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**Science and Experience**  
**A Deweyan Pragmatist Philosophy of Science**

A dissertation submitted in partial satisfaction of the  
requirements for the degree  
Doctor of Philosophy

in

Philosophy

by

Matthew J. Brown

Committee in charge:

Professor Paul Churchland, Chair  
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Professor Michael Cole  
Professor Gerald Doppelt  
Professor Roddey Reid  
Professor Donald Rutherford

2009

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Co-Chair

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Chair

University of California, San Diego

2009

## DEDICATION

In memory of Professor Jon J. Johnston (1928-2008)

Teacher, Mentor, Friend

## EPIGRAPH

*To work exclusively within the context provided by the sciences themselves is to ignore their vital context. The place of science in life, the place of its peculiar subject-matter in the wide scheme of materials we experience, is a more ultimate function of philosophy that is any self-contained reflection upon science as such.*

— John Dewey, *Context and Thought* (LW 6:19-20)

## TABLE OF CONTENTS

Signature Page . . . . .	iii
Dedication . . . . .	iv
Epigraph . . . . .	v
Table of Contents . . . . .	vi
List of Figures . . . . .	ix
Preface . . . . .	x
Acknowledgements . . . . .	xix
Vita and Publications . . . . .	xxviii
Abstract of the Dissertation . . . . .	xxix
Chapter 1    Introduction . . . . .	1
1.1    Whatever happened to American Pragmatism? . . . . .	1
1.2    The Transformation of Philosophy of Science, 1930–1960 . . . . .	4
1.3    The Current Impasse as Legacy of Late Logical Empiricism . . . . .	14
1.4    Back to Dewey: A Counterfactual History . . . . .	17
Chapter 2    John Dewey’s Philosophy of Science . . . . .	21
2.1    Introduction . . . . .	21
2.2    Background . . . . .	26
2.2.1    Lived and Historical Context . . . . .	26
2.2.2    Guiding Principles . . . . .	30
2.2.3    Dewey’s Philosophical Projects . . . . .	34
2.2.4    The relation of logic to science . . . . .	36
2.3    Anti-skeptical fallibilism . . . . .	38
2.4    Anti-foundationalism without coherentism . . . . .	40
2.5    Inquiry defined . . . . .	42
2.6    The importance of the situation . . . . .	45
2.7    Normative force . . . . .	47
2.8    The Pattern of Inquiry . . . . .	49

	2.8.1	Facts and problems . . . . .	50
	2.8.2	Ideas and solutions . . . . .	54
	2.8.3	The Coordinate Development of Facts and Ideas . .	55
	2.8.4	Reasoning and Conceptual Frameworks . . . . .	59
	2.8.5	The Necessity of Experiments . . . . .	60
	2.8.6	Final Judgment and the Close of Inquiry . . . . .	62
	2.9	The role of values . . . . .	63
	2.10	Summary . . . . .	69
Chapter 3		Inquiry and Evidence . . . . .	70
	3.1	Introduction . . . . .	70
	3.2	Evidence and the Pattern of Inquiry . . . . .	72
	3.2.1	The $E \Rightarrow H$ (Non-Dynamical) Model . . . . .	72
	3.2.2	Dynamical models . . . . .	74
	3.2.3	The Inquiry-Model . . . . .	75
	3.2.4	Snow on Cholera . . . . .	78
	3.2.5	Evidence on the Inquiry-Model . . . . .	85
	3.3	Some Problems of Evidence Dissolved . . . . .	87
	3.3.1	Theory-Ladenness and the Experimenter's Regress .	87
	3.3.2	Discordant Evidence . . . . .	96
	3.3.3	The Value of Robustness . . . . .	101
	3.3.4	Evidence for Use . . . . .	106
	3.4	Real Perplexities of Evidence . . . . .	108
	3.4.1	"Evidence"-Based Policy . . . . .	109
Chapter 4		Genuine Problems and the Significance of Science . . . . .	116
	4.1	Introduction . . . . .	116
	4.2	Why Significance? . . . . .	119
	4.3	Kitcher's Theory of Significance . . . . .	125
	4.3.1	Significance Graphs . . . . .	125
	4.3.2	Problems for Kitcher's Theory . . . . .	127
	4.4	The Pragmatist Model of Inquiry . . . . .	129
	4.4.1	Peirce's Insight . . . . .	130
	4.4.2	Dewey's Elaboration of the Model . . . . .	131
	4.5	Genuine Problems and Scientific Significance . . . . .	134
	4.6	Consequences for Science & Democracy . . . . .	138



Chapter 5	Pluralism, Perspectivism, and Pragmatism . . . . .	140
5.1	Giere's <i>Scientific Perspectivism</i> . . . . .	142
5.2	Feyerabend on representation in art and science . . . . .	149
5.3	Pragmatism on purpose and inquiry . . . . .	155
5.4	Conclusion . . . . .	161
Chapter 6	Answering Rorty . . . . .	164
6.1	Introduction . . . . .	164
6.2	Rorty's Attack on Epistemology . . . . .	164
6.2.1	Three Versions of 'Epistemology' . . . . .	167
6.2.2	Rorty's Critique of Global Epistemology . . . . .	169
6.2.3	Hermeneutics as an Alternative to Epistemology . . . . .	176
6.2.4	Rorty's Critique of Dewey . . . . .	178
6.3	Feyerabend Against Epistemology . . . . .	195
Chapter 7	Conclusion: Transforming Experience . . . . .	197
7.1	Science in a Precarious World . . . . .	197
7.2	Transforming Experience . . . . .	199
7.3	Changing the World . . . . .	201
7.4	Objects, Events, and Meaning . . . . .	203
7.5	Existence, Value, and Criticism . . . . .	206
Appendix A	Epilogue: Science as D-Cog . . . . .	209
A.1	What is D-Cog? . . . . .	210
A.2	Science as D-Cog . . . . .	213
A.3	The Cognitive and the Social . . . . .	214
A.4	Challenges . . . . .	217
A.5	Prospects for a D-Cog Theory of Science . . . . .	220
	Bibliography . . . . .	224
Index	. . . . .	233

## LIST OF FIGURES

Figure 2.1: The Pattern of Inquiry . . . . .	50
Figure 4.1: Toy Significance Graph . . . . .	126
Figure 4.2: Measure of Intrinsic Significance. . . . .	137
Figure 5.1: Observational Perspectives on the Milky Way . . . . .	145
Figure 5.2: Giere’s Models-Based Account of Theories . . . . .	146
Figure 5.3: Rules of Projection for Single-Point Perspective . . . . .	150
Figure 5.4: Brunelleschi’s Perspective Experiment . . . . .	151
Figure 5.5: Brunelleschi’s “Stage” . . . . .	153
Figure 5.6: The CERN Stage . . . . .	154
Figure 5.7: Feyerabend’s Dramatic Model of Scientific Representation . . . .	154
Figure 5.8: Dewey on the Temporal Development of Inquiry . . . . .	158
Figure 5.9: Dewey on the Production of Judgment . . . . .	159
Figure 5.10: Dramatic-Perspectivist Model of Scientific Inquiry . . . . .	160
Figure A.1: Longhand Multiplication . . . . .	212
Figure A.2: Chemical Formulae . . . . .	213

## PREFACE

# Why Dewey? Why Philosophy of Science?

Philosophy of science is headed towards an impasse. The way of thinking about science that has been passed down to us is woefully inadequate for our present purposes. Given the questions that now interest us, this legacy creates more problems than it solves. Further, it tends to alienate us from science, rather than make science seem actually or potentially connected to our lives. At best, it renders science strange, and at worst, it renders it dangerous and frightening. Rather than a set of practices with a human face, striving after goals comprehensible to mere mortals, science has been treated as some or another abstract system of ideas and technocratic processes of measurement. I think it likely that the promotion of thinking about science in this way lies behind the reaction to science within the humanities that culminated in the so-called “science wars.”

Philosophy of science has never been given to a global orthodoxy (talk about “the received view” notwithstanding), so any talk about what the tradition has handed down will necessarily proceed in terms of family resemblances, common trends and shared styles of thinking, rather than a coherent body of doctrine, a single method, or a unified research program. The way in which I attempt to lay bare the common assumptions within the tradition—the source of the mistakes—is by providing a comprehensive alternative, very different from the approaches that have been the main life of philosophy of science. This is my main aim, to provide such an alternative, which I have discovered in the work of the great American philosopher John Dewey.

Dewey’s long-forgotten philosophy of science was one of the most important players in the formation of that area of specialization, and it wouldn’t be an exaggeration to suggest that, as in most areas of intellectual culture, no public discussion

of science in the inter-war period passed without seeking the input of Dewey and his students. Nevertheless, Dewey's philosophy of science came to be eclipsed by the rise of logical positivism in America and the institution of so-called "analytic" philosophy. The reasons for this shift are many and complicated, and I give a partial treatment of them in the introduction; they include a growing fascination with the new formal logic, the perceived "scientific" credentials and promise of progress in "analytic" method, and the increased danger of doing normatively-laden and politically relevant philosophy in the context of McCarthyism.

Post-war philosophy of science thus turned primarily to "self-contained reflections on science as such" and away from the "vital context," their place in our lives and our experience.<sup>1</sup> Today, we are seeing a return to these concerns—to questions about the use of science, the relation of science to policy and action, the ethics of science, the role of human cognition and purposes within science, even to science education. Unfortunately, we come to the table with the resources developed for the former task, and as so often when one imports the tools specialized for one pursuit into a very different sort of pursuit, they fail to live up to the task—a hammer does a poor job at chopping wood. The reason that Dewey provides such an invaluable resource at this stage is that *his* tools were developed for precisely the sort of aims that now occupy us. The reason to try and get Dewey right is that he has covered much of this ground before us, carefully and without the tendency to fall back on old, unhelpful ways of thinking about such things. Dewey's philosophy of science is far from complete or perfect; nevertheless, it provides a better starting point than the ways we've come to think of philosophy of science in the half-century since his death.

Why focus on Dewey's philosophy of science, though? Especially since it is worked out largely in the context of his dense and intimidating *Logic*, a tome that has been frequently panned by such leading lights as Russell and Carnap? Why not focus,

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<sup>1</sup>See the epigraph, from *Context and Thought* (LW 6:19-20).

instead, e.g., on the analogies he draws between democracy and science in works like *The Public and Its Problems* or *Freedom and Culture* or his writings on science and education? Such texts provide important clues for understanding and using Dewey's ideas. But the core of these ideas is not fully comprehensible nor defensible without an understanding of Dewey's systematic approach to the philosophy of science—a part of his work that is so central that, paradoxically, it never receives a fully independent treatment. Instead, one sees it refracted in every area of his philosophy, from his work on education to logic to aesthetics. In order to address the problems we now face, we have to systematically rethink our views about science and its context; if we are to follow Dewey's way of doing so, we have to follow him all the way down.

## Science, Context, and Life

In *The Structure of Scientific Revolutions* (Kuhn (1996), see also Doppelt (1978)), Kuhn claims that scientists in different paradigms are committed to different problems. Their standards of justification and problem-solution are different. They speak a different language from one another. Kuhn even goes so far as to claim that these different scientists (in some sense) practice in different worlds.

I think we should ask whether these claims make sense when we start thinking of our scientists not as creatures of the laboratory, but instead as full human beings living amongst the rest of humanity, and the laboratory not as some arcane epistemological space, but a human social institution. Do scientists really speak a different language than the rest of us? Perhaps when we hear them speak words we *think* that we understand, they're really talking past us? Or perhaps scientists are bilingual, forced to translate their ideas into our primitive language, distorting much of the content in the process? When we elect to give research grants to scientists, can we legitimately ask that they justify their results with reasons that we would accept, or must we allow them to insist that they have their own problems to solve and their

own standards of solution? When we place our children's education in the hands of scientists, do we expect them to be indoctrinated, or do we expect the teachers to provide reasons and arguments? When scientists go from the lab to the home, do they travel between different worlds? Are they forever trapped in a different world from their spouse and children?

These overwrought questions draw out a problem I want to bring to light: the problem of the continuity of scientific practice and everyday life. Kuhn provides a particularly clear example of how dissatisfying many discussions of the nature of science are when we start asking about the relationship between science and experience. The radical break between scientific and everyday practice makes it difficult to see how science ever arose out of human concerns, not to mention how it could ever touch base with human life at present. What is the place of everyday human experience and practical life in knowledge and cognition? How does science arise from and feed back into everyday life? What is the role of experience and implicit knowledge in science and scientific method? What is the relationship between science and technology? How does science appropriate the more plastic elements of life and experience into more rigid, formalized structures? Many approaches to the philosophy of science from positivism to today have obscured these questions or made them impossible to answer. Too often have scientists and philosophers been eager to assert a radical break between science and the rest of human life.

I want to discuss a broadly pragmatist approach to the nature of science and human knowledge that, by focusing on the continuity of scientific practice and lived experience, helps us to ask and answer these questions. In order to cope with these questions, we must deal with a number of problematic but interrelated 'oppositions': theory and practice, concrete and abstract, cognition and action, scientific knowledge and everyday cognition. While it would be an unmanageable task to attempt a definitive statement on the nature of each of these relations, to treat one without keeping an eye on the others would also seem impossible. Just as human cognition

generally grows out of and remains continuous with the rest of life, scientific practice, perhaps our most sophisticated cognitive activity, likewise grows out of and remains continuous with human cognition and life generally. By trying to see continuity where others have seen only ‘oppositions,’ I hope to find a more satisfactory approach to understanding science and knowledge.

## Historical Exegesis and Philosophical Argument

My project threatens to fall between two stools.<sup>2</sup> On the one hand, I spend an awful lot of time doing historical exegesis on a few characters from the history of the philosophy of science, most notably John Dewey. On the other hand, I am engaged in ongoing arguments with various contemporary figures and attempts to solve contemporary problems. Readers interested in the history for its own sake may find that work frustratingly instrumentalist; while I endeavor to “get the history right,” to faithfully portray the ideas of those who I take so seriously, I do so always with an eye to the future, to problems to be solved and philosophical insights to be gained. The committed Dewey scholar may feel that my uses of Dewey will inevitably lead to abuses (as it did for Rorty). On the other hand, readers from contemporary philosophy of science may find some of the arguments frustrating; where they expect a reason, or a case study from the history of science, they are instead handed Dewey exegesis.

Part of the explanation for my methodology will be cleared up in my “counterfactual history” argument in the introduction. But allow me to provide a more general explanation of what I think I am up to. In part, my mode of argument betrays a certain kind of methodological predilection: I don’t believe that philosophy should always be a matter of piecemeal contributions to a paradigm or widely-accepted theory. I am pessimistic about the possibility of philosophical progress by essentially

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<sup>2</sup>Thanks to Dick Arneson for the suggestive metaphor.

*dialectical* methods, and in any case, I am not overly interested in making small moves within the contemporary dialectic. Nevertheless, I think that contemporary discussions are often, in more or less apt ways, struggling with real perplexities, and it is those underlying concerns that I seek to address. Furthermore, I share John Dewey's view that often the way to resolve a perplexity is to revise our way of thinking about it, rather than answer the question directly. The source of some problems may be the terms of the question in which the problem is asked, rather than something inherent in the model itself.

This is precisely the sort of thing that the history of philosophy teaches us to do: Take the long view on our intellectual history. Attempt to understand the startling variety of different philosophical views that have held sway over time. Attempt to render comprehensible ideas and conceptual frameworks that on first appearance are bizarre and unbelievable, to live inside them and understand their logic and motivations. These pursuits provide the flexibility needed to approach contemporary problems. Add to this the pragmatist idea that an essential component of *understanding* an idea is seeing what it can *do*, or how one can use it—not just practically but intellectually—and you might be quickly led, as I have, to seeing the study and use of the history of philosophy as an invaluable resource for (perhaps even essential to) philosophical inquiry itself.<sup>3</sup>

## Case Studies and Philosophic Method

In the current climate, it is at the very least imprudent to engage in a work of philosophy of science as ambitious and broad as this one without including one or more detailed case studies, and I have routinely been criticized by my colleagues in informal presentations of this work for not having done so. My reticence to engage

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<sup>3</sup>My thinking of this has been influenced in no small part by Don Rutherford's unpublished work on the relationship between the history of philosophy and philosophy itself.



in case studies is not simply due to lack of interest—I *like* history of science, though I think most philosophers of science are much worse at it than they think they are—but rather because I think they distract from the point of this project, and I believe in general that they tend to support a false consciousness about how philosophy of science works. As I’ve already mentioned, my goal is to elaborate a systematic view of science—one that, I argue, best captures what is explicit and implicit in a wide swath of Dewey’s writings about science—and show how this approach better illuminates a variety of recent problems posed or issues addressed by philosophers of science. It is an interesting question how well my approach will illuminate particular episodes in science, though that is not the point of the theory. It is not the analysis of science *as such* that holds my interest, but rather the place of science in the materials of life and experience, and so it is the “big questions” about the human face of science that I hope to address. If the best theory for the micro-analysis of scientific cases is otherwise, then we should be pluralists about frameworks in science studies, though as I argue in the epilogue, my suspicion is that the right account of this latter will be drawn either from sociology or cognitive science, not from philosophy.

The prevalence of case studies in contemporary philosophy of science represents at once an advance and a potential mistake. It is a great advance over earlier logicist approaches to the analysis of science by virtue of engagement with the actual practice of science. Replacing the “rational” reconstruction of science according to arbitrary “a priori” norms with a careful study of how scientists actually do what they do and why gives much-needed content to philosophy of science. The problem comes in the implicit assumption that scientific case studies form something like an inductive or hypothetico-deductive base for work in the philosophy of science. The presumption seems to be that philosophers of science make *generalizations* from particular cases, or *predictions* to be tested by comparison to specific cases, or something of the sort.

This presumption seems mistaken on many fronts. For one, it mistakes phi-

losophy for descriptive sociology or history.<sup>4</sup> Unless your metaphilosophical views are a certain extreme form of naturalism,<sup>5</sup> it seems uncontroversial to say that whatever the concerns of philosophy of science (understanding, interpretation, normative methodology), they are different from the concern of producing an inductive generalization or a predictive theory of the practice of science. Relatedly, the presumption suggests that the skill-set of philosophers is well-suited to the descriptive analysis of scientific practice (which seems false, if that skill-set is the one that an ordinary Ph.D. program in philosophy seeks to inculcate), or that those who are well-versed in scientific practice are particularly well-placed to make general philosophical conclusions about science (which anyone familiar with the philosophical pronouncements of most—or even particularly broad-minded—scientists will be inclined to turn a skeptical eye to). Furthermore, the presumption seems to promulgate an idea that almost every philosopher of science will reject with respect to *scientific* theories: namely, that a single false prediction is sufficient to reject the theory.<sup>6</sup> Finally, the trend of demanding extensive case studies suggests that only those “close to the ground,” i.e., intimately engaged with specific concrete cases, have any right to theorize. This presumption, if applied to experimentalists versus theoreticians, would have doomed the discipline of physics from the start, and something like this presumption is one of the great weaknesses in many of the special sciences.

Despite the mistaken nature of this presumption, the rise of concrete cases in philosophy of science represents a real advance, and I have tried to indicate throughout the bearing of my hypotheses on specific cases (though often the cases have to do less with science-in-itself and more with the lived context of science). As part

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<sup>4</sup>Though it seems to me that these two endeavors ought to be closely related to one another (see the epilogue), they should not be confused with one another.

<sup>5</sup>See chapter 6 for an argument for why we cannot simply *eliminate* normative-epistemological considerations from philosophy of science.

<sup>6</sup>The greatest weakness in Feyerabend’s philosophy of science is his occasional tendency towards mad-dog Popperian falsificationism about *philosophical* theories, even after he has rejected Popper’s views about scientific theories.

of aiming towards generality, and to avoid the false consciousness just discussed, I have tended towards brief discussions of *illuminating examples* rather than extended discussions of cases. The greatest exception to this trend comes in chapter 3, which discusses two lengthier examples: John Snow on cholera, and the case of gravity waves. The former provides my clearest example of a single process of inquiry, but no claims are advanced about the nature or structure of inquiry in any way *based* on the case. The latter is necessary to the clear exposition of H.M. Collins' argument about the experimenter's regress, since it is the context from which Collins' ideas arose.

It is my hope that this method will fruitfully contribute to my aims. First and foremost, my aim is to provide for contemporary philosophers of science a new option for thinking about the nature of science which meets the call currently being made for a new image of science. Second, my aim is to make clear, to philosophers, scientists, and perhaps even laypersons, the lessons of science for human life, personal and social. Finally, my aim is a *réhabilitation* of John Dewey into the canon of major philosophers of science.

*Matthew J. Brown*  
*La Jolla, California*  
*April 2, 2009*

## ACKNOWLEDGEMENTS

It takes a village!

This document has been made possible and made better by a long list of people; surely, if I was less stubborn in ignoring their good advice, it would be even better. I have discussed the topics of this dissertation with a great many people, too many to name. Foremost among them are the various members of the UCSD Pragmatism Reading Group, which I feel privileged to have reactivated and been allowed to lead for most of my time here. I hope it continues on. The Philosophy of Science Reading Group and the History of Philosophy Roundtable have also been crucial sources of advice, criticism, and support. Members of both groups have read multiple drafts of different parts of this work and provided enormously helpful comments. The extremely high level of quality of both of these groups ensures that any philosopher of science or historian of philosophy coming out of UCSD can get an especially deep and rigorous education. I have the happy privilege of having been involved to some degree with both.

Amongst my elder peers, Ryan Hickerson was a particularly helpful mentor, and our discussions of Feyerabend and Dewey helped spur much of the work I've done. I owe a similar debt to Carl Sachs, who likewise encouraged my nascent interest in pragmatism and neo-pragmatism. While we never overlapped at UCSD, I was often encouraged by the legacy of P.D. Magnus, and then later by the actual person. Amongst those students who are roughly my contemporaries, Adam Streed and Lyn Headley have taken a special interest in my work on Dewey. Adam has been one of the most sympathetic critics of my work, a lively interlocutor, and a good friend. Lyn Headley from Communication was an unlikely but welcome ally in the study of Dewey. Amongst the most recent group of UCSD graduate students, my thanks to Per Milam and Cole Macke for lively discussions of pragmatism and to Joyce Havstad for reading, commenting on, and discussing parts of my work, and for discussions about various issues in philosophy of science.

Paul Churchland has been the kind shepherd of this dissertation. He has read many of its main sources with me (even Dewey's *Logic!*), and read and commented on multiple drafts of every part of this dissertation, and for his feedback and encouragement I am grateful. Nancy Cartwright has pushed my work towards much greater clarity and concreteness, forcefully but always encouragingly, and has likewise read and commented on multiple drafts of nearly everything in the dissertation. Jerry Doppelt has provided much-needed moral and philosophical support, and has acted as the stalwart defender of the Kuhnian tradition in philosophy of science; the lack of systematic engagement with that tradition here is entirely my own fault. Don Rutherford provided some much-needed recommendations for reorganizing the dissertation that significantly improved it and helped me see better what precisely I wanted to say. Mike Cole and Roddey Reid have both been far more supportive of me and my project than I could have ever expected, and I hope they can see their influence in this work. I especially appreciate the opportunity to present aspects of this work to the Laboratory of Comparative Human Cognition, which Mike directs, even when it has not been entirely apropos to the work done there. Clinton Tolley showed an active interest in my project, and I have had some useful discussions with him about the structure of the dissertation and some of Dewey's key ideas.

My thanks to Professor Larry Hickman, Director of the Center for Dewey Studies, who read the entire draft of this dissertation carefully. He made several insightful and useful comments, and I am grateful for his encouragement. Shane Ralston read and commented extensively on the draft up through Chapter 3, and his comments were an education unto themselves. I fear I have only begun to properly address some of the issues he raised.

I began to think about the issues of evidence I discuss in Chapter 3 during an extremely fruitful visit to the London School of Economics, for which I should thank Nancy Cartwright and the Center for Philosophy of Natural and Social Science. Nancy asked Philipp Dorstewitz and I to do a presentation on pragmatism and

evidence for a joint seminar with Hasok Chang at UCL, and my first reaction was that Dewey didn't have much of anything to say about the subject of evidence; I now believe that, while Dewey didn't much use the term "evidence," his thinking about issues of evidence are some of the most important contributions he has to make to contemporary problems. My great thanks to Nancy, