SCIENCE, VALUES, AND THE PUBLIC

Heather E. Douglas, Editor

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FOREWORD

Some problems never go away. How we act on what we know is surely a problem that predates *Homo sapiens*, and over our long history one aspect of that problem has evolved into the vexed relationship between science and values, which has puzzled philosophers from the ancient Greeks on. In various manifestations, such as the is/ought problem or the fact/value distinction, it has been discussed by many writers, and has remained one of the most important philosophical problems we have. Indeed now that our powers of action in the world have grown so much, by way of the application of science as a set of tools, this area of thought is more important than ever.

But over the course of the twentieth century, philosophy became theory. This shift began with thinkers like Nietszche, Wittgenstein, and Gödel bringing into question the bases of philosophy itself, and it gained momentum in the second half of the century when an immense intellectual pressure was exerted on all received ways of thought, including language and cognition. Entire academic disciplines dissolved at their foundation under this intense interrogation of first principles, and fields like anthropology and history have had to struggle ever since to understand what they are and where their validity lies, if anywhere.

For the field called philosophy of science, the shift to theory manifested as what we now call science studies. What before had been regarded as a nearly independent history of ideas, based largely on mathematics and therefore almost metaphysical in nature, became radically historicized and situated in the particulars of the social contexts in which science arose and evolved. Science was now understood as a particular kind of praxis, meaning a political-social-economical-instrumental intervention into relations of power among human groups, and between humanity and the rest of Earth's biosphere. As such, science studies blossomed in a period of detailed phenomenological inquiry into a fractal array of historical circumstances that had never been properly investigated before. In this garden of forking paths, philosophy of science became much more history of science; using one of the last great models out of philosophy of science, Thomas Kuhn's paradigms, we might say that the old paradigm of science itself broke apart under the impact of theory's radical questioning of language, history, power, and cognition.

One result of this paradigm breakdown was that academic science studies became more and more technical and ingrown, such that only other practitioners of science studies could understand the context and import of new work. The academic field plunged down rabbit holes; it got lost in the weeds, such that its use value to working scientists and the general public, or let's just say everyone interested in science, which really ought to be everyone alive (there's that word ought again, but let's keep it), can no longer get much use out of the discipline. And yet the big problems of science and society remain, and are even growing more pressing as we move into an era of unprecedented scientific-technical powers and unprecedented ecological damage. More than ever we need a solid sense, if we can make one, of the relationship between facts and values, between our actions and our hopes. Maybe you could say we are between paradigms and need a new one, if this now old-fashioned formulation has any residual power to clarify things.

This is the situation I think Matthew Brown is taking on in his very interesting book. His is an attempt at paradigm construction by way of clarification, in a moment when clarity is both difficult and crucial. To achieve that clarity, he uses aspects of the scientific method itself, which in a philosophical discussion of science is recursive but also effective and pleasing. Among other aspects of the scientific method deployed here, we see a kind of structuralist description of the problem, de-stranding conglomerate realities in hopes of finding causes and effects; also reductionism, where the problem is contained to the point where it can be understood and discussed—even Occam's razor, by which I mean that Brown has decided to trust the language to convey commonly agreed-upon meanings.

This is crucial, because after the stupendous and no doubt useful work of linguistic deconstructionism, which turned the lens of philosophical inquiry onto language and cognition itself, one now has to work with a level of uncertainty concerning the level of discourse one wants to use to make the points one wants to make. Everyone now has to acknowledge that words are mysterious bags of allusions and connotations, completely contingent on their placement in sentences and in history. Fine, undeniable. But what then? In this moment, which used to be called postmodernism, and now is maybe better called the Anthropocene, choices have to be made about the level and mode of one's discourse. High modernism in literature, which was may be another kind of theory devoted to tightly focused case studies of subjectivity, was famously dense and obscure, difficult and challenging, a mode which supposedly reflected the actual nature of thought. In that model the common parlance of popular culture and ordinary people was seen as a degraded commercial product, thus a kind of false consciousness. This was the famous high/low split in modernist aesthetics, but what if it was wrong in its basic assumptions? In any case, in postmoderism, in the age of theory, that divide collapsed, and all aesthetic forms had the potential for artistic distinction, and any individual artist could choose any style from the past and do something interesting with it. This is one of the greatest (one of the only?) strengths of postmodernism as an aesthetic, as it represented an opening up of possibilities, acknowledging that different styles are appropriate to different purposes, with no hierarchy of better or worse applicable to the choices made.

How that has played out in science studies is harder for me to see. There are dense networks of technical literature, difficult to the point of being esoteric; there are also shorter and longer forms of popular nonfiction, explaining various aspects of science to the general public. These are the extremes, but there is also a realm in between the two, which is maybe just a way of saying that philosophy persists, despite all. And philosophy, to be effective in the world, has to be comprehensible. Sometimes it's appropriate to chase an idea into depths where only a few dozen fellow specialists can understand you. Other times it's appropriate to speak in registers that will reach the widest audience possible. And the more important the topic, the more important it is to communicate widely and effectively about it.

In this case, concerning the relationships between science and value, where the problems are central to the fate of human civilization and affect every person alive, it makes sense to try for clarity. It's a choice that has been made many times before in philosophy, and often when science is the subject of inquiry. Philosophers like William James, John Dewey, and Alfred North Whitehead made this choice, and their school of philosophy was called pragmatism partly as a result of that choice. Brown references them here, and his book is in their tradition. Science, value, and all these vast words that contain entire worlds in them, are here defined for their particular use in this particular text—the definitions are provisional; they constitute a kind of hypothesis or supposing in and of themselves. Then these words are used in a structured argument that is called out in advance, in the introductory material and the early chapters. It's as if we are being shown the architectural blueprint for a house before being walked through it. The walk-through then includes all the particular historical examples and the details of the case Brown is making, for this is a book that intends not just to clarify but to persuade. The book's explanatory notes give it the feel of a transcript of a lecture which includes the lecturer's added interpolations and clarifications; this is both aesthetically pleasing and easier to understand. The foregrounded structure of the argument is a rhetorical and aesthetic choice; there's a pleasure in seeing a clean line of thought, just as there is in a well-wrought stone wall or the nimble, swift surfing of a wave.

So this is indeed a pragmatic book, and as such it is made to be used. All scientists working in their various fields need to have a better philosophical grasp of the ramifications of their work, which is rapidly taking civilization into uncharted waters, both in human history and the history of the biosphere. Scientists need to become imaginative political actors at all levels of policy; they are going to have to better imagine both their values and the actions called out by those values. This book can help them with that part of their project, and that will make them better scientists. And then all citizens (and maybe we are all "citizen scientists" now) need to have a better understanding of the situations we face as a global society, living on a planet we are biologically damaging every day. What should we do? How can we deploy this amazingly powerful method we have invented, that we call science, to make ourselves and our descendants and our biosphere, which is to say our extended body, safer and happier? Bioethics, ecology, social planning, political economy, daily life—this book speaks to all these realms. This is perhaps the greatest virtue of Brown's project: he has gone right at one of the central problems of our time and faced it creatively and productively. In offering us a structure for comprehending our big mess better, he has performed an act of cognitive mapping that we can all put to use.

—Kim Stanley Robinson

PREFACE AND ACKNOWLEDGMENTS

In late 2017 the American Association for the Advancement of Science (AAAS) adopted a "Statement on Scientific Freedom & Responsibility": "Scientific freedom and scientific responsibility are essential to the advancement of human knowledge for the benefit of all. Scientific freedom is the freedom to engage in scientific inquiry, pursue and apply knowledge, and communicate openly. This freedom is inextricably linked to and must be exercised in accordance with scientific responsibility. Scientific responsibility is the duty to conduct and apply science with integrity, in the interest of humanity, in a spirit of stewardship for the environment, and with respect for human rights."[1] This statement is remarkable in that it links the freedom and integrity of science to larger responsibilities to humanity and beyond. Through this statement, a central organization not only in the national but in the global scientific community takes a stand that the practice of science is an *ethical* practice, one that is not aloof from, but must serve the interests of society, the environment, and human rights. As such, I take it as a strong stance against the myth that science is value-free, that its only duty is to objectivity and truth. Human, ethical, and social values must be a part of science.

But how far do the responsibilities of science to society extend? And how can scientists, who are experts in technical matters but not in ethics or values, fulfill their responsibilities? These are the central questions this book asks and answers. The book is informed especially by an ongoing discussion in the field of philosophy of science about the role of values in science, a discussion that has been with us since the beginning of that field, though it has waxed and waned over time. In the early twentieth-century development of the field, the philosophers arguing for a role for values in science or the need for science to serve society were pragmatists and Marxists. The topic waned briefly at mid-century, but in the last decades of the twentieth century it was forcefully revived by feminist philosophers of science. At the turn of the twenty-first century another important thread, focused on public policy and regulatory science, especially environmental and biomedical, entered the discussion. This book has benefited from the current renascence of engagement and creative activity that draws on all three of these threads.

There are many people whose contribution to the work in this book I should acknowledge. First and foremost, Sabrina Starnaman, not only my strongest supporter and closest companion, but a scholar whose sense of the need to make scholarship actively serve the cause of justice is a constant inspiration. Between us, she is the true pragmatist. Her work, her life, and her support inspire me.

There are many philosophers who have influenced my thinking, who, despite my attempt at judicious citation and discussion, no doubt had a greater influence on this book than is communicated in the manuscript. First, my ideas would not be what they are without a long intellectual engagement with Heather Douglas. I have benefited both from her excellent writings exploring these topics as well as her warm personal support and friendship. Heather is in a large part responsible for the current liveliness of discussions of values in science. Second, in many respects, the book builds on the work of Elizabeth Anderson, who is both an excellent interpreter of the moral and political philosophy of John Dewey and a foundational pragmatist-feminist philosopher of science. Much of what I try to do here is one way of working out ideas about the role of values in science and the influence of science on values that I first encountered in her work. Finally, Janet Kourany has likewise been a friend, supporter, and interlocutor from whom I have learned much and sharpened my own thinking about these topics.

