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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"

2.1 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 is, 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 hypothesis 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 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 an 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/logical account of the process, i.e., 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 provide guidance for scientists practicing and others evaluating what science has done. Finally, it should provide a picture that help us with our goal of understanding the interplay of science and values. The image of science as inquiry best meets those goals. This chapter will provide that account of scientific inquiry.

[1] For instance, images of science that emphasize practice can be more inclusive than those that emphasize theory alone, because theorizing is one of the practices that need to be analyzed.

2.2 An Image of 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 outline in the previous section. In §3, I look at some of the limitations of this image, as well as addressing some objections.

2.2.1 Science as a Practice

Scientific 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 pay attention only to the logical structure of theories and the logic of evidential support has 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 philosophers of science seek to address today. First, philosophers insisted that the complex details of the history of science were relevant to understanding how science works,[3] and today, many philosophers

[2] Much of this image originates in the ideas of Charles Sanders Peirce and John Dewey. See 2.3.1.

[3] Ludwik Fleck, *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1979); Norwood Russell Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: University Press, 1958); Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3rd ed. (University Of Chicago Press, 1962 [1996]); Mary Hesse, *Models and Analogies in Science* (London: Sheed; Ward, 1963); I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," *Criticism and the Growth of Knowledge*, 1970, 91–195; Paul K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London; Atlantic Highlands: New Left Books, 1975); Larry Laudan, *Progress and Its Problems: Toward a Theory of Scientific Growth* (Berkeley: University of California Press, 1977).

have shown that detailed attention to science as practice problematizes many of our common assumption about how science works.[4] 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?[5] 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.

Scientific practice has two core activities or sets of activities. First are the activities of prediction, explanation, and control based on a previous established store of knowledge. The these activities are logically distinct but in practice so intertwined that it often makes sense to talk about them as a single activity. Second is the searching, problem-solving process of inquiry into the gaps, problems, and inadequacies of our attempt to predict, explain, and control, i.e., perplexities that arise in the first set of practices. The need for prediction, explanation, and control are what occasion scientific inquiry, and successful scientific inquiry enables and improves our ability to predict, explain, and control in one respect or another. I will focus on providing an account of scientific inquiry, and will have relatively less to say about what prediction, explanation, and control involve outside of their role in inquiry. These are understood in various ways in the philosophical literature (with the most ink spilled about the nature of explanation), and a review of that space is beyond the scope of our needs here. One distinctive feature of prediction, explanation, and control in science is their relative systematicity, as opposed to the more ad hoc versions of these activities in other aspects of everyday life.[6]

These are not the only activity that constitute the practice of science, though they are the main ones. Subsidiary activities that support and extend the practice of science include contributing to public education, training future scientists, advising and informing specific groups and the public at large, fundraising, and, as we will seek in Chapter 12, speculative creation of worldviews. 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.

2.2.2 Science as 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.

[4] Nancy Cartwright, *How the Laws of Physics Lie* (Oxford University Press, USA, 1983); Allan Franklin, *The Neglect of Experiment* (Cambridge: Cambridge University Press, 1986); David L Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Chicago: University of Chicago Press, 1988); John Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science* (Cambridge, Mass.: Harvard University Press, 1993); Joseph Rouse, *Engaging Science: How to Understand Its Practices Philosophically* (Cornell University Press, 1996); J. Rouse, *How scientific practices matter: Reclaiming philosophical naturalism* (University of Chicago Press, 2002); Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford University Press, USA, 2004); Hasok Chang, *Is Water H_2_O?: Evidence, Realism and Pluralism*, vol. v. 293, Boston Studies in the Philosophy of Science (Dordrecht: Springer Verlag, 2012); Léna Soler et al., *Science After the Practice Turn in the Philosophy, History, and Social Studies of Science* (Routledge, 2014); C. Kenneth Waters, "Shifting Attention from Theory to Practice in Philosophy of Biology," in *New Directions in the Philosophy of Science,* ed. M.C. Galavotti et al. (Berlin: Springer International Publishing, 2014), 121–39. Many of these philosophers were strongly influenced by engagements with social studies of science, e.g., in David Gooding, Trevor Pinch, and Simon Schaffer, *The Uses of Experiment: Studies in the Natural Sciences* (Cambridge: Cambridge University Press, 1989) and Andrew Pickering, *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992), or by close engagement with scientists, especially philosophers of biology engaging biologists.

[5] My analysis of practices and activities draws on Cultural-Historical Activity Theory as exemplified by Michael Cole, *Cultural Psychology: A Once and Future Discipline* (Belknap Press, 1996); Yrjö Engeström, "Activity Theory and Individual and Social Transformation," in *Perspectives on Activity Theory*, ed. Yrjö Engeström, R. Miettinen, and R.-L. Punamaki (Cambridge, England: Cambridge University Press, 1999).

[6] For an image of science emphasizing its systematicity, see Paul Hoyningen-Huene, *Systematicity: The Nature of Science*, Oxford Studies in Philosophy of Science (New York: Oxford University Press, 2013).

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, and one that thus has an 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."[7] 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.[8]

The Scientific Method is variously understood as a recipe, algorithm, inference structure, attitude, or process of inquiry. There is a serious risk of reifying a particular approach to science when discussing "The Scientific Method" that we must avoid.

[7] See James Blachowicz, "How Science Textbooks Treat Scientific Method: A Philosopher's Perspective," *The British Journal for the Philosophy of Science* 60, no. 2 (2009): 303.

