to bear on the problem of what effects language itself has on and within the life sciences, particularly in an era in which "life" itself is figured in terms of a kind of language, from genetic codes to neurochemical messages. It's a pursuit he shares with a number of scientists as well, such as the biophysicist Henri Atlan:

> [T]he notion of the genetic program, originally a metaphor proposed by biologists to target new problems and define new research directions more than as an answer to the eternal questions about life, has become the constant refrain and crux of reflections about the innate and acquired, which are so many false problems derived from the fact that its metaphorical character has been so quickly forgotten; the "dogma" of molecular biology, so designated by molecular biologists in an attempt to be provocative and facetious, has indeed effectively become a dogma, in the vulgate of this discipline. The zenith of this line may have been reached by the theory of "selfish genes," abusively applied to extend sociobiology to political sociology. . . . What was only a joke. . . . became a serious description of how life is supposed to be in reality, beyond the appearance of living beings, their behavior, and their functions, as we experience these. In all these cases, scientific discourses were taken as new dogmas to be handed down religiously after being dissected out from the context of the works and discoveries that motivated them.[19]

While we wouldn't say, with Dawkins, that "it is raining DNA," there's no question that in scientific culture of the life sciences today, and in our culture more broadly, DNA reigns. We would say, with Atlan and Doyle, that understanding what metaphors do for the sciences can help demystify some of the power the gene currently holds in the public imagination. Such an understanding can also help scientists pursue better, less dogmatic sciences—and perhaps help Dawkins write better, less dogmatic popularizations.

We'll take a deeper plunge into the complexities of contemporary life sciences in a later chapter. There we'll see how the "discourse of gene action" was articulated within the research context that Atlan alluded to above, connecting up with new tools and techniques to transform many areas within biology. We'll also see how the "dogma of DNA" might be mutating into something less dogmatic, under the developmental pressures created by that very transformation. This brief look at life here has only scratched the surface of the ways in which metaphor and other rhetorical features of language are inseparable from the practices of the sciences—indeed, are inseparable from the power and beauty of science. Metaphors are not simply good or bad, productive or unproductive, but both simultaneously: they limit and unleash, constrain and generate. They flow not only from science to the larger culture, but from the larger culture to the world of science, and are equally vital to each sphere.

Metaphors can be attended to, questioned, and rearticulated, but never eliminated. Perhaps ironically, the purity of supposedly transparent, plain, unmediated truth leads to a kind of vulgarity, in the same sense in which we saw the "vulgar triumphalist" view of Copernicus at work. To lose sight of metaphors, to forget about the ways in which they both enable and constrain the sciences, is to fail to appreciate the real subtleties and complexities that pervade organisms, the sciences, history, and language alike.

Gender Bending

In the last twenty years or so, scholars developing a variety of "feminist analyses" of the sciences have traced the many ways in which gender metaphors have been articulated within and about the sciences. The scare-quotes are there to remind you that saying *exactly* what constitutes a good "feminist analysis" of the sciences is a precarious, contentious, and, in the end, a somewhat quixotic endeavor. Within this diversity of feminist analyses of the sciences there are many intellectual and political insights, as well as inevitable limitations and blind spots. The best of them, however, share a commitment to pursuing sciences that offer "a more adequate, richer, better account of a world, in order to live in it well and in critical, reflexive relation to our own as well as others' practices of domination and the unequal parts of privilege and oppression that make up all positions," in the words of one practitioner, Donna Haraway; the problem is "how to have *simultaneously* an account of radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own 'semiotic technologies' for making meanings, and a no-nonsense commitment to faithful accounts of a 'real' world, one that can be partially shared and friendly to earthwide projects of finite freedom, adequate material abundance, modest meaning in suffering, and limited happiness."[13] It's the problematic project we're calling "pursuing sciences" here, the challenge of muddling through reality.

Grand articulations involving rigid categories aren't very helpful to such endeavors. To argue that the sciences are not only a male enterprise (i.e. socially dominated by male practitioners), but a masculine one—the intellectual embodiment of a knowing, masculine subject which dominates and tortures a feminine nature, with disastrous ecological and social consequences—is not going to get us very far. Nor will reversing the privileged terms: Nature is a Woman, and because women are more organically connected to it, they constitute better "knowing subjects" employing a "feminine epistemology."

What will be more productive in the long run are those many feminist analyses that detail the historical, social, semiotic, and technical specificities that go into and out of particular episodes and endeavors in the sciences. One such area has been primatology, a field that provides powerful understandings, or articulations, about what it means to be human through the study of other members of the primate order.