Practically speaking, the bulk of the work on this manuscript was completed due to the kind support of the University of Texas at Dallas in the form of a year's sabbatical coincident with a visiting fellowship at the Center for Philosophy of Science at the University of Pittsburgh. Many thanks to the director of the Center during my fellowship, Edouard Machery, to the many faculty whom I had the pleasure to interact with, including Sandra Mitchell, Peter Machamer, John Norton, and Nicholas Rescher, and to the other fellows who provided feedback on two of my chapters as part of the weekly reading group and from whom I constantly learned new things and enjoyed great companionship, including Kareem Khalifa, Viorel Pâslaru, Anjan Chakravartty, Sharon Crasnow, Greg Frost-Arnold, Tobias Henschen, Yann Benetreau-Dupin, Alison Fernandes, Katie Kendig, Mikael Cozic, and Daniele Muttini. I also had the pleasure of interacting with some of the fantastic graduate students from the History and Philosophy of Science and Philosophy departments, including Zina Ward, Nora Boyd, David Colaço, Siska De Baerdemaeker, Haixin Dang, Joshua Eisenthal, Kathleen Creel, and Jennifer Whyte. In particular, in Q&A after an early talk on one element of the book, followed by email correspondence, Zina Ward pushed me on elements of my argument that helped me improve them. Thanks also to the attendees at the several conferences and workshops that took place during my time at the Center.

Three people read the first complete draft of the manuscript and provided extensive and valuable feedback. Kevin Elliott especially helped me think more carefully about the structural relationship between inquiry, value judgment, and contingency that is central to the book's argument. I benefited from both face-to-face discussion with and detailed notes from Dan Hicks. His feedback allowed me to make many points clearer and more compelling. Among other things, Dan pushed me where he saw my views as insufficiently political, as tending toward the value-neutral (rather than the value-free). I suspect we will find that we disagree on some aspects of that, but I have benefited immeasurably from engaging with his comments. Last, I shared draft chapters with the members of my graduate seminar on Science in Values at the University of Texas at Dallas (UT Dallas) in the fall semester of 2017, and Natacha Guyot of her own accord provided extensive, helpful written feedback on the manuscript. The other members of the seminar, whose questions and discussion of the ideas and arguments in the manuscript were also quite valuable, were Sara Cardona, David Lyons, Rick Townsend, Alan Alanis, and Aaron Stewart.

I am deeply grateful to Heather Douglas and Justin Biddle for reading a later draft of the manuscript and providing extensive and helpful feedback, from helping me resolve large-scale conceptual problems in the argument, all the way down to grammatical problems. I am also thankful to Abby Collier, my editor at University of Pittsburgh Press, who provided helpful feedback on the manuscript and enthusiastically shepherded my work through the publication process, making it a better book.

I am very grateful to my friend and collaborator Joyce Havstad, with whom I have worked on a variety of issues in science and values and science and policy, especially connected to climate science and climate policy. Much of my thinking has been improved thanks to discussions with her, and she has especially helped me think through different ways of understanding the argument from inductive risk. Likewise, I owe a debt to my collaborators at UT Dallas on an extended project about ethics and values in engineering and engineering education, particularly Nicholas Gans, Magdalena Grohman, Eun Ah Lee, and Marco Tacca. While engineering is not the focus of this book (though it is included in my broad definition of "science"), our project has helped me think about the role of ethics and values in the contingencies that arise in research, and how to make decisions in those contexts responsibly.

The broader community working on values in science and socially relevant philosophy of science is one of the best, most supportive parts of academia to work in, and my thanks to the many people who make that area great. I have had the honor to host the Values in Medicine, Science, and Technology (VMST) annual conference through my position as director of the Center for Values at UT Dallas. My thanks to Dennis Kratz for founding and supporting the Center and to Madga Grohman, associate director of the Center, who is the one who really makes things happen. I have also enjoyed and benefited from the opportunity to learn from the many speakers who have agreed to participate in the Center's annual lecture series. I have also had the pleasure of participating in the meetings and serve on the board of the Consortium for Socially Relevant Philosophy of/in Science and Engineering (SRPoiSE). My thanks to all of the many guests of the Center for Values, VMST Conference attendees, SRPoiSE organizers, and SRPoiSE meeting attendees, especially Sarah Wieten, Ian James Kidd, Mark Tschaepe, Sean Valles, Carla Fehr, Katie Plaisance, Roberta Millstein, Eric Martin, Catherine Womack, and several others already mentioned. My thanks to the local members who have joined the Values in Science Research Lab that I've organized in the Center for Values, including Fred Grinnell, Pam Gossin, Karen de Olivares, Richard Scotch, Luna Allen, Mitchell Owens, Ahmad Askarian, Mehri Mirzaei Rafe, Justin Bensinger, Thomas Rocha, Rebekka Michaelsen, and Elizabeth Escalante.

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1. "AAAS Statement on Scientific Freedom and Responsibility," *Science* 358, no. 6362 (2017), http://science.sciencemag.org/content/358/6362/462.2. As of fall 2018 I am currently serving as a member of the AAAS Committee on Scientific Freedom & Responsibility, which wrote the statement and won the support of the AAAS Board of Directors. The statement was adopted before my involvement with the committee.

INTRODUCTION

> We are complex and intelligent creatures and we can hold multiple ideas in our heads at the same time. We can be critical of the things that we love.
>
> **—Anita Sarkeesian**

THREE CASES OF THE INTERPLAY OF VALUES AND SCIENCE

Science and values mutually influence each other. Values are implicated in scientific knowledge and practice. Science helps us to understand our values; its progress alters our values. I argue that the influence of values on science is pervasive and that science also can and should have an influence on our values. I argue further that this interplay must be guided by accounts of scientific inquiry and of value judgment that are sensitive to the complexities of their interaction in practice. Scientists and moralists, as well as philosophers of science and ethicists, have often presented distorted and even harmful pictures of science and of values for lack of nuance about their interplay.

This book is unabashedly normative, where *normative* means making claims about what *ought* to be and guiding our *evaluation* of the quality and worth of certain things. An argument could be made that, historically, sociologically, and psychologically speaking, science has been influenced by the values held by scientists and by the society in which science is embedded.[1] Likewise, one could argue that, as a matter of fact, our beliefs about values, our norms, our mores, our culture have changed in part as a result of scientific progress. Both of these arguments could be understood as merely descriptive, leaving untouched our ideals about how science ought to work and about where moral truths come from. By contrast, this book directly challenges the views that science ought to be value-free and that values ought to be evidence-free, independent of science. Though I will challenge the very idea of a "merely descriptive" argument, and thus the fundamental nature of the descriptive/normative distinction, I do not shy away from making normative claims. This book provides normative arguments about how we ought to evaluate episodes and decisions in science as to the way they incorporate values, as well as providing guidance to scientific practitioners and institutions on how they should incorporate value judgments into their work. As such, it seeks to revise our understanding of how science ought to work.

To introduce the kind of ideas that structure this book, I will start by briefly telling the story of three cases where values have played an important role in science. The first is the long history of scientific racism, the second is a specific early example of feminist psychology, and the third concerns embryonic and adult stem cell research. In the rest of this Introduction, I will describe the book's basic presuppositions and philosophical orientation, give an overview of the argument, and explain the general structure of the book and the ways it can be read.

Scientific Racism

The history of modern ideas of "race" is intertwined with the history of scientific racism; the emergence of each of the human sciences is tied up with emergence of modern ideas about race.[2] Starting with natural historians and philosophers as early as the sixteenth century, the modern concept of race was developed to explain the superficially obvious differences between human geographical populations and to justify the racist atrocities that Europeans began to instigate throughout the world starting in the fifteenth century. While some argued that racial differences were merely superficial and environmentally caused, many others insisted that racial differences included deep differences in capacities, including mental abilities, and that the differences were biologically determined.

In the nineteenth century, concepts and theories of race were further developed by physical anthropologists and evolutionary biologists. Pre-Darwinian scientists like Samuel Morton and Louis Agassiz made extensive physiological and anthropological comparisons of members of different races in order to argue that the races were different, hierarchically ordered species. Many Darwinians and social Darwinists like Herbert Spencer used the theory of natural selection as a mechanism to justify the racist ideology of biological determinism. (Darwin himself, who certainly did not completely escape the racism of his time, does seem to have largely opposed a biological determinist view of racial differences.)

In the early twentieth century, with the emergence of the new scientific psychology, came attempts to measure the differences in mental ability between races that had been posited by earlier thinkers and defended by Morton on physiological grounds. A variety of psychophysical, behavioral, and cognitive tests were developed in the early days of psychology, the most (in)famous of which was the intelligence quotient (IQ). When IQ tests became common in the early twentieth century, they were soon added to the repertoire of ways that scientific racism attempted to establish the innate hierarchy of the races. Ironically, the creator of the IQ test, Alfred Binet, did not believe the test measured a heritable trait, or even a single property that could be called "general intelligence." But the essentialist, biological determinist reading of IQ grew in popularity as use of the test became widespread, especially in America.

By and large this history of thinking on race reinforced status quo racism and white supremacy by making it seem natural or inevitable. While some did defend racist and paternalist policies on a cultural/environmental view of racial differences, historically biological determinism has been more commonly linked to such policies. Today it is relatively easy to see the fallacies and biases behind such research, and there have been several prominent analyses. Yet the research in its time was well regarded and considered of high quality, and such research reappears regularly in the press, despite the fact that it is invariably shown to be of poor quality.

Stephen Jay Gould's *The Mismeasure of Man* provides a classic example of racist values leading to low-quality science in the case of Samuel George Morton.[3] Morton was an early physical anthropologist who is most well known for his collection and study of human skulls. He measured the cranial capacities of the skulls from people of different racial groups, taken by Morton and his contemporaries as an indirect measure of intellectual ability. Gould shows how Morton's (run-of-the-mill nineteenth-century) racist values influenced his work, leading Morton to literally *mismeasure* the skulls in his collection in order to confirm his racist views about different racial groups.[4] While there has been some criticism of Gould in defense of Morton,[5] Gould was essentially correct in his analysis of Morton's biases, despite some errors.[6] Moreover, in his reanalysis and critique, Gould seems to have tacitly accepted a variety of problematic assumptions, without questioning them, that Morton made about there being a meaningful answer to questions about the average cranial capacities of racial groups, including major sampling and conceptual problems.[7] The whole project of finding such racial differences is problematic, not just Morton's biased implementation of the project.

Nothing about the processes of science as they exist prevents biases like racism from being reinforced. Indeed, science is a relatively conservative institution that often reinforces the status quo, not because it contains big-C "Conservative" political values (many scientists are liberal),[8] but because science works on a system of peer review in which established experts vet the work of less-established members. In addition, scientific careers are still difficult to access for those with less social privilege, and in the past they were completely closed to all but white men of means. Furthermore, as the relevant sciences were all intertwined with racist ideologies from the beginning, overcoming them is a long-term process, still incomplete.