[8] This is one of the key arguments of Paul K. Feyerabend, *Against Method*, 3rd ed. (Verso, 1993).

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. Confirmationists 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 hold 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 largely the sense of "method" that Paul Feyerabend was *against*—the idea that there was one rational inference-structure for all of science, though the line between method as recipe and method as inference structure has not always been drawn very clearly by defenders or opponents of universal method.

Another sense of "method" is a "method of inquiry." "Inquiry" means something a bit different from "inference." Inference suggests a final judgment. Inquiry also 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. A process of inquiry is neither a recipe nor an abstract inference-structure; this kind of "method" is impervious to critiques of "method" in those senses.

Inquiry is a deliberate response to a perplexity that arises in a practice, which treats the perplexity as a problem to be understood and resolved through a process of investigation, inference, testing, and judgment.

If science is a type of inquiry, what distinguishes it from other types of inquiries? There are, e.g., 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 practices and activities require inquiry when they run into problems that need to be solved. What distinguishes the different types of 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 whodunnit, establishing their means, motive, and opportunity, and bringing them to justice, or order to uphold and maintain the rule of law. Likewise, scientific inquiry in chemistry is problem-solving failures in the practice of prediction, explanation, and control of chemicals and chemical change (or however we best characterize the subject-matter of chemistry). It differs from the rote activity of prediction, explanation, and control on the basis of established chemical knowledge, and from other fields of inquiry in its relationship to that activity and 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 byproduct 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 unusual attitude.

2.2.3 Science as Practical Inquiry

Some would answer the question very differently. What is distinctive about scientific inquiry? 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 or discoordinations that arise in scientific activities of prediction, explanation, and control. Thus, to say that science is "practical inquiry" is not to downplay its theoretical nature, but to emphasize its relationship to practices. The sense of "purity" has less to do with the un-practical nature of scientific inquiry as it does the *systematic* ambitions of the activities of prediction, explanation, and control that are distinctive to science. Basic technology is happy to develop in an *ad hoc* or organic fashion, and the long history of technology in many areas (e.g., farming technology) 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. The same goes for particle physics; though more distant from directly "useful" applications than agronomy, it is nevertheless practical, intervening as it does on certain practices that we care about.

Science is not distinguished from other modes of inquiry by being practical in this sense; in this degree, all genuine inquiry is practical inquiry. But science raises the activity of inquiry from a unfortunate necessity to a celebrated art. Science is the art of problem-solving. Where 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 with a practice—the practice of prediction, explanation, and control; (2) the art is practical in that it is very useful to anticipate and resolve problems in advance for them 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 taken as serious problems, even in science. Genuine scientific problems generally begin as felt perplexities, found in part intuitively, understood intellectually only when inquiry is well underway. 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 art-work.

2.2.4 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, this means that the practices 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 nicely 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 effective in the degenerate case that the problem is "all in our heads," but that's more a matter of therapy than inquiry. Genuine scientific inquiry involves a discoordinated, situated practice. The resolution of the discoordination involves changing the relationship between the constituents of the practice, physical and ideal, and perhaps the constituents themselves.

2.2.5 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.[9] There are some important distinctions to be made between the elements of scientific inquiry, not as a series of steps or stages, but as what we might call "moments" or "phases." These 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, etc.

[9] The origins of the version taught in school are probably an oversimplication or misunderstanding of Dewey. See Blachowicz, "How Science Textbooks Treat Scientific Method."

The phases are:[10]

[10] The categorization of phases is an interpretive process. There are ways of drawing the distinctions with fewer or more categories, which may be better or worse for various purposes, but there are no neat cuts in nature. I find these five to be a perspicuous picture in general.

Observation Operations of observation must take place in order to take stock of the perplexed situation that evokes inquiry, the failures of prediction, explanation, and control that motivate the inquiry. We need to gather data on the situation that helps us begin to understand the problem at hand and the conflicting tendencies in our response to it. Prior to the discoordination that begins the inquiry, the distinction between theoretical and observational elements of description is vague.

In habitual activity, we tend to run together the facts and our ideas about them, and we behave as if there is no difference between the model of a thing and the thing modeled. This is a reasonable and necessary way to go on, so long as no problems arise. But inquiry requires that we discriminate, so far as possible (a) the factual vs. conceptual contribution to the materials we have to work with, (b) features of the subject-matter in question. These constitute the relevant features of the situation which has become perplexed, and are required to determine the nature of the problem and our response.

The products of observation are **facts of the case**, and they represented what is present and fixed in the problematic situation.

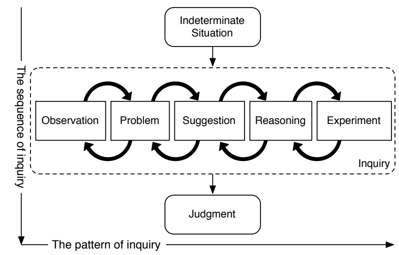

Figure 2.1: **The Pattern of Inquiry.** The phases of inquiry are functionally interrelated, but do not proceed in a linear fashion. There are two dimensions to this account of inquiry. The first dimension shows that inquiry proceeds from an initial perplexity (or indeterminate situation) to a final judgment. The second consists of the functionally-defined phases.

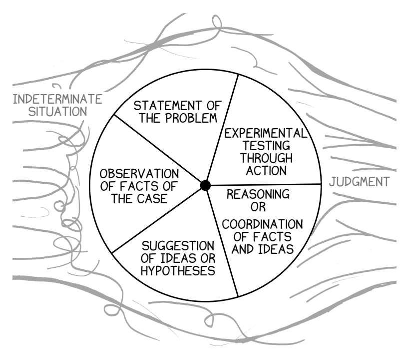

Figure 2.2: **The Wheel of Inquiry.** Again, the phases of inquiry do not proceed linearly, but are each interlocking parts of the whole process, which make be revisited iteratively as each phase is revisited and refined.

Problem-framing The situation must be assessed in order attempt to formulate a **statement of the problem** that adequately captures the given perplexity, the discoordination or indeterminacy that defines the problematic situation. Scientific inquiry does not begin with a set problem or question at which science is directed. The agenda of inquiry cannot be set by fiat. Where no genuine perplexity exists, there is no room for scientific inquiry. Where it does, the problem cannot be accurately or adequately stated ahead of time; the framing of the problem is a phase of the inquiry itself, and it evolves as the inquiry is pursued, more adequate and sophisticated observations are made, and the facts of the case are made clearer.

Sometimes I will call the "statement of the problem" simply "the **problem** for short."

Hypothesis-generation The first pass at determining the factual conditions of the situation, the conceptual possibilities in our theories, and the terms of the problem suggests **hypotheses** for solving the problem. Forming a problem-statement and suggesting a hypothesis are coordinate activities. The former connects to the settled features of the situation in which a tension arises, while the latter connects to some possibility for further action that resolves the tension. If the factual side of inquiry represents what has been determined as a fixed feature of the problematic situation, then the hypothetical (conceptual, theoretical) side of inquiry represents what the possibilities inherent in the situation that make a resolution possible.

Systematic reasoning Reasoning is a process of refinement and mutual coordination of the problem-statement, observed facts, and hypothetical ideas produced so far by inquiry. There are several aspects of this process which depend on each other and need not proceed linearly. Background theoretical materials, well-tested models, and other conceptual resources are brought to bear on the problem at hand. None of these can be taken for granted, but all can be treated as potential tools for crafting a solution. Hypotheses are developed by processes of reasoning to be more specific and relevant to the case at hand, to be in greater concert with more general theoretical materials, to suggest further operations of observation, and to take into account the evolving body of data and statement of the problem. New observations are made in response to the evolving series of hypotheses and theoretical ideas, to answer questions posed by them and fill in information needed to specify the relevant features of the ideas. From the set of putative evidence constructed so far, certain are selected or amplified as relevant, while others are rejected as irrelevant, imprecise, poorly executed, or explained away as effects of interfering phenomena that must be controlled. The statement of the problem is refined to reflect the changing understanding of the situation and the evolving series of hypotheses.

In addition to a refined hypothesis, the process of reasoning produces a **series of deductions** that lend support to the hypothesis on the basis of broader theoretical considerations. (This process may be less that strictly deductive, and may start with less than first principles.) The facts of the case and experimental results are refined into **data sets**, **models of data** and a body of **evidence** that support the hypothesis.

Experimental testing A series of controlled, limited, or tentative, experimental applications of the hypotheses are made in order to evaluate their probable efficacy in solving the problem. Earlier experiments can suggest more refined experiments, or the necessity of further articulating data and hypothesis, or the need to "go back to the drawing board." Experimental tests are importantly different from observations. While the evolving hypothesis may guide observation, only experimentation really puts the hypothesis to the test.

Experimentation as I used it here need not mean controlled laboratory experimentation. The key feature is intervention on the basis of the hypothesis, as an application of the hypothesis prior to full implementation. A variety of cases of what we might call "degenerate experiments" can still play the functional role of experimental testing. For instance, *natural experiments* involve no active interventions on the part of the inquirer, but they consist of naturally occurring phenomena that can suitably play the role of experimental testing because they are constituted *as if* someone had set them up to test the hypothesis. *Novel observations* similarly do not involve intervening on the system itself, but they do involve changing our activities in certain ways, guided by the hypothesis, whether that means designing a new instrument or merely pointing a telescope in a certain place at a certain time, and observing results unexpected but for the hypothesis. *Thought experiments* and *simulations* push the boundaries of what can reasonably be called an experiment, as the influence of the world upon them is only through the experience of the experimenter or what they

Degenerate experiments play the same functional role as any experimental tests, but they diverge from protoypical experiments in various respects.

program into the simulation. Nevertheless, in certain limited cases, these play the role of experimental tests as well.

The body of **experimental results** produced is most relevant to warranting the results of inquiry.

This "pattern" represents one way of dividing up the phases of a well-conducted inquiry. As such, it represents an ideal image of the process of inquiry.

2.2.6 Judgment and the Appearance of Order

If inquiry begins in perplexity, it ends in a final judgment. "Judgment" names both the process and the product at the conclusion of inquiry. That is, when we make a final judgment, we produce something new, a judgment. Judgment is not a private mental act, but an assertion. Judgment does not leave things as they were, but transforms the situation that occasioned inquiry. In the case of scientific inquiry, the judgment modifies the activities of prediction, explanation, and control that had become indeterminate and perplexed, and institutes new or modified activities, in many cases with new content.

The process of judgment is the result of the evaluation of functional fitness of the phases of inquiry in light of each other. Once the reciprocal adjustment has reached the point where things are coherent enough to warrant as adoption as 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.

Pragmatic coherence is a property of practices, actions, imperatives, and material means than can successfully work together.

Scientific inquiry is generally a messy process, and the materials that inquiry works with—data sets, instruments, inscriptions, hypotheses, derivations, calculations, etc—can be quite disorderly in the middle 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 (somewhat) standardized, charts and graphs have been produced and made presentable, derivations, calculations, and other reasoning has 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 graduate adoption thereby of the new modes of prediction, explanation, and control.

This ordering and packaging also involves reordering the process of science for linear presentation. This in particular has sometimes mislead those who looked primarily at the published record for their understanding of scientific practice (including science educators and philosophers of science). Again, the highly stylized format in which the process of inquiry is presented in the published record serves a particular communicative and rhetorical function, and is not confused by practicing scientists as a literal report of the history of the research that produced the judgment. Rather, it serves to communicate the *results* of specific aspects of the inquiry process. In particular, as inquiry moves towards judgment, experimental procedures and processes are purposefully regularized and repeated within the laboratory, and the description of "methods" often provide a (highly schematic) report of the final outcome of this process. Likewise, other parts which superficially appear to describe the research process in fact serve to explain the pragmatic coherence between the phases of inquiry. Both in reordering the materials and the process of inquiry, the ultimate aim is communication and potential use for future inquiry.

2.2.7 Inquiry as Situational

This image of scientific inquiry implies a strong form of contextualism. According to this contextualism, the materials of inquiry are tools evaluated for their instrumental value in resolving the perplexity that occasioned inquiry. These evaluations are tied to specific situations of inquiry; there is no way to guarantee, prior to further inquiry, that they will be adequate there. Of course, scientists are always aiming to systematize and extend their practices, and this is one of the best ways to find new problems and occasion new inquiries. There is no algorithmic way to determine relevance of results across situations, no "short cut" to actually engaging in scientific inquiry.

This situational picture also implies that the core distinctions of phases of inquiry and their materials are functional distinctions, valid within a situation. In other situations, they likely do not apply. Facts of the case in one inquiry may function as hypotheses in another, or may be rejected entirely in another. Acceptance of a hypothesis in one situation does not imply its validity in another situation. Though the diversity of situations should not be exaggerated—there is continuity between inquiries—where 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. Epistemologist 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.

2.2.8 Inquiry, Credibility, and Certification

Fred Grinnell's own highly praised work on the *Everyday Practice of Science*, which is also broadly speaking pragmatist in nature, draws a central distinction between "discovery" and "credibility."[11] 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."[12] 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, 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."[13] Not only does credibility accrue to discovery claims, but researchers, particularly principle investigators of research groups, gain in personal credibility or "credit-ability."[14] 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 the certification process.[15]

My account of inquiry thusfar 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 them in Chapter 8 in terms of the design of scientific institutions, and in Chapter 10 directly in terms of values in the processes of credibility and certification of knowledge. There are three 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 is insufficient for

[11] Grinnell, *Everyday Practice of Science.*

[12] Ibid., 64.

[13] Ibid., 65.

[15] Philip Kitcher, *Science in a Democratic Society* (Amherst, N.Y.: Prometheus Books, 2011).

responsible science; (2) because a focus on the social processes of science can be distracting and somewhat less than entirely helpful for the central tasks of this book, guiding scientists in their work, and guiding citizens in their evaluations of science.

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 social processes of science. 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 3, many of the contingencies faced in these processes have a central impact on the structure of scientific knowledge. Now, of course, there is a sense in which the activities of the laboratory are social rather than individual, but this is not the sense of sociality relevant to the discovery/credibility distinction. (See 1.3.6 and 2.3.2.)

Second, while it is important to understand the larger social processes of science to understand science as such and to understand the role of values in science, accounts that center the social process of credibility or social input into the direction if science can be misleading and unhelpful for the purposes of guiding and evaluating specific scientific projects. I will make this point in detail in Chapters 4 and 7 in response particularly to the views of Helen Longino and Philip Kitcher. At the same time, once an adequate image of science at the inquiry-level is established, accounts at the credibility-level like Longino's or Kitcher's seem to be moving in the right direction. It is not that these accounts are no good, just that they are insufficient without an account at the inquiry-level. Generally speaking, we require individual guidance for scientists and those evaluating and using science, in addition to whatever account of social structures and processes we have.

2.2.9 The Distinctive Value of Scientific Inquiry

Many of the features of inquiry that I have laid out apply to inquiry more broadly, rather than scientific in particular. It is reasonable to ask what, in particular, is special about science as compared to inquiry in general? In general, this has nothing to do with scientific *inquiry* being different in kind from other types of inquiry, but rather because the *scientific* practices that inquiry intervenes on, and so the *subject-matter* or *domain* of inquiry, are different. In particular, the scientific practices of prediction, explanation, and control within a particular domain determine some of the aims of inquiry, and they shape the techniques, protocols, theories and conceptual frameworks that tend to be used in those inquiries. In many cases, these are highly domain-specific, e.g., to molecular biology or stellar astrophysics. But there are some broad resemblances between the activities of the sciences across domains that are worth remarking upon.

Science is systematic rather than piecemeal. That said, the systematicity of science is an aim and practice of science; it is not equivalent to the universal validity or general applicability of science's results, which is rarely had and represents a high level achievement with respect to those aims. Systematicity is valuable for a variety of reasons. It leads to efficiency in the public store of knowledge, allowing terse formulas to be used instead of a large number of ad hoc solutions. It also aims problem-finding, by pushing existing ideas and theories further and further afield.[16]

[16] For more on systematicity, see Hoyningen-Huene, *Systematicity.*

Science is problem-seeking rather than reactive. Whereas in many areas, problems interrupt the thing that is sought (fulfilling the aims of a practice), and inquiry is to be pursued hastily in order to return to that state of equilibrium, the relation is reversed in the case of science. That is, successful, smoothly functioning prediction, explanation, and control is not very interesting to scientists, whereas the failures and problems in that practice are highly motivating. As such, scientists are constantly probing the boundaries of practice, attempting to unsettle it.