MONKEYS IN THE MIDDLE

One of the most thorough, thoughtful, densely articulated, and popular books has been Donna Haraway's *Primate Visions*, a kaleidoscopic treatment of the kaleidoscopic production of this branch of science. In a zone of myriad intersections, it seems Haraway has explored them all: where the history of primatology has had to cross with racism, colonial projects, space exploration, popular culture, changing professional and disciplinary standards, shifting developments in biology, and many others. This book, and the rest of Haraway's work, is already having profound effects on the teaching and thinking of sciences today, and perhaps of all the books we refer to here most deserves to be called essential reading.

Haraway's work is a real contribution to the field of primatology, not despite, but precisely because of the fact that she incorporates historical, feminist, cultural, and political analyses into her writing and research. This would be true even if she didn't have a Ph.D. in biology from Yale. But the work of another, more conventionally credentialed practicing primatologist, Sarah Blaffer Hrdy, serves as a better example of how scientists themselves have a much more expansive view of what the sciences are than is traditionally thought or claimed.

Hrdy has identified herself as both a feminist and a sociobiologist, two words that are not generally found together. Sociobiology is most frequently associated with Harvard biologist E.O. Wilson, and readers might be familiar with the debates on sociobiology that began in the 1970s, debates both within the scientific community and around it. "Left-leaning" biologists like Wilson's Harvard colleagues Stephen Jay Gould and Richard Lewontin publicly criticized sociobiology, arguing that its emphasis on reproductive success (focused at the genetic level), its extrapolations from the behaviors of other primates (and other species altogether) to the behaviors of humans, and other features made its status as a science at best problematic, at worst a kind of pseudoscience. Moreover, such critics argued that sociobiology was shot through with "conservative" values: a valorization of competition, a deterministic (and pessimistic) view of human nature, a tacit legitimation of violence and male sexual domination. There is much merit to some of these arguments about sociobiology; a good guide to the articulations made among evolutionary theory, ethology, social theory, and political science is Philip Kitcher's book *Vaulting Ambition*. But there is also something to be gained by leaving these arguments aside, and zooming in to a tighter focus, where we discover Hrdy rearticulating the supposedly essential connections between "conservative values" and sociobiological theory.

Charles Darwin (whom we look at in more detail in the next chapter) had described in his 1871 book *The Descent of Man and Selection in Relation to Sex* what Hrdy calls the "partially true assumptions" that would shape this particular field of inquiry for the next century. "The males are almost always the wooers," Darwin wrote, and the females of the species were mainly restricted in their activities to choosing the best suitor of the bunch. Darwin didn't see himself as making assumptions, but as reporting the transparent facts: "It is shown by various facts, given hereafter, and by the results fairly attributable to sexual selection, that the female, though comparatively passive, generally exerts some choice and accepts one male in preference to the others."[18]

Is that a statement of fact? Is it a theory? It could be named a number of things, but in calling it an articulation, we suspend those questions and adopt a kind of ethnographic perspective on it. It was written by an eminent scientist, an expert (if not *the* expert at the time) in evolutionary theory, according to the rules for what would count as fact or theory in that context. Whether or not it was a solid fact or a good theory at the time, or whether we would judge it so now, this articulation was *powerful*. Hrdy renames this articulation "the myth of the coy female," which sets up a myth/science distinction that we want to suspend for now. But we can still follow Hrdy as she traces the descent of this myth (in her terms) or powerful articulation (in ours) through subsequent articulations of evolutionary theory.

The 1930s and 1940s saw the growth of what was later called the "evolutionary synthesis," in which Darwinian thought was combined with, reinterpreted in light of, and reworked with new theories from genetics, new statistical methods of analysis, and much new empirical data. A key paper in the field was published in 1948 by the plant geneticist Angus John Bateman, who in this case was working not with plants but with the fruit fly *Drosophila*, the research organism most important to the development of (animal) genetics.[19] Conducting a series of sixty-four experiments with these flies, Bateman found that "successful" males could produce three times the offspring that a "successful" female could. He concluded that male fruit flies could gain (in evolutionary terms, by simply having more offspring; in more sociobiological terms, by reproducing their genes) by mating again and again, while the female fruit flies had little to gain from multiple mating. (The language of "gain" in these kinds of articulations signals a muddling between evolutionary and economic theories, which will come up again in the next chapter.) Bateman's extrapolation from flies to all of nature made his 1948 publication the second most widely cited paper in the literature of sociobiology. "There is nearly always a combination of an undiscriminating eagerness in the males," he wrote, "and a discriminating passivity in the females. Even in a derived monogamous species (e.g. man) this sex difference might be expected to persist as a rule."[20] It didn't matter whether one was talking about female or male flies, female or male

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