Science need not, and does not always, problematically reinforce the status quo. Science has the capacity to self-correct, but only when scientists and society carefully foster that capacity. Antiracist and egalitarian values, used appropriately, have helped debunk bad science and led to better methods and results across a variety of fields in the human sciences. Gould made clear his values in writing *The Mismeasure of Man*, citing his personal experience in the civil rights movement and arguing that "we have a much better chance of accomplishing something significant when we follow our passionate interests and work in areas of deepest personal meaning."[9]

Feminist Psychology

Patriarchy and feminism play much the same sort of roles in science as white supremacy and antiracism. Starting in the 1970s, the feminist movement had a significant impact on science, on philosophy of science, and on science studies more broadly. One interesting and much earlier episode comes from the work of three collaborators: William Moulton Marston, Elizabeth Holloway Marston, and Olive Byrne.[10] The three made important contributions to scientific psychology from 1915 to 1931, to popular psychology in the 1930s, and to pop culture in the 1940s. Holloway, Byrne, and Marston invented the systolic blood pressure lie detector (a component of the modern polygraph) and wrote widely on emotions, consciousness, and the relation of psychology and neurology. They did work that anticipated the positive psychology movement decades later. After an academic career cut short by social prejudice toward their unconventional lifestyle, Holloway, Byrne, and Marston went on to create, write, and popularize the comic book superhero Wonder Woman (who was often a mouthpiece for their psychological theories).

Holloway, Byrne, and Marston were convinced that the status quo of their time was deeply unjust *and* psycho-emotionally unhealthy. This was a judgment based on an engagement with major feminist political writers and movements, on scientific experiments and clinical observations, and on the personal experience of living a marginalized lifestyle. Near the beginning of *Emotions of Normal People*, Holloway, Byrne, and Marston make this striking claim: "I submit that the backbone of literature has been transplanted intact into psychology, where it has proved pitifully inadequate."[11] "The backbone of literature" is their colorful phrase for referring to commonsense or folk categories, which they understood most contemporary psychologists to merely take for granted. This "transplant" job tended to reinforce the social status quo as natural and scientifically justified. They wrote apt criticisms of the psychoanalytic and behaviorist systems of psychology, in part based on this problem of taking social categories for granted as real mental kinds. Contemporary feminist psychologists continue to break down sexist assumptions in psychology, neuroscience, and society.[12]

Holloway, Byrne, and Marston sought to provide a radically revisionary psychological theory that dispensed with unhealthy and unjust social relations. They forwarded an account of psycho-emotional health or "emotional normalcy" based on the promotion of "normal" emotions and relations between emotional states. They had a revisionary theory of the basic (or "primary") emotions based on neuroscientific ideas, which they termed "dominance," "compliance," "inducement," and "submission." These four basic emotions and their compounds tended to fall under the categories of *appetite* (dominance, compliance) or *love* (inducement, submission). For Holloway, Byrne, and Marston, the love emotions were primary, and relationships of "love leadership" would govern a healthy society. Women, due to their innate superiority with respect to love emotions, were better fit to be love leaders. On this ground, they defended more and less radical feminist social reforms, from equal rights, education, and economic independence of women to eventual gynocentric matriarchy. Contemporary feminist psychologists tend to reject and criticize essentialist ideas about gender difference, including emotional differences, instead forwarding accounts where gender differences are culturally conditioned and socially constructed. Holloway, Byrne, and Marston are, however, part of a long if minority feminist view that emphasizes essential differences.[13]

One potential concern is that the sociopolitical motivations behind their work were generally not presented in a straightforward way. They did not argue, for example, that Freud's work was problematic because it was sexist, nor did they make clear their values in their scientific work. Radical value judgments are presented, if at all, as conclusions, not assumptions, of the scientific research. In one way, this was a good thing: in many cases, they were able to provide compelling arguments on value-neutral grounds, in much the same way that Gould criticized scientific racists not merely for being racist, but on the basis of methodological, empirical, and technical errors in their work. For rhetorical purposes this approach is common and often more effective, but the lack of transparency is somewhat problematic.[14] One wonders what decisions lay behind the empirical results they presented.

The academic argument later became an activist project. This, too, was problematic. Interventions as diverse as legal advice, self-help writings, clinical psychology, and creative fiction in popular media are all based on scientific views far from widely accepted in their scientific community, as well as on values that were quite rare in their time. Their psychological views are presented as expert knowledge but were often quite idiosyncratic. Of Wonder Woman, William Moulton Marston once wrote, “Frankly, Wonder Woman is psychological propaganda for the new type of woman who should, I believe, rule the world.”[15] One can respect Holloway, Byrne, and Marston for attempting to use their scientific work to have a beneficial impact on society without completely admiring their propagandistic approach.

Feminist science is largely another case of values having a beneficial influence on science, but we can see here that there are better and worse ways to do it. Ideally we would like to have a world where scientists are transparent about their values and their influence over their decisions, though misconceptions about the relationship between values and science often make hiding value commitments more rhetorically effective.[16] Likewise, finding ways to use science to the benefit of society is highly desirable, but using propagandistic techniques to forward idiosyncratic and uncertified views is problematic.

Stem Cell Research

Research on human embryos has been a hot-button political and ethical issue for decades. Today, the controversy is on CRISPR gene editing of human embryos,[17] but for years the central controversy concerned human embryonic stem cells. In the United States bans on using federal funds for research on human embryos go back to shortly after the legalization of abortion in 1973. Some moves were made toward lifting the ban and authorizing the use of federal funds during Bill Clinton's presidential administration, but the blocks were never fully removed. The greatest controversy over the issue came during George W. Bush's administration. Bush was ironically considered the greatest opponent of embryonic stem cell research, despite the fact that he actually authorized the first federal funding for research on nineteen embryonic stem cell lines. Nevertheless, many restrictions remained in place and more were added, and most embryonic stem cell research had to seek private funding. The second major liberalization of funding for stem cell research came with Barack Obama's executive order of March 9, 2009.[18]

The motivation for restricting research on human embryos is clearly a matter of religious and ethical values. The question concerns what is and is not permissible to do to an embryo, and support for banning such research primarily came from the right-wing Christian religious groups that command significant political power in US politics. The values in question are controversial—and some would argue inappropriate—grounds for public policy in a pluralistic, secular democracy. Whether or not you agree with the policy, there is no ground for calling it "antiscientific," as many supporters of such research have done. Ethical restrictions on research because of impact on human subjects, animal subjects, or the environment are common and today are considered unremarkable. What counts as a morally considerable subject and what is permissible to do to that subject may depend on scientific information, but are straightforwardly questions of ethics and values.

As a result of the funding environment in the United States from 1973 to 2009, there was limited funding for exploratory research on stem cells. Such funding is largely provided by the federal government, as private funders prefer to support research that is more clearly and immediately commercially viable, and charitable and state funding is in relatively shorter supply. While there was never an outright ban on embryonic stem cell research, it was no doubt slowed considerably by the funding bans.

One unanticipated result of the funding restrictions was innovation in the area of *adult* stem cell research and the development of induced pluripotent stem cells. The restrictions spurred the imagination of some researchers who went on to develop techniques for deriving stem cells without involving embryos. Constraint is a spur to creativity; the values-based limitations on funding spurred scientific innovation and progress. Though adult stem cells are less versatile, they also have their own virtues. For example, transplanting tissues grown from one's own stem cells has little risk of rejection, which is not the case for tissues grown from embryos. It is doubtful that as much progress would have been made as fast in the United States on adult induced stem cells without the ban in place.[19]

Values need not only be a hindrance to science, even when they create constraints and limitations on what science can do, and even when we disagree with the values or how they were applied. The silver lining in the stem cell case shows that values can interact with the imagination to push science in beneficial new directions.

THE PHILOSOPHICAL ORIENTATION OF THE BOOK

While this book aims to give generally accessible arguments for the views it lays out and to engage closely with previous ideas about values in science, inevitably it is shaped by my own philosophical orientation and personal perspectives. I believe it is helpful and somewhat more honest to lay bare my personal commitments and assumptions. While I believe each viewpoint is defensible and well defended insofar as it has an impact on the book, none is entirely uncontroversial, and it will help you as a reader to know ahead of time where I am coming from.

Normative Pragmatism

Normative arguments are central to this book; the goal of the book is to guide scientists and to inform our evaluation of science, particularly with respect to the ethical responsibilities of science. The general philosophical viewpoint of the book is normative pragmatism, in two senses. The first is that its approach to normativity is *pragmatic*. This means that the norms are engaged with practice, and ultimately evaluated by their impact on the practice. All normative claims are ultimately claims about how we should act, and nothing prior to actual action—no a priori arguments, no nonnatural facts, no process of value judgment—can ultimately determine the truth of such claims. Only the impact on action and practice consequent to adopting a value judgment can be the test.

The second is that *pragmatism* itself is taken as a normative framework for scientific practice and value judgment. Thus scientific inquiry is conceived as properly practical inquiry, and the theory of values is considered according to a framework of pragmatic pluralism. I do not claim that pragmatism adequately captures the folk understanding of knowledge or truth, nor that it best explains what scientists are thinking about or trying to do when they engage in scientific practice. Nor do I claim that folk conceptions of ethics, or conceptual analysis of folk beliefs about values, will deliver a pragmatic pluralist theory of values and value judgment. Rather, I claim that our practices and beliefs should be revised to be more pragmatist, because pragmatism is the best normative framework for science and ethics. In concert with the first sense of "normative pragmatism," I think this second sense is justified: (1) by the recurrent problems that arise in current scientific practice as well as in accounts of science and of values (and especially of their interaction), and (2) that the ultimate test of the claim is the improvement of scientific and ethical practice.

The normative pragmatist approach is consistent with and supportive of two growing trends in philosophy of science and ethics, respectively. In philosophy of science, it is the best framework for bringing to fruition the increasing focus on and responsiveness to scientific practice, without allowing philosophy of science to collapse into a merely descriptive enterprise. In ethics, the increasing focus on the complexities of our moral lives and frameworks of practical ethical deliberation over foundational, principle-based moral theorizing is best accommodated by a pragmatist, pluralist theory of values. Many, though not all, of the elements of the argument in this book are independent of the pragmatist theory of inquiry and the pragmatic pluralist theory of values. Nevertheless, the latter two theories are the best way to fulfill the ambitions behind these current trends and provide the most robust normative ideal for the interplay of values and science.

Moral Imagination

A central concept at the heart of the positive recommendations of this book is that of "moral imagination." Moral imagination plays a central role in the theory of value judgment laid out in Chapter 5, and thus a central role in the ideal for values in science laid out in Chapter 6. Moral imagination means a few different things, each of which plays a role in the book. In one sense, moral imagination is about the role of imaginative and creative thinking in ethics and value judgment. Our capacities for empathy and compassion depend on our understanding of the perspectives, feelings, and values of others, and are thus acts of imagination. Likewise, integrating values through creative thinking about moral problems is an important element of ethics that is undervalued and sometimes positively undermined by the philosophical literature, especially its focus on clear-cut dilemmas. In another sense, moral imagination represents a special constraint on our decision making: we should judge our actions in part by thinking expansively about their implications and consequences beyond the here and now, beyond our inner circle, and these considerations require imagination.

The third sense of moral imagination has to do with the formation of our ends and ideals. In my view, our highest ethical and social calling is to create new ends or goals and to strive for more complex values and a more intentional life, not to live habitually, unthinkingly, or for some purpose conceived remotely from ourselves. The horizon of our ethical life should not be the way things are now; we should imagine ways the world could be better, should be better, in light of the problems we face now. Our current situation is a starting point, not a destiny.

We cannot fulfill this calling alone; our ethics must be a democratic, social ethics. As Jane Addams wrote:

> If in a democratic country nothing can be permanently achieved save through the masses of the people, it will be impossible to establish a higher political life than the people themselves crave; that it is difficult to see how the notion of a higher civic life can be fostered

> save through common intercourse; that the blessings which we associate with a life of refinement and cultivation can be made universal and must be made universal if they are to be permanent; that the good we secure for ourselves is precarious and uncertain, is floating in mid-air, until it is secured for all of us and incorporated into our common life.[20]

No good can be adequately chosen for us from without. As the slogan goes, "Nothing about us, without us." Though ends do not become worthy *merely* by being chosen by us, no end can be entirely worthy of us unless we choose it freely and intelligently, rather than having it imposed upon us.

What we need is an ideal for values in science that is not concerned with merely policing a minimum boundary of acceptable conduct, nor a concessive realpolitik, but an ideal that guides us to strive for a better world. Minimal bounds must sometimes be outlined and policed when we're in real danger of violating them, but focusing on minimal criteria can also be counterproductive insofar as it leads us to think of all ethics as a negative force, a restriction rather than a higher target to aim for. Realpolitik has a role to play in the short-term assessment of means to ends; it has no place in the determination of ends. There is a strong anti-idealism in certain quarters of philosophy of science and practical ethics today, which justifies itself in a mistaken reference to being realistic and practical. But there's nothing unreal about the ability of ideals properly formed to guide us toward improving the world, and there is nothing less practical than allowing bad actors and unjust systems to limit your hopes and your aspirations.

Pervasiveness of Evaluation and the Contingency of Science

In my view, evaluation is a pervasive feature of intelligent practices generally, and scientific inquiry particularly. This word, *evaluation*, carries a lot of freight. It means both making a judgment about something and determining the worth of something. Judgments are not mechanical but, as in a "judgment call," require the careful exercise of intelligence, wisdom, and wit; still it is often the case that equally wise experts judge the same case differently. This suggests open options, a contingency to the direction of evaluations. Making a judgment call generally requires determining the relative worth of the options to the situation at hand. If we are making a decision about how to act, the worthiness of the actions (their meaning and their consequences) is what we judge. If we are deciding between theories, their worthiness to explain, predict, or control the phenomena in question is perhaps foremost.

Science is hard, requiring determination, creativity, and luck; it cannot be reduced to a set of rules. Also, there are many potential paths to success in science. Some scientists move piecemeal and conservatively; others make wild leaps and suggest radical changes. Sometimes novel discoveries depend on opportunities that arise—right place, right time; others depend on whether the right confluence of training, techniques, ideas, and technologies are available to make the leap—the right person or tool for the job. For all these reasons the direction of science is highly contingent.[21] As such, evaluation, or judgment, is necessary at many steps along the way. Any account of science must wrangle with these features of scientific practice.

Avoiding Extreme Optimism or Pessimism

My introduction to the philosophy of science came through William James, Thomas Kuhn, and Paul Feyerabend. As a result, I am highly skeptical of Pollyanna theories of science as everywhere rational, comprehensive, cumulative, and authoritative. The authority, objectivity, and beneficence of science have always been an open question for me, and I think careful research on the history and nature of scientific practice shows that it really is a mixed bag. There are incredible successes and feats of staggering genius. There are also examples of rank bias, exploitation, skullduggery, and obvious mistakes. The negatives are not particularly more prevalent in science than in any other human endeavor, particularly those endeavors that are the traditional province of the privileged, as science is and has been.

On the other hand, I have always been fascinated by science and technology, and I acknowledge that it is easy to take skepticism about science too far. It is not plausible to hold that science is *inherently* sexist and racist (even if most of its institutions have been), that it is mere politics (power struggle and clash of opinion), that it has no epistemic authority of its own. Again, it seems to me that careful research on the history and nature of science shows that something special has happened on the historical occasions when the active, experimental methods of knowledge production and the speculative, theoretical methods of knowledge production work together. Each has a long history of separate development (as the active, experimental tradition of the artisan and technician and the speculative, theoretical tradition of the mathematician and philosopher, respectively) in many cultures. Their particular combination is more historically rare and is what makes modern science so productive.

There is a difference between having a critical attitude toward science and a skeptical one. Skepticism about science denies wholesale the very possibility of science generating knowledge. I recommend and try to teach my students how to have a critical attitude toward science. To uncritically accept every bit of scientific information would be foolish, as is the wholesale skeptical rejection of science common in certain segments of modern society.[22] It is not as difficult as many think for the well-equipped layperson to evaluate science, to tell the difference between the novel results in a single study and something established by a large literature, to recognize potential conflicts of interest and sources of bias, and to identify failures to check potential harms to society. It takes work, but it is not beyond the grasp of most. I find providing the tools for such evaluations much more satisfying than providing a partisan defense of (or attack on) science.

The Unity of Science, Engineering, and Medical Research

For some purposes we may want to distinguish science proper (or "pure" science) from engineering and medical research (or "applied" science). For example, we may want to reserve a certain percentage of funding for "basic research" that has no obvious or immediate application to technology, medicine, or policy, based on our sense of past successes of such research or its intrinsic worth, especially in an environment where such research is undervalued by granting agencies.[23] For the purposes of this book, namely understanding the general nature of scientific inquiry, the ethical responsibilities of scientists, and the impact of science on society, there are no significant differences among the three.[24] Likewise I see no significant differences between natural and social sciences with respect to these questions. Of course the different sciences have different subject matters, different relations to society, and different values relevant to their inquiries. As such, when writing in general about "science" or "research," you should know that I have all of these things in mind. *Science* throughout the book can generally be read as shorthand for "STEM" or "natural and social science, technology, engineering, and biomedical research," and "scientists" for "STEM researchers" or "scientists and engineers."[25]

A Heuristic Focus on the Individual and Small Groups

I will present many of the ideas and arguments in this book, at least at first, from the point of view of the individual scientist in the midst of research, or from the small-scale research collaboration. This is not because I think science or scientific knowledge is fundamentally individualistic, nor because I think the influence of society is irrelevant or can safely be ignored. Rather, my reason for presenting things in this way serves three related, heuristic purposes.

First, I think one place where we really need guidance, where there is a large gap between the way things ought to be and the way things are, is the individual level. Individual scientists and small groups in the lab have a great degree of power over the shape of the scientific process. While the larger social processes of peer review, funding, extended controversies and their settlement in the scientific community, and the codifying of knowledge for application, textbooks, and so on, are also extremely important, many important decisions take place within the research process itself, which is governed mainly by individuals and small groups; unlike their results, those decisions are often not open to scrutiny of the scientific community. Science involves a lot of trust—we trust researchers to report their results honestly and accurately, to follow the protocols that have been approved for their use of research subjects and sensitive materials, to evaluate the work of other scientists on the merits. We trust experts to give us an accurate representation of the state of scientific knowledge. Social checks and balances themselves are not enough if the conduct of scientists is not responsible. Yet the guidance we provide to science on what it means to be responsible is woefully narrow and inadequate.

Second, I follow thinkers like Ron Giere and Nancy Nersessian in thinking that the larger social processes can be treated as cognitive processes and that there is a unified framework for describing the work of the individual thinker and for describing groups, even large groups, thinking together.[26] As such, I think it is possible to read the individualistic-sounding language of *choice*, *decision making*, and *judgment* literally even when the processes in question cannot in principle be done by an individual, but are the product of the whole scientific community.

Third, there are many issues of values, ethics, and politics which appear at the larger social level that are intentionally outside the scope of this book. For instance, the commercialization of science has huge impacts on the larger workings of science today, impacts that are largely negative and have led to unreliability and fraud in whole areas of research, especially certain areas of biomedical, environmental, agricultural, and nutritional research. There are practical limits to what individuals can do here. The recommendations in this book may help individuals make better decisions in the face of the problematic incentives created by commercialization, but they are admittedly insufficient to resolving the problem. Also there are large-scale religious, conservative, and populist attacks on the authority of science that are incredibly difficult to fight, and focusing on those attacks has led to reactionary responses that distort our understanding of science. Frankly, I am not only at a loss personally to provide useful guidance on these issues, I am not optimistic that they can be addressed at all without significant social, cultural, and political-economic change. Thus I focus on the level where I think we can make some real progress in ameliorating science and its impact in the midstream of the research process.

THE ARGUMENT OF THE BOOK

Contingency and choice are ubiquitous throughout the research process. Scientists, engineers, and biomedical researchers face choices of what to investigate and how to investigate it, what methods to use, what hypothesis to test, how to model phenomena, what data to collect, when to stop data collection, and what conclusions to draw based on the evidence. Peer reviewers for funding bodies decide to fund this grant application and reject that one. Committees decide to hire or tenure this scientist but not that one. Likewise, institutions have evolved in one direction but could have evolved in another; individual researchers have certain levels of talent and skill that could have been otherwise; sometimes researchers are in the right place at the right time, but other times they are not. Many of these contingencies are out of the control of individual choices, but others are matters of explicit decisions, and many things that are decided by habit, luck, or institutional practice could be made explicit and decided differently.

On what basis are scientists to decide what to do in the face of these contingencies and choices? Some would say that they must be decided *objectively*, by the evidence, by logic and statistics, by scientific standards (sometimes called “epistemic values”) such as simplicity or Okham’s razor. But right away, we can see that this answer is inadequate for many scientific questions, such as which question out of the infinity of possible questions we should study, or what methods are ethical and humane to use on animal or human subjects. In order to make these decisions, we must also consider our values, what we care about, our goals, ethics, duty, responsibility, what is right and good.

This book argues that few, if any, of the decisions scientists face can, in principle, be decided by logic and evidence alone. Nor are epistemic standards sufficient. Even if those decisions could be settled that way, it does not follow that they should. Values are relevant throughout the research process, and scientists have an ethical responsibility to weigh values and make value judgments in the course of the research process, even when dealing with data and drawing conclusions. Each contingency in science could, in principle, become an explicit choice. Any such choice could have foreseeable consequences for what we value; to find these out for any particular case, we have to think about values, exercise moral imagination to determine the consequences of each option, and exercise value judgment as part of the choice. We cannot always foresee the consequences; the choices may sometimes be irrelevant to any values, but we cannot determine that ahead of time without looking at the details of the case. Thus scientists have a responsibility to make value judgments about scientific contingencies, and thus science is value-laden through and through.

I call this general argument "the contingency argument," which I develop in detail in Chapter 2. This argument is meant to undermine the ideal of science as value-free (or "the value-free ideal" for short), according to which values (except for scientific standards) have no role to play in scientific inquiry proper. That is, in the ideal, scientists should not consider values in science, except to ensure that their work is impartial toward and neutral for our values.[27] The value-free ideal is motivated by the thought that it will minimize the bias, subjectivism, and potential for wishful thinking that values would bring into science. Science, after all, is supposed to be objective. And yet, as the contingency argument shows, scientists have an ethical obligation to bring in values. While this may appear to create a conflict between the scientists' responsibilities, I argue that the apparent conflict is based on a mistake, an implicit view about values—that they are necessarily biasing, subjective, arbitrary, or, as I will put it, that they have no cognitive status. To deny that value judgments have cognitive status is to deny them meaning, warrant, credibility, and truth. To insist, as I do, that values can have cognitive status means that they need not be biasing or subjective, that they need not lead to wishful thinking, that they are meaningful and can be warranted and credible. Indeed, we cannot make sense of human practices, human passions, heartfelt disagreement over values, or the genuine difficulty of moral quandaries, without attributing some cognitive status to our values.[28]

If values have their own cognitive status, then they need not necessarily lead us to subjectivism and wishful thinking. On the other hand, we still need to know how to manage values in science. Attributions of "cognitive status" are no panacea against wishful thinking. Nevertheless, there is no general reason to think that value-laden science is deficient or problematic.

What we need is a better theory of values, one that avoids the simplistic idea that values necessarily lead to unacceptable bias, one which allows us to acknowledge the cognitive status of values, one that can help us distinguish the legitimate roles for values in science from those that lead to rigid and wishful thinking. This theory of values should be "science friendly," neither presupposing some mysterious, supernatural realm of values, nor removing values from the realm of evidence altogether. Science allows no unmoved movers. I propose a pragmatic pluralist theory of values, according to which values are inherently connected with action; come from many sources in human life, practice, and experience; and come in many different types according to the many different roles they play in our activities. According to this view there is a crucial distinction between unreflective or habitual values and reflective value judgment, where the latter is understood as a type of empirical inquiry into questions of what to do. The cognitive status of values tracks both their success in guiding human activities and the quality of the inquiry that warrants their evaluation. This theory of values may not be the only one for the job, nor does it necessarily satisfy the deeper questions of metaethics and ethical theory, but it has many benefits as a practical theory of values.

On this account scientific inquiry and value judgment share common aims and a common structure, laid out in Chapter 1 in the case of scientific inquiries, and Chapter 5 in the case of value judgment. Both are conceived as problem-solving inquiries occasioned by problematic situations of practice. Both involve determining the facts of the case, proposing hypotheses for resolving the problem, and experimental testing. Both are contextualized by the problematic situation they respond to. Both are judged by whether they resolve the problematic situation in practice, rather than by merely intellectual criteria.

Central to the pragmatic pluralist theory of values is the concept of moral imagination. Value judgment requires considering stakeholders and the various implications and consequences of various courses of action connected with values. As such, it requires exercising imagination via empathy, dramatic rehearsal, and creative problem solving. The exercise of moral imagination is not mere fantasy but a part of all evidence-based inquiry. The emphasis on imagination is an important feature of this theory of values, one compatible with any ultimate ethical theory.

Based on this account of values, I define a new ideal for values in science, a replacement for the value-free ideal, which has been undermined by the contingency argument. I call this "the ideal of moral imagination," defined as follows: *Scientists should recognize the contingencies in their work as unforced choices, discover morally* *salient aspects of the situation they are deciding, empathetically recognize and understand the legitimate stakeholders, imaginatively construct and explore possible options, and exercise fair and warranted value judgment in order to guide those decisions.* Legitimate stakeholders are those who either rightfully participate in or affect the decisions in question, or who will be affected by the decision. Moral imagination is an open-ended ideal to strive for, difficult in principle to satisfy, just as the value-free ideal was. It is not a minimal criterion for all inquiry to satisfy, but it is a genuine ideal.

To say that contingencies are "choices" is to say that there is more than one open option that reasonable inquirers could settle on. To say that the choice is "unforced" is to say that no factor decisively settles the matter and shows one of the options to be the best, all-things-considered, at least from the perspective of the scientific inquirer at the moment the choice is made. Not all contingencies are, in the moment, recognized as unforced choices by the inquirers. They may not imagine that there are other options and let force of habit or convention, or the appearance of only one option, decide for them. But ideally they would recognize those contingencies for what they are and exercise their moral imagination in order to make a responsible choice.

The ideal of moral imagination in turn allows us to recognize a second kind of irresponsibility in scientific research. Already thoroughly discussed are cases of *misconduct*, when scientists violate clear minimal constraints on responsible research (for example, fabricating data, plagiarism, experimenting on human subjects without consent). The ideal of moral imagination allows us to recognize a distinctive form of irresponsibility in *failures of moral imagination*, where scientists fail to live up to the ideal by, for example, failing to consider a reasonable range of options (including the superior option) or by not considering the impact on legitimate stakeholders. The second is the new form of evaluation that the book defines and advocates. It is generally a matter of degree, where misconduct is usually an all-or-nothing question.

While the ideal of moral imagination allows us to identify a distinctive failure of responsibility, its emphasis is on the positive, on what values and value judgment can contribute to scientific inquiry. The ideal of moral imagination gives scientists something to strive for and tools for responsibly making the choices that pervade the research process. It can guide decisions about research agenda, methodology, and framing hypotheses; it provides guidance on the questions that arise in the conduct of inquiry, of gathering data, of testing and refining hypotheses; it can improve the way that scientific results are presented and applied.

THE STRUCTURE OF THE BOOK

Before concluding this Introduction, I will explain the way the book is written, both the unusual structure of each chapter, the grouping of chapters, and the nature of the argument. There are different ways to read the book, depending on your interests and backgrounds.

The Structure of Each Chapter

If the audience for this book was only philosophers of science, each chapter would probably be structured in a familiar way: First, review previous work on the topic, arranged according to the structure of the dialectic or debate. Then identify the need for intervention through arguments showing the limits of what has come before. Provide a general argument for an alternative view. Then examine a case study that exemplifies or illustrates the alternative. (Alternatively, case studies can come before the general argument.) Finally, pose and respond to potential objections.

This book is different because it is written and structured with multiple audiences in mind, with each chapter organized so as to highlight the main argument without presupposing specialist knowledge. Each chapter (except this introduction) is structured in four main sections: First, the "introduction" provides a brief characterization of the problem or question the chapter is meant to address. The "argument" gives the positive account or argument that addresses the problem or answers the question. The "analysis" section deals with further complications, including tying the argument to historical sources and the contemporary academic debates, and defends the positive view in greater technical detail, responding to objections and exploring further related issues. "Next steps" briefly reviews open issues and questions and sets up the transition to the next chapter. Through this structure, I hope to provide multiple pathways through the book for audiences with different interests and backgrounds.

Pathways through the Book

Anyone simply wanting to understand the unique positive arguments and theory I'm proposing, including scientists who want motivation and advice for improving their practice, can focus on sections 1, 2, and 4 of each chapter (that is, introduction, argument, and next steps), and read the last chapter in its entirety.

Chapters 1–6 give the general account of scientific inquiry, the need for values therein, the nature of values and value judgments, and the ideal of moral imagination. The conclusion ends with a discussion of the application of the ideal of moral imagination to specific cases, its use in training scientists, and future directions concerning the credibility, dissemination, and application of science.

If you want motivation for thinking that values really do matter to science, that scientific knowledge is significantly value-laden, that scientists need to exercise value judgments, chapter 2 is key. The full argument for the need for the kind of ideal I provide proceeds primarily in chapters 2–4. The argument for the ideal itself is the business of chapters 5–6 and the conclusion.

If you want to use the book primarily for practical training purposes in the responsible conduct of research, then you can focus on the entirety of the introduction and conclusion and sections 1, 2, and 4 of chapters 5–6.

Sections that focus on specialized philosophical discussions will be marked as such, occurring primarily in the "analysis" section of each chapter; these can be safely skipped by other readers without losing the thread of the book.

Additional Apparatus

At the end of the book you will find a glossary, which contains definitions of key terms that appear throughout the text. You will also find as an appendix a page which you can photocopy that provides a helpful tool for applying the ideal of moral imagination in practice. Its use is explained in the Conclusion. A digital copy of this tool, along with other useful materials, can be found on the book website at https://valuesinscience.com.

THE PROOF OF THE PUDDING

The proof of the pudding is in the eating, and the proof of a philosophical argument is in the insight it provides when put to use. In my view the appearance of definitive argument in philosophy on foundational grounds is typically an illusion. Of course, each chapter has plenty of arguments, but as far as I am concerned, the real value of the ideas is seen in their usefulness in making practice more intelligent and responsible. The best philosophical arguments proceed from the careful analysis of a genuine problem, provide arguments that justify betting on a certain way of solving the problem, and then point the way to how that solution will alter our practices and activities and how we can tell if they have been improved thereby. This is an atypical mode of argument in many philosophical traditions, but quite common to pragmatists, among others.

The entire structure of the book is geared toward this style of argument. Chapters 1–3 provide background and set up a problem, chapters 4–6 provide an alternative account and reasons to think it is plausible, while the conclusion provides details on how to apply the account to various types of decision. Each chapter to some extent also recapitulates this structure (1 for problems, 2 for the theory or account, 3 to showing how the account can handle various complexities).

My hope is that the cogency with which my account handles specific cases discussed in the conclusion will convince you of the plausibility of my account, and give you reason to try it out in your own practices, whether you're a working scientist or someone who has to be a critical consumer of scientific results. I will not be satisfied, however (and neither should you be), until the ideas here defended are put to use and make some improvement in science and in society. All I can do here is convince you to give them a try.

Epigraph: Qtd. in Collins, "Anita Sarkeesian on GamerGate."

1. Such arguments have, in fact, been made many times by feminists, sociologists of scientific knowledge, and other thinkers. For example, Fausto-Sterling, *Myths of Gender*; Haraway, *Primate Visions*; Douglas, "Values in Science," §3.1.

2. The history summarized in this section can largely be found in Gould, *Mismeasure of Man*; Smedley, "Science and the Idea of Race."

3. Gould, *Mismeasure of Man*.

4. Gould thinks this influence was probably unconscious, as the influence of pernicious status-quo values often is.

5. Michael, "New Look at Morton's Craniological Research"; Lewis et al., "Mismeasure of Science."

6. Weisberg, "Remeasuring Man"; Kaplan, Pigliucci, and Banta, "Gould on Morton, Redux."

7. Kaplan, Pigliucci, and Banta, "Gould on Morton, Redux."

8. Eighty-one percent of US scientists are Democrats or lean Democratic, according to a 2009 Pew poll. Pew Research Center for the People and the Press, "Scientists, Politics and Religion."

9. Gould, *Mismeasure of Man*, 37.

10. The work of the Holloway, Byrne, and Marston is discussed in detail in Brown, "Love Slaves and Wonder Women." William Moulton Marston is usually assigned sole credit for most of this work, but much of it was actually collaborative, as argued by Lepore, *Secret History of Wonder Woman*. I here attempt to correct that problematic attribution by listing all three collaborators irrespective of the official "author" of the work. Olive Byrne also went by the name "Olive Richard," and some of her published writings can be found under that name.

11. Marston, *Emotions of Normal People*, 3–4.

12. Eliot, *Pink Brain, Blue Brain*; Fine, *Delusions of Gender*.

13. Gilligan, *In a Different Voice*; Ruddick, *Maternal Thinking*.

14. On transparency, see Douglas, "Weighing Complex Evidence in a Democratic Society"; Elliott and Resnik, "Science, Policy, and the Transparency of Values"; Elliott, *Tapestry of Values*.

15. Letter to early comics historian Colton Waugh, quoted in Walowit, "Wonder Woman," 42.

16. John, "Epistemic Trust and the Ethics of Science Communication," among others, has contested the norm of transparency, arguing that it actually undermines trust in experts, with deleterious epistemic and political consequences. Given the extreme prejudice in society at the time of their writing, Holloway, Byrne, and Marston may have been right to conceal the role of their values in their scientific work; however, they still potentially run afoul

of the obligation John articulates to assert only "well-established claims." It is worth noting that John's argument focuses on "communication in contexts where speakers know that their words may be twisted and manipulated for others' political or economic ends" (75), whereas we might hope for a situation where transparency might be positive rather than detrimental.

17. Cyranoski and Reardon, "Embryo Editing Sparks Epic Debate"; Evitt, Mascharak, and Altman, "Human Germline CRISPR-Cas Modification."

18. For more on the history of stem cell research funding and politics in the United States, see Wertz, "Embryo and Stem Cell Research in the United States"; Murugan, "Embryonic Stem Cell Research."

19. Vogel and Holden, "Developmental Biology"; Rao and Condic, "Alternative Sources of Pluripotent Stem Cells"; Murugan, "Embryonic Stem Cell Research"; Grinnell, *Everyday Practice of Science*, 95.

20. Addams, *Twenty Years at Hull-House*, chap. 6.

21. Some scientists and philosophers of science deny that science is really so contingent. They point toward things like simultaneous discoveries and argue for the inevitability of certain conclusions. They would explain this fact on the basis of the constraints provided by reality. But note that *contingency* does not mean absence of constraints on successful science. Success of course depends on external constraints, but this is a judgment made retrospectively. We're focused instead on the situation of scientific practice, where the inquirer is faced with frequent contingent decisions. See Hacking, *Social Construction of What?*; Franklin, "Is Failure an Option?"; Soler, Trizio, and Pickering, *Science as It Could Have Been*.

22. There may be some few areas of science where near-wholesale skepticism is warranted. See Jacob Stegenga, *Medical Nihilism*.

23. Whether we are in such an environment at present is another question.

24. For more on the interdependence of science and technology and the history of the boundary between them, see Channell, *History of Technoscience*. To avoid jargon, I have not followed those who adopt the term *technoscience* to capture the blurring of boundaries between science and technology, but my use of *science* here is inclusive of that concept.

25. Medical practice (what doctors do) has many aspects that are distinct from the research activities covered in this book and should not be understood

as covered by the arguments herein. For the use of moral imagination in guiding medical practice, see Elliott and Elliott, “From the Patient’s Point of View”; Mackenzie and Scully, “Moral Imagination, Disability and Embodiment.”

26. Giere and Moffatt, “Distributed Cognition”; Nersessian et al., “Research Laboratories as Evolving Distributed Cognitive Systems.”

27. Lacey, *Is Science Value Free?*

28. This claim is consistent with the sophisticated contemporary philosophical positions of metaethical “noncognitivism” and “antirealism,” which do not necessarily support the view that values are necessarily biasing, meaningless, or unwarranted.

CHAPTER 1

EMPIRICAL SCIENCE AS PRACTICAL INQUIRY

> To say that something is to be learned, is to be found out, is to be ascertained or proved or believed, is to say that something is to be done.
>
> **—John Dewey, "The Logic of Judgments of Practice"**

INTRODUCTION: HOW SHOULD WE THINK ABOUT SCIENCE?

How we think about the interplay of science and values depends very much on how we think about science—what it aims to do, how it works, what it produces. There are several different images of science that can be found in popular culture, in science pedagogy, and in the philosophy of science that emphasize different aspects of what goes on in the sciences and depict them more or less accurately. These different images of science are in the background of the ways we think about science. We can think about science as a body of theory or knowledge, an image that emphasizes the products of science, its content. We can also think about science as a social process, a practice engaged in by a certain group of people in our society. Or we can think of science as a method, that is, an idealized logic of inquiry that emphasizes the objective and rational nature of science.

We think about science differently if we emphasize hypothesis testing around more specific empirical hypotheses versus the large-scale dynamics of major theory change, if we emphasize laboratory practice versus the logical structure of fundamental theories like general relativity, or if we emphasize highly applicable work in biomedical science versus basic research in particle physics. A lot of confusion arises from focusing too narrowly on one particular image. When the image of science in primary education derives from simple experimental practices in classical physics (such as Galileo's simple experiments with balls, towers, planes, and so on), the public may be confused or suspicious when they learn how, say, climate science works. When pop culture portrays scientists as cold, aloof, and calculating, we may distrust scientists who are passionate about their work. When philosophy of science looks exclusively at the published results of research, they may misunderstand the process that led to those results and even what the results ultimately mean. While different images of science are better and worse for different purposes, some are more inclusive than others.[1]

For the purposes of this book, the best image of science to think with is one that emphasizes scientific inquiry. Thinking about scientific inquiry emphasizes the practice of science or the scientific process in a way that makes room for both understanding how science is actually practiced and providing a normative account of the process, that is, of scientific method. It also contextualizes the products of science, explaining the role of theory and evidence. The theory of inquiry laid out in this chapter is general without being too simplistic. It is normative, that is, it tells us something about how science *should* proceed, but it is not rationalistic, based in philosophical ideas of what is logical or rational prior to investigating how science actually works, when it works well.

The goal for our thinking about science should be that it accurately describes much of scientific practice, that it gets at what is distinctively valuable about science, that it provides guidance for practicing scientists and for others evaluating what science has done. Finally, it should provide a picture that helps us with our goal of understanding the interplay of science and values. The image of science as inquiry provided by this chapter best meets those goals.

ARGUMENT: EMPIRICAL SCIENCE AS PRACTICAL INQUIRY

In this section I provide an image of empirical science as practical inquiry[2] and show that this image is the best way to think about science, given the goals outlined in the previous section. In "Analysis: Developments of and Challenges to Science as Practical Inquiry" (p. 41), I delve into the history of related views, look at some of the limitations of this image, and address some objections.[3]

Science as a Practice

Science is, of course, a human practice. Any image of science that fails to acknowledge this is inadequate on its face, but nearly every serious philosophy of science at least pays lip service to that fact. Even philosophers who have resolutely insisted that we need to pay attention only to the logical structure of theories and the logic of evidential support have acknowledged that these are products of a process of "discovery" and have provided arguments for why the details of that process can largely be abstracted away. It has become harder and harder for philosophers of science to see that abstraction as credible or adequate for dealing with the problems they seek to address today. First, starting around 1960 philosophers of science insisted that the complex details of the history of science were relevant to understanding how science works,[4] and today many philosophers have shown that detailed attention to science as practice problematizes many of our common assumptions about how science works.[5] The image of science that I will defend thus needs to take the details of scientific practice head-on.

What is a practice, and what do practices involve?[6] In the relevant sense, a practice is an activity or set of activities undertaken by a community of practitioners. The community is constituted not just as a collection of people, but involves norms or expectations, shared objectives, as well as a division of labor. A practice has a history, and that shared history is also partly constitutive of the community of practitioners. The activities have objects or ends, and are composed of actions, operations, tools, and rules or standards.

The core activity of scientific practice is *problem-solving inquiry*. Science primarily consists of inquiries into the gaps and inadequacies of previously accumulated knowledge and the perplexities that arise from the use and development of prior knowledge. This is a broad and schematic claim. Characterizing the objects or ends of particular scientific inquiries is much more difficult, in part because of the great variety of subject matters and activities that science includes. There is no principled demarcation criterion between scientific inquiries and inquiries that we typically do not call scientific.[7] Scientific inquiries commonly feature prediction, explanation, and control as central foci, at least instrumentally if not as the main aim. This does not differentiate science from other inquiries we are less likely to consider scientific; car mechanics must engage in prediction, explanation, and control in the course of their work. Scientific inquiries often aim at systematicity beyond the narrow context where perplexities arise,[8] though that systematicity is always a partial achievement rather than a universal guarantee. This does not distinguish science, either; theology and metaphysics are highly systematic disciplines. Finally, scientists are proactive in searching out gaps and perplexities in our current knowledge, rather than being only reactive to failures; then again, so are gadfly philosophers.

Inquiry is not the only activity that constitutes the practice of science, though it is the most central one. Beyond inquiry proper, the practice of science involves general science education, training of future scientists, expert advising, and grant writing. What's more, inquiries in particular fields of science fulfill many goals, including goals that do not arise primarily within science itself. Subsidiary activities that support and extend the practice of science include contributing to public education, devising and informing specific groups and the public at large, and fund-raising. A full account of scientific practice would address each of these as well, and I will touch on them as they become relevant throughout the book. For now, though, it is important to center on scientific inquiry.

Science as a Method of Inquiry

Much of our understanding of science comes from treating science, especially scientific theory, as a subject matter to be learned and applied. This product-oriented view of science is good for certain projects, but it is of limited value, and it can become distorted if it is not responsive to other ways of thinking about science. We also think of science as a method, and in many respects this is a more inclusive image of science, one that thus has a significant influence on early science education. But "method" is an ambiguous concept, and different accounts of method can be more and less helpful.

One way to think of method is as a recipe or algorithm. In popular discussions of "The Scientific Method," this is almost certainly what people have in mind: a step-by-step recipe for solving problems or producing knowledge. When elementary school students learn "the five [or six or seven] steps of the scientific method," this is what is meant by *method.*[9] Studies of scientific practice have shown this sense of *method* to be a myth. Science does not proceed linearly according to such a recipe. It is rather a messy process, with very different techniques, standards, tools, and procedures used by different scientists and across different fields. Attempts to enforce uniformity of method in this sense would destroy science.[10]

In another sense, *method* means an inference structure. This is the primary sense of *method* in most of the history of philosophy of science. There have been clashes between confirmationist, falsificationist, holist, and similar inference structures under discussions of scientific method. Confirmationist accounts hold that there is a logical inference according to which successful predictions based on a theory or hypothesis provide support for that hypothesis. Falsificationists hold instead that the key inference structure in science is falsification of a generalization through a refuting instance. Holists maintain instead that it is the best fit between general theories and the empirical basis that forms the core of scientific inquiry, where both theory and data can be revised. This is one sense of *method* that Paul Feyerabend was *against*—the idea that there was one rational inference structure for all of science. That type of uniformity would hamper science, in Feyerabend's view.[11]

Another sense of *method* is a "method of inquiry." *Inquiry* means something a bit different from *inference*. *Inference* suggests a final judgment. *Inquiry* suggests a searching, an ongoing process. *Inquiry* is a little less abstract, a little closer to practice. Nevertheless, inquiry is deliberate, methodical, and there are norms and standards that make some inquiries better than others. Inquiry is a deliberate process of resolving a problem through investigation, testing, and judgment. Inquiry begins in doubt or perplexity, an inchoate sense that something is wrong—there is some problem, but we don't know what it is yet. It concludes when a clear sense of the problem and a solution grounded in evidence are brought together in judgment. A process of inquiry is neither a recipe nor an abstract or rigid inference structure; *method* in the sense of inquiry is impervious to critiques of *method* in those senses. When John Dewey referred to "science as method," it was this sense of *method* that he meant.[12]

If science is a type of inquiry, what distinguishes it from other types of inquiries? There are, for example, legal inquiries, police inquiries. There are everyday attempts to figure out what to do that barely seem to deserve the name *inquiry*, though they fit the definition. Indeed, any human practice or activity requires inquiry when it runs into problems that need to be solved. As I have said before, there is no principled demarcation of scientific inquiry from other types of inquiry, no bright line separating science from other kinds of practices. However, it may be useful to distinguish different types of inquiry in some contexts; we can give a rough-and-ready distinction according to the subject matters of the inquiries, as well as the particular way a practice values inquiry.

First, the subject matter of the inquiry, the practice in which the inquiry arises and is meant to resolve, distinguishes different types of inquiry. In a (proper) police inquiry, a crime has been committed, and the public order problematized. The inquiry seeks to solve the problems of whodunit, establishing their means, motive, and opportunity, and bringing them to justice, in order to uphold and maintain the rule of law. Likewise, scientific inquiry in chemistry is problem solving in response to gaps in our knowledge of chemicals and chemical change (or however we best characterize the subject matter of chemistry), or perplexities that arise in our attempt to extend that knowledge, or failures in activities of prediction, explanation, and control related to chemistry, as such or for specific purposes. This type of inquiry differs from the habitual application of chemical knowledge. It differs from other fields of inquiry as well in its relationship to its specific subject matter.

A second distinguishing mark of scientific inquiry has to do with the value of inquiry itself in scientific practice at large. In our everyday lives and many of our practices, inquiry is an unfortunate thing, the result of a failure or problem, to be conducted quickly so that a valued practice can continue on its way. Scientists have almost the opposite attitude: the inquiry itself is the part of science that matters most; the knowledge it produces, and the powers of prediction, explanation, and control it enables, are almost a by-product or afterthought. Unlike many other areas of inquiry, scientific inquiry is proactive rather than reactive, seeking out problems rather than waiting for them to arise naturally. Scientists should always be plumbing the depths and limits of our knowledge, rather than resting securely in it. Part of the distinctive value of science arises from this peculiar attitude.

Science as Practical Inquiry

What is distinctive about scientific inquiry? Some would answer the question very differently. They would say that unlike other types of inquiry, scientific inquiry is interested only in the truth, in being faithful to the facts, in discovering fundamental or foundational theories and rigorously testing them. What distinguishes science, in other words, is its purity, its pure objectivity, its remove from the everyday, and the slow, steady revelation of the truth about nature.

I think, for the most part, this is an unhelpful myth about science. Science is not distinctive because it is pure, theoretical inquiry. Science includes "applied science" as well as biomedical science and engineering. Rather, scientific inquiry is inquiry into perplexities that arise in scientific practices and activities with a variety of aims. To say that science is "practical inquiry" is not to downplay its theoretical nature, but to emphasize its relationship to practices. The good sense behind claims of "purity" has less to do with the unpractical nature of scientific inquiry as it does the *systematic* ambitions of the activities of prediction, explanation, and control that play such important roles in science. Basic technology is happy to develop in an ad hoc or organic fashion, and the long history of technology in many areas (for example, agricultural technology prior to the late nineteenth century) largely consists of such unsystematic inquiry and development. But scientific agronomy, agrotechnology, and agricultural engineering seek to systematically understand and control farming. They are not therefore more "pure" or less practical; they are no less directed at so-called non-epistemic goals. The same goes for particle physics; though more distant from directly "useful" applications than agronomy, it is nevertheless practical, intervening as it does on practices that we care about, practices concerning prediction and control of basic forces and constituents of matter.

Science is not distinguished from other modes of inquiry by being practical in this sense; in this degree, all genuine inquiry is practical inquiry. Science is the necessary response to an unfortunate situation, elevated to an art: science is the art of problem solving. While elsewhere in human life, problems, and thus inquiry, are a cause of distress; a good problem is as much a matter of delight for the scientist as a good solution. This art is practical in two senses: (1) the problems that it seeks to find and solve are problems that arise in specific human practices; (2) the art is practical in that it is very useful to anticipate and resolve problems in advance to prevent them from becoming immediately threatening.

As a practical art, there is an affective-aesthetic dimension often ignored by philosophers of science, though occasionally adverted to by philosophically inclined scientists. Problems are not always obvious, especially the precise nature of the problem. Nor are problems merely intellectual; they must be genuinely perplexing in order to be serious problems, even in science. Genuine scientific problems generally begin as felt perplexities, found in part intuitively, understood intellectually only when inquiry is well under way. Throughout inquiry, feeling or a qualitative sense of the problem continues, and the successful solution is as much a culmination of feeling and aesthetic experience as any artwork.

Inquiry as Transformative

The aim and final product of inquiry is a judgment of how to proceed, how to resolve the perplexity that initiated inquiry. What does it take to resolve a practical problem? We must be able to transform the problematic practice so as to remove the perplexity, provide clarity about how to proceed, and restore a degree of order to the practice so that it may operate relatively smoothly and successfully. In the case of scientific inquiry, one frequent result is that the activities of prediction, explanation, and control are transformed in some way. This is not just a change of mind, a matter of some item of knowledge missing that has been discovered. Of course linguistic and intellectual habits will change as a result of the transformation, but so will a variety of social interactions, technological projects, and manipulation of the actual objects the inquiry is concerned with. Prediction, explanation, and control are not, ultimately, purely intellectual activities; they essentially involve interaction with the world.

We might describe the collective of actors, activities, and objects in a practice as a *situation*, which captures the fact that the human activities involve more than just our minds, and take place on the background of a world of objects. Changing the perplexing situation is crucial to the resolution of problems. Changing our minds is only one extreme of possible changes to the situation, and not the most general case. In general, our operative ideas, our practices, and the elements of the world that contribute to that situation will all potentially be transformed by inquiry. Of course sometimes the problem is mainly in our heads, and the type of inquiry needed is primarily therapeutic (and not less genuine, necessary, or worthwhile because of it). But this is an extreme case where only one side of a relationship needs to change. Scientific inquiry addresses a discoordinated, situated practice. The resolution of the discoordination involves changing the relationship between the constituents of the practice, physical and ideal, and often transforming the constituents themselves.

Phases of Scientific Inquiry

Scientific inquiry as it is practiced is a messy, nonlinear affair, very distant from "the scientific method" (as recipe) that is taught in school. Nevertheless, there is a kernel of truth in that oversimplified, step-by-step picture of science proceeding from problem to observation, to hypothesis, to experiment, to conclusion.[13] The temporal ordering of a scientific inquiry is simpler than this, but the functional structure is much more complex. In terms of stages, the precondition of inquiry is a *perplexity*, or what we might call "an indeterminate situation," where the practices and activities in question have become disordered or incoherent, even though recognition of that may be inchoate. Recognition of a perplexity as a problem for inquiry, and the decision to treat it as such, is where *inquiry* begins. When a perplexity or indeterminate situation becomes the subject of inquiry, we call it a "problematic situation." Inquiry has a complex, iterative or recursive structure of functionally differentiated phases, which concludes with an act of *judgment*, which puts a solution to the problem into place. We call the inquiry successful if practice is rendered determinate enough to proceed.

There are some important distinctions to be made between the elements of inquiry, not as a series of steps or stages, but as what we might call "moments" or "phases." These nonlinear phases are distinguished from each other not by their order in time, but by their different functional roles in a scientific inquiry. Each phase is a process or action that produces and refines the materials of inquiry—facts, data, evidence, hypotheses, models, problem statements, chains of reasoning, and so on.

The phases are as follows.[14]

Observation

Gathering data about the problematic situation in order to understand how and why it has become problematic, to discriminate between the factual and conceptual contributions to the current situation, and to determine the facts of the case, which represent what is present and fixed about the situation. These "facts" are not given states of affairs but represent decisions about how to represent the fixed features of the situation. They are open to revision through the course of the inquiry.

Problem framing

The process of creating a statement of the problem that represents what is problematic, perplexing, discoordinated, or indeterminate about the situation, in light of the facts of the case.

Suggestion

The generation of hypotheses that would solve the problem. These hypotheses represent the possible developments of the situation in directions that could overcome the perplexity or indeterminacy represented by the problem statement.

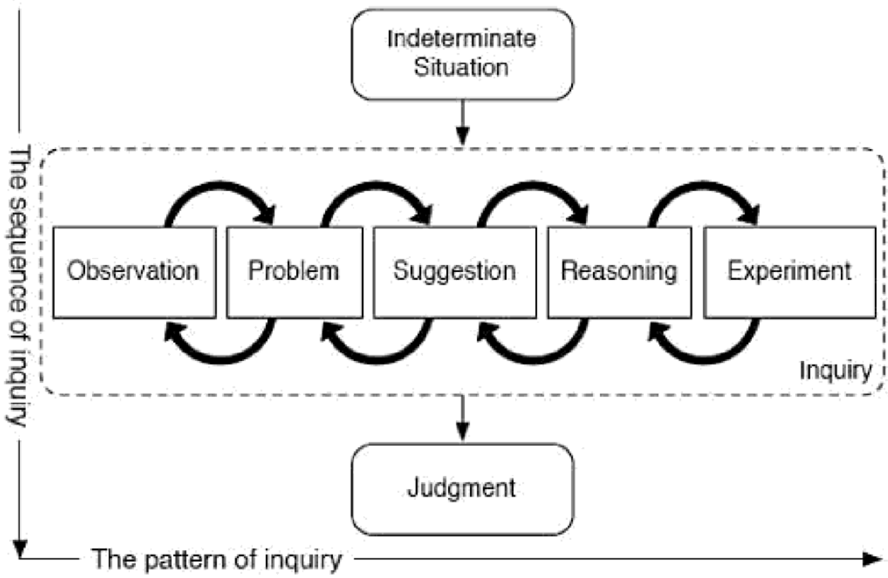

FIGURE 1.1. The Pattern of Inquiry. There are two dimensions to this account of inquiry. The first dimension shows that inquiry proceeds linearly from an initial perplexity (or indeterminate situation) to a final judgment. The second consists of the functionally defined phases. The phases of inquiry are functionally interrelated, but do not proceed in a linear fashion. The connections between the phases are simplified for presentation; in principle, they are all mutually connected.

Reasoning

The systematic refinement and coordination of the problem statement, facts, and hypotheses in order to make them function together in a way that might resolve the problem, as well as functional whole to resolve the problematic situation. Once the mutual coordination of the components of inquiry has reached the point where things are coherent enough to warrant the adoption of a solution to the problem of inquiry, or more carefully, a resolution of the perplexed and problematic situation that began the inquiry, then a judgment is rendered. The coherence in question is a pragmatic coherence, the successful working together of processes and materials of inquiry to transform the situation and practice in such a way that the problem is eliminated or ameliorated. It is functional fitness that determines the suitability of the inquiry, and not some criteria imposed outside of the problematic situation.

Scientific inquiry is generally a messy process, and the materials that inquiry works with—data sets, instruments, inscriptions, hypotheses, derivations, calculations, and so on—can be quite disorderly in the midst of inquiry. By contrast, by the point of judgment these materials are reorganized for orderly presentation. This means that data sets have been cleaned up; equipment has been (relatively) standardized; charts and graphs have been produced and made presentable; derivations, calculations, and other reasoning have been rendered into a carefully worded argument. These orderly materials are then presented (often in highly formalized and stereotyped fashion) at conferences and published in journal articles, preprint archives, and databases. This ordering serves a variety of important purposes: it makes the justification of the results easier to evaluate, and it packages data and theory in a way that makes them more portable into future research where they might serve as useful tools. A long process may then proceed from the judgment of the inquirers to the certification of their results by the relevant scientific community, and the gradual adoption thereby of new knowledge, new activities, and new modes of continuity and difference will fall cannot be assumed in advance.

This also means that there is no algorithmic way to amalgamate facts across inquiries into different problems. This raises all manner of problems for various philosophical-epistemological projects that assume, to the contrary, that we can get cross-situational generality for free. Epistemologists often speak of "all the evidence" or the "total evidence condition," the assumption being that our knowledge has to be compatible with (or stronger, confirmed by) all the evidence available at a particular time. This presumes that there are a set of things, "*the* evidence," that are evidence *in their essence* and across all contexts. To the contrary, determination of what counts as the evidence in each particular case is a highly selective and context-sensitive matter, a difficult thing to figure out. To treat everything that has ever been considered a fact in some inquiry as a constraint on *every* inquiry would stifle scientific progress completely.

Inquiry, Credibility, and Certification

Fred Grinnell's *Everyday Practice of Science*, which is also broadly speaking pragmatist in nature, draws a central distinction between *discovery* and *credibility*.[16] According to Grinnell, the credibility process is the "process through which discovery claims put forth by individual researchers and research groups become transformed into the research community's credible discoveries."[17] The link between the discovery process and the credibility process is the research paper, which is the final product of the discovery process and the starting point of credibility assessment and which starts with the initial peer review of the paper, continues through the discussion and citation of the paper in the literature, and ends with the most significant and credible discoveries becoming "the textbook facts of science education."[18]

Not only does credibility accrue to discovery claims, but researchers, particularly principal investigators of research groups, gain in personal credibility or "credit-ability."[19] Both the publication process and the research grant awarding process are connected with the accrual of personal credit-ability. A related account of the process of the "context of certification" is given by Philip Kitcher, focusing on the later stages of this credibility process.[20]

My account of inquiry thus far deals mostly with what Grinnell calls "discovery." Why not instead focus more on credibility? One might argue that the latter is more relevant for the questions the book ultimately poses about values in science and the responsibilities of scientists. I do believe that questions about the social structure of credibility are important to these topics, and I will discuss one example in the Conclusion. There are several reasons, however, that the topic of credibility remains on the margin, while discovery (or, I prefer to say, "inquiry") remains at the center: (1) because a discussion of credibility alone is insufficient for responsible science—an account of responsible inquiry is also needed; (2) because there are already many good accounts of the credibility process and the role of values therein in the literature;[21] and (3) because the primary focus of the book is to provide guidance to scientists in the course of inquiry and to citizens who want to evaluate specific scientific results.

First, it is not enough to have an adequate theory of science, much less an adequate account of the responsibilities of scientists, to focus on the large-scale social processes of scientific credibility. A significant amount of the work that goes on in science happens "in the laboratory" and "at the chalkboard," so to speak. These activities all take place in the discovery process, and what happens here is not washed out by what happens at the credibility phase. As we will see in Chapter 2, many of the contingencies faced in

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generations working on science and values prior to Scriven (for example, Dewey, Rudner) recognized the need for such an account, and some attempted to provide a theory adequate to the task. Contemporary philosophers of science, with a few exceptions,[24] have not recognized that they need such accounts, and they have tended to make serious errors as a result.

The Roots of Noncognitivism

Noncognitivism has a complicated history within philosophy of science. Noncognitivism was a broadly shared commitment of the members of the Vienna Circle,[25] and the influential strain of logical empiricism that followed from it. These philosophers were keen to distinguish the meaningful parts of philosophy and science that could be based in empirical data and logical/mathematical reasoning from the nonsense of metaphysics, normative ethics, and superstition. These philosophers centered logic and philosophy of science, and while their views have largely fallen into disfavor in analytic philosophy in general, within philosophy of science they still have a certain influence. In particular, I think their commitment to noncognitivism survives in the intuitions many philosophers of science have about values.

Rudolph Carnap's thesis that value judgments are meaningless is the most well-known statement of noncognitivism among the logical empiricists, and usually it is thought to be representative. However, even for Carnap the situation is complex. In his early philosophical work, *Der logische Aufbau der Welt* (*The Logical Construction of the World*), Carnap made a place for values as meaningful, one of the objects of study for scientific philosophy. However, around 1929–1931 Carnap came to accept a strict separation of facts and values and to regard value judgments as "meaningless metaphysics" (a term of abuse applied widely by the

Vienna Circle).[26] Carnap did not hold that science could be entirely value-free; in particular, in Carnap's mature view the choice of linguistic or ontological framework, undeniably a feature of science and scientific philosophy, was decided on "pragmatic grounds." They could not be decided by logic or empirical evidence alone, but must be decided by value-laden considerations about how the framework was suited to our purposes. He called these "external questions," and they are clearly decided by value judgments.[27]

Hans Reichenbach fortified Carnap's noncognitivism by drawing a sharp distinction between the context of discovery and the context of justification, where the former includes all the contingent, messy, and potentially value-laden aspects of scientific inquiry, while the latter concerned only the objective, abstract, logical relations between theory and evidence. Reichenbach likewise thought that nothing normative belonged to the context of justification, and that there could be no knowledge of normative matters. Reichenbach puts the point in no uncertain terms: "The modern analysis of knowledge [into the purely analytic, tautologous statements of logic and mathematics and the synthetic statements about matters of fact] makes a cognitive ethics impossible: knowledge does not include any normative parts and therefore does not lend itself to an interpretation of ethics . . . if it could be carried through . . . ethical rules would be deprived of their imperative character. The two-thousand-year-old plan to establish ethics on a cognitive basis results from a misunderstanding of knowledge, from the erroneous conception that knowledge contains a normative part."[28] According to Reichenbach, values have no part in science (at least in the context of justification, the part of science that delivers knowledge), and values have no cognitive status whatsoever.[29] Carnap's "external questions," properly shunted into the context of discovery, could be treated as totally separate from science proper, which is value-free.

Otto Neurath is something of an exception to the rule on many of the supposed doctrines of the Vienna Circle, and this case is no different. Neurath articulates a version of the underdetermination argument (see "The Underdetermination Argument," p. 80), and a form of epistemological holism, and on that basis sees science as necessarily value-laden all the way down.[30] Yet Neurath seems to have accepted the standard noncognitivist line on values themselves, eschewing the term "values" itself in favor of "auxiliary motives," emphasizing that though one could be motivated by values, the attempt to reason about them was a kind of pseudo-rationalism.[31]

While few philosophers today subscribe to the tenets of logical empiricism, and even philosophy of science is undoubtedly in a "post-positivist" era, this does not mean that these noncognitivist views are entirely dead. Rather than being an explicit commitment grounded in a theory of meaning, as it was for Carnap, for example, today this view of values survives as part of an implicit model.[32] Implicit models tend to drive philosophical intuitions and beliefs when the subject of the model is not an explicit topic of investigation. The implicit model of values that most philosophers of science work with, whether or not they think that science ought to be value-free, is generally an emotivist or at least noncognitivist one. Noncognitivist assumptions or presuppositions pervade discussions of values in science on both sides. There are a few key exceptions to this rule (for example, Elizabeth Anderson, Sharyn Clough, Dan Hicks, Lynn Hankinson Nelson), and others explicitly deny that they hold a noncognitivist view, while continuing to defend claims that seem to me to necessarily presuppose some kind of noncognitivism.[33]

The roots of noncognitivism run deep in philosophy of science.

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