Science is controlled inquiry in a more extreme sense that inquiry in some other areas. That is, while commonsense inquiry into immediately pressing practical problems may be satisfied by groping around for a solution that is "good enough" to remove the debris from the smooth road of practice, scientific inquiry takes a high level of care, applies a variety of standards and checks, proceeds deliberatively and reflectively. Despite its messiness, scientific inquirers ought not to come to hasty conclusions. This norm is sometimes troubled by the pressures of scientific careers, the requirement to "publish or perish," to get funding or fail, and lately the pressure to "patent and prosper."

These are distinctive features of science. What makes them distinctly *valuable*? Why should we want to pursue science, given these features? What is *special* about science? As Ian James Kidd points out, it is question-begging to base our assessment of the value of science on its relation to curiosity, understanding, or the unanalyzed "intrinsic value" of science.[17] These are legitimate appeals only insofar as we are within a perspective that values science, unless we can provide further substantiation for them. Different cultures and communities within Western culture already value science more or less (or not at all). The general prestige and authority that are granted to science show that science is valued by us, by why? For one thing, despite the fact that generality is never guaranteed and scientists are always looking to destabilize knowledge by looking problems, those results that to become certifying parts of scientific knowledge are highly robust practices of prediction, explanation,

[17] Ian James Kidd, "The True, the Good, and the Value of Science," *Philosophical Writings: Proceedings of the 13th Durham-Bergen Postgraduate Philosophy Conference*, 2010, 47–55.

and control. When treated with the proper caution and restricted scope, they afford a wide variety of other goals, and particularly they are useful in other areas of inquiry. Nevertheless, Kidd reminds us that there's nothing ultimate, obvious, or inevitable about the value of science.

2.3 Further Issues

2.3.1 Pragmatism and Practical Inquiry

The core components of this account of inquiry can be found in Charles Saunders Peirce's series of articles, "Illustrations of the Logic of Science" (publishes in *Popular Science Monthly* from 1877-1878), and in the writings of John Dewey, particularly his *Essays in Experimental Logic* and *Logic: The Theory of Inquiry*.[18] In 1916, Dewey published his second of three major works on logic, *Essays in Experimental Logic*. The significance of the final essay in that work, "The Logic of Judgments of Practice," has often been overlooked.[19] Therein is stated Dewey's views on science as a practice, the relation of scientific inquiry and value judgment, his account of truth, and indeed Dewey's fundamental definition of his pragmatism. The rhetorical structure of the chapter is somewhat difficult, beginning innocently enough by positing a form of judgment—practical judgment—that has hitherto been ignored or inadequately treated by logicians:

> Propositions exist relating to agenda—to things to do or be done, judgments of a situation demanding action. There are, for example, propositions of the form: M. N. should do thus and so; it is better, wiser, more prudent, right, advisable, opportune, expedient, etc., to act thus and so. And this is the type of judgment I denote practical.[20]

Dewey considers, as an example of practical judgment, the question of buying a suit (MW 8:31). The situation calls for making a practical judgment, e.g., "I should buy that gray suit," or "I should buy this pinstripe suit," or "I should not buy anything today." Facts and value judgments about the different suits—e.g., price, durability, style, comfort, seasonal appropriateness—play a significant role in coming to that judgment.

Judgments of practice have a variety of features that Dewey enumerates throughout the chapter.[21] Judgments involve an open, incomplete future situation (an indeterminate situation, as described above); without such a situation, the judgments would be otiose. Judgments of practice modify their subject matter, because they require the subject-matter be acted upon. They make a difference for better or worse by way of those modifications. Judgments of practice carry an assertion of both the rationality and acceptability of the end pursued and the possibility and efficacy of the means to reach it. They require (tentative) factual propositions that are accurate, relevant, and adequate. They propose a course of action, rather than (merely) describing a state of affairs.

[18] Charles Sanders Peirce, "The Fixation of Belief," *Popular Science Monthly* 12, no. 1 (1877): 1–15; Charles Sanders Peirce, "How to Make Our Ideas Clear," *Popular Science Monthly* 12 (january 1878): 286–302; Charles Sanders Peirce, "Deduction, Induction, and Hypothesis," *Popular Science Monthly* 13 (august 1878): 470–82; John Dewey, *Essays in Experimental Logic*, ed. D.M. Hester and R.B. Talisse (Southern Illinois University Press, 1916 [2007]); John Dewey, *Logic: The Theory of Inquiry*, ed. Jo Ann Boydston, vol. 12, The Later Works of John Dewey (Southern Illinois UP, 1991, 1938).

[19] John Dewey, "The Logic of Judgments of Practice," in *The Middle Works, 1899–1924*, ed. J. A. Boydston, vol. 8 (Carbondale: Southern Illinois University Press, 1915).

[20] Ibid., 14.

[21] See Jennifer Welchman, "Logic and Judgments of Practice," in *Dewey's Logical Theory: New Studies and Interpretations*, ed. F. Thomas Burke, D. Micah Hester, and Robert B. Talisse (Vanderbilt Univ Press, 2002) for discussion of these features.

Judgments of practice have modal qualities referring to, e.g., possibility, necessity, permissibility, futurity, betterness, etc.

Dewey points out that judgments of practice have peculiar truth conditions:

> Their truth or falsity is constituted by the issue. The determination of end-means... is hypothetical until the course of action indicated has been tried. The event or issue of such action is the truth or falsity of the judgment... In this case, at least, verification and truth completely coincide.[22]

[22] Dewey, "The Logic of Judgments of Practice," 14.

If my judgment was "I should buy *this* suit," then that judgment was *true* if doing so worked out;[23] if the consequences of that judgment are satisfying, they fulfill the needs that prompted buying the suit, they do not have unintended negative consequences, if I do not feel regret for my decision, then it was the right to say that I should buy it. What else could the truth of a judgment of practice involve? And indeed, there is a straightforward way in which truth of the judgment is due to correspondence—the judgment corresponded with the future consequences intended by the judgment.

[23] Dewey rejects the sort of non-cognitivism about practical judgment that would argue that such judgments are not candidates for truth or falsity, though admittedly the significance of this particular judgment is minor.

Here is where Dewey makes the clever rhetorical shift that has often been missed or misunderstood. Having established judgments of practice as a particular kind of judgment, with interesting features and truth-conditions, different from "ordinary" judgment, Dewey proposes the following hypothesis:

> We may frame at least a hypothesis that all judgments of fact have reference to a determination of courses of action to be tried and to the discovery of means for their realization. In the sense already explained all propositions which state discoveries or ascertainments, all categorical propositions, would be hypothetical, and their truth would coincide with their tested consequences effected by intelligent action.[24]

[24] Ibid., 22.

This is Dewey's definition of *pragmatism*: pragmatism is the hypothesis that all judgments are judgments of practice. What he originally forwarded as a special form a judgment (practical) with a logic different from ordinary (descriptive, theoretical) ones, he ends up arguing is in fact fully general, and thus that the traditional ideas about the form and logic of judgments are empty.

Pragmatism is the view that all inquiry is practical inquiry, and all judgment is practical judgment, i.e., concerning what is to be done.

Based on this point, the connection to science should be clear.

> 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. Every such proposition in the concrete is a practical proposition. Every such proposition of inquiry, discovery and testing will have then the traits assigned to the class of practical propositions. They imply an incomplete situation going forward to completion, and the proposition as a specific organ of carrying on the movement.[25]

[25] Ibid., 64.

Science is a type of inquiry, inquiry ends in judgment, and all judgments are judgments of practice. As Dewey puts it in one of the later sections of "The Logic of Judgments of Practice," science is a "practical art."

Namely, science is the practice of systematized problem solving.

From a pragmatist point of view, science is a practice, and scientific inquiry, like all inquiry, is an attempt to resolve an indeterminate situation that arises in that practice. The form of the final judgment that resolves an inquiry is what Dewey has called a "judgment of practice." Like all practical judgments, scientific judgments are true or false according to their consequences. This is not the vulgar pragmatism that would measure the truth of a claim according to whether the consequences of believing it are congenial. Rather, the consequences in question are tied to the consequences *intended* by the judgment. As all judgments involve a solution to a particular problem and a transformation of an indeterminate situation, then the truth of that judgment is determined by whether the transformation of the situation, carried out, resolves the problem and eliminates the specific indeterminacy in question.[26]

[26] Of course, such resolutions are in a sense temporary—as situations change, causing new problems to arise, the matter will eventually need to be revisited. If there is any pragmatist sense to be made of William James's talk of "temporary truths" (William James, *Pragmatism: A New Name for Some Old Ways of Thinking* (New York: Longmans, Green,; co., 1907)), it must be in terms of the contextual nature of judgment.

2.3.2 The Sociality of Science

The account of inquiry I've mentioned here doesn't emphasize the social nature of science. That is, though I have discussed the central importance of the credibility and certification processes to the way that science works, and particularly in the way that some scientific results become public knowledge and have an influence over practices like education and public policy. Nevertheless, I have dwelled primarily on particular inquiries, rather than these larger social processes, and throughout the rest of the book, I will mostly focus on responsibilities and values in the process of inquiry, which is largely the sphere of individuals and small groups of researchers engaging. I think this is the area that needs the most attention, where scientists and the public need the most guidance. So many decisions are made during inquiry that are rather opaque if we look just at the discourse about credibility in the published literature, for instance.

There are a few ideas about the social side of science that can flesh out the pictures of inquiry and credibility that I have mentioned previously. This involves making a distinction between two distinct meanings of the claim "science is social," and it requires relying on the basic outlines of distributed cognition theory.[27] In terms of the former, the claim that science is social can mean two different things, which can be conflated, but that I think it is useful to distinguish. The first is a claim that science is a collective social process, engaged in by groups of scientists rather than individuals working in isolation. I will call this "collectivity" and translate the associated claim as "science is a collective enterprise." The second claim that we might mean by "science is social" is the claim that science is highly dependent on or connected to the surrounding society and culture in which it takes place. I will call this "sociality" and

[27] E. Hutchins, "The social organization of distributed cognition," in *Perspectives on Socially Shared Cognition*, ed. Lauren B. Resnick, John M. Levine, and Stephanie D. Teasley (Washington, DC: American Psychological Association, 1991), 283–307; Edwin Hutchins, *Cognition in the Wild* (Cambridge, Mass.: MIT Press, 1995); Nersessian et al., "Research Laboratories as Evolving Distributed Cognitive Systems."; Ronald N. Giere, "Scientific Cognition as Distributed Cognition," in *The Cognitive Basis of Science*, ed. Peter Carruthers, Stephen Stich, and Michael Siegal (Cambridge University Press, 2002), 285–99; Giere and Moffatt, "Distributed Cognition"; P.D. Magnus, "Distributed Cognition and the Task of Science," *Social Studies of Science* 37, no. 2 (2007): 297–310; Matthew J Brown, "Science as Socially Distributed Cognition: Bridging Philosophy and Sociology of Science," in *Foundations of the Formal Sciences Vii: Bringing Together Philosophy and Sociology of Science*, ed. Karen François et al., vol. 32, Studies in Logic (London: College Publications, 2011).

translate the associated claim as "science is socially situated."

The collectivity of science is not really a challenge to the seemingly individualistic perspective I have adopted here, thanks to the resources provided by distributed cognition theory. Both the team work of the science lab and the credibility process involve groups of individuals working together in a shared cognitive task (problem-solving inquiry or assessing credibility), and distributed cognition theory (d-cog) allows us to analyze the behavior of that group analogously to the way we would analyze the behavior of an individual agent. Whether the decision-making (and the value judgments) are the work of an individual or, more likely, a small group, the basic structure of the task remains the same. D-cog allows us to abstract away in some respects. I will use the term "primary collectivity" to refer to the level of the research group or laboratory team engaging in collective inquiry, whereas I will use the term "secondary collectivity" to refer the level of the field or discipline engaging in credibility assessment as a larger-scale collective process.[28]

[28] To be a little more precise, d-cog treats all cognitive processes as the use and transformation of representations across representational media, and so treating groups as cognitive processes is not an analogy, but a literal case of cognition, even a paradigmatic one. Whether and which systems can be treated the same as processes is a further issue.

On the side of sociality, I will use "initial sociality" to refer to the connections to a particular society that inform the individual sociocultural background of individual scientists. Initial sociality tends to be important in terms of the cultural values and vocational goals that the particular scientist brings to scientific practice. Insofar as certain social-institutional arrangements are also relatively fixed in the past relative to a particular inquiry, we can regard those arrangements as part of the initial sociality of science as well. I will use "final sociality" to refer to the interaction between science and society that come as a result of scientific inquiry, around the moments of certification of knowledge, dissemination, and application. Some have also argued that there is significant or their ought to be greater interaction between science and society during the inquiry process, what I will call "midstream sociality." In later chapters, I will discuss whether and to what extent midstream sociality might be desirable.

Table 2.1: **Collectivity versus Sociality**. The two senses in which science is social.

Collectivity	**Sociality**
Primary Collectivity	Initial Sociality
Secondary Collectivity	Midstream Sociality
	Final Sociality

2.3.3 Radical Pluralism

Some may balk at the one-sized-fits-all picture of science I have provided. After all, didn't Kuhn show us that different scientific paradigms function in different, incomparable ways?[29] Didn't Feyerabend show us that there is no single "scientific method" guiding all scientific inquiries, but rather a variety of different local practices?[30] Modern-day pluralists as well have shown us that different approaches, different styles of reasoning, disagreements about theory and concepts, and many other kinds of plurality and disunity are rampant in the sciences.[31] In the face of that, my picture of *the method* of scientific inquiry, despite all of the situational, practice-oriented aspects of the account, may strike you as too naive.

[29] Kuhn, *The Structure of Scientific Revolutions.*

[31] Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*; P.L. Galison and D.J. Stump, eds., "The Disunity of Science: Boundaries, Contexts, and Power" (Stanford University Press, 1996); Stephen H Kellert, Helen E Longino, and C. Kenneth Waters, *Scientific Pluralism* (Minneapolis, MN: University of Minnesota Press, 2006); Chang, *Is Water H_2_O?*

Diversity, dissent, and disunity are real and crucial aspects of science. Nevertheless, there is something valuable about insisting on a core image of inquiry. The account I have given is quite schematic; there may be a great diversity of ways of satisfying the functional roles connected with the five phases of inquiry between different fields, time periods, and groups. Many different local techniques and methodologies might be used in observation and experimentation; hypotheses may take the form of causal claims, models, mathematical equations, or narratives, and they may be more or less tightly connected with background theories of various sorts. This account of inquiry doesn't require the kind of substantive assumptions about methods or theories that are worrisome from the pluralist standpoint.

2.3.4 The Large-Scale Dynamics of Theory Change

Thomas Kuhn, Imre Lakatos, Paul Feyerabend, Larry Laudan, and others are well-known for their dynamical accounts of science. They differ from the account of inquiry that I've provided in virtue of their scale and scope. These sorts of accounts of science focus on the largest scale of scientific disciplines, the most central or fundamental theories shared by a large number of scientists, and the dynamics at such a scale. These accounts tend to elide the processes of inquiry and credibility. That is, large-scale accounts of the dynamics of theory-change do discuss particularly central experiments, conceptual innovations, and arguments in science, but only in the context of their influence over the large-scale social processes within the field. This is not a strike against such images of science; these accounts have different scale and purpose from the one we're currently considering, for the most part.

2.3.5 The Autonomy of Experiment

An important turn in philosophy of science, sometimes called "the new experimentalism," can be summed up by Ian Hacking's memorable phrase: "Experimentation has a life of its own."[32] The philosophers of science in this tradition insist on the autonomy of experiment from its role in hypothesis- and theory-testing that had been assigned to it by most philosophers of science. They point to a variety of experimental practices with purposes independent of the testing of theory. This seems to pull against the role for experimental testing identified in my account of inquiry. Have I neglected their lessons? How can I reckon with the new experimentalists' insistence on the autonomy of experiment?

[32] Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983).

Partly, the disagreement is verbal. The new experimentalists call any controlled manipulation in the lab (any other kinds of controlled intervention processes, perhaps) an "experiment." This fits just fine with the usage of the term within science. But this account of experiment is *essentialist* rather than *functionalist*, i.e., it regards some activity or happening as an experiment in virtue of inherent features of the activity (such as taking place in a laboratory, being controlled, or being a manipulation / intervention). On the other hand, the phase of inquiry that I have called "experimental testing" has a mainly functional definition: it is focused on the tentative application of the hypothesis as problem-solution. While I agree with the new experimentalists to a point, that *usually* and *ideally* this role will be filled by an actual manipulation of intervention, my discussion above of "degenerate" experiments shows the functional nature of this distinction.

What the new experimentalists mean by "experiment" serves several distinct functions in scientific practice: the problem-finding attempts to push the limits of prediction, explanation, and control that precede inquiry proper, the phase of inquiry I've called "observation," and experimental testing as such. We might add training and education to this list, as well as testing and calibration of equipment. Each of these stages has been discussed in detail above. While for some purposes, linking them all together under the heading of "experimentation" may usefully distinguish them from other kinds of activities, for the purposes of distinguishing different aspects of inquiry, or the problem-finding activity that often precedes scientific inquiry, it is important to keep them distinct. I know of no better name for the third role than "experimental testing," which is why I reserve that name for that role. The key, for me, is accurately distinguishing the different functions within inquiry, not legislating terminology.

The last potential substantive argument with the new experimentalists may be the centrality of hypotheses and theories in inquiry. I concede to the experimentalists that much of scientific inquiry does *not* involve testing of major or fundamental theories. However, I insist that guiding hypotheses are central to problem-solving inquiry. Without a guiding hypothesis, the experimentalist is merely noodling around in the lab. The new experimentalist argument may serve as a reminder that we should be open-minded about the kind of hypotheses play a role in different kinds of scientific inquiries.

2.3.6 Reflexivity of this Philosophy of Science

The model of inquiry I have described in this chapter is a hypothesis, in the sense of the model itself. This hypothesis is meant to contribute to the resolution of the perplexities denoted and characterized in the introduction: namely, how should we understand the nature of science such that the various interactions of science and values can be reckoned with, and adequate guidance be provided to scientists so that they can be responsible in the face of those interactions, as well as guidance for the rest of us in evaluating the way that scientists discharge those responsibilities.

The model of inquiry provided here, like many background theoretical resources in other inquiries, was developed for other purposes, and builds upon a prior tradition of inquiry. As mentioned above, the original thinking behind this image of science derives from the ideas of Peirce and Dewey. In prior work, I have developed their work further in order to address contemporary concerns about the nature of evidence, the use of models in science, and the evaluation of the significance of various scientific research projects.[33] These prior uses lend only tentative plausibility to use of the model for new purposes. In the context of this book, the model of inquiry is justified not only or primarily by the facts of the case (facts about prior inquiries, successful and unsuccessful) or systematic reasoning about the nature of science or knowledge, but first and foremost by its ability to resolve the perplexities of values in science. This does not need to be the best account of inquiry for other, arbitrary purposes, though such systematicity has its value.

[33] Matthew J. Brown, "Science and experience: A Deweyan pragmatist philosophy of science" (PhD thesis, University of California, San Diego, 2009); Matthew J. Brown, "Models and perspectives on stage: remarks on Giere's Scientific perspectivism," *Studies In History and Philosophy of Science Part A* 40, no. 2 (2009): 213–20; Matthew J. Brown, "Genuine Problems and the Significance of Science," *Contemporary Pragmatism* 7, no. 2 (2010): 131–53.

What's more, this account of inquiry is fallible and revisable. Though I have based this work on the ideas of competent scientists and philosophers of science whose aims are in some ways close to mine, as well as on my own experiences in science and knowledge of a wide variety of cases in the history, sociology, and philosophy of science, I have far from a synoptic view of the workings of science. The model is itself normative, stating in certain ways an ideal of scientific practice, but (as we shall see later on), this is no barrier to its revisability in the face of new evidence. Though normative, it seems to encode norms discovered in the course of a long history of scientific practice, though these lessons are not always uniformly appreciated by the practitioners. (Culture is difficult to see from the inside, as is water for the sea-dweller.) The failure of this model of inquiry to function as the background of an adequate account of science and values, as well as strong evidence against the applicability of the model to a variety of scientific inquiries, would both speak heavily in need of its revision.

2.4 Values in Scientific Inquiry

The account of scientific inquiry I have provide in this chapter is to a large degree independent of the kinds of questions about values in science that have motivated philosophers of science to explore the issue in recent decades. It may strike you as a strange place to start. The reason I have started here is that so much confusion in philosophy of science arises from taking for granted certain fundamental ideas about the nature of inquiry, but these so often begin from a narrow image of science whose emphasis is ill-suited to the problem at hand. Science as practical inquiry is an inclusive image that allows us to understand the nuances of scientific practice and the role of values in science.

In the next chapter, I turn to attempting to refine the perplexities of the relation of values and science identified in the first chapter into a clear statement of the problem. That is, I provide a general argument that science cannot and should not be value-free, and that scientists have a responsibility to weigh values in making scientific judgments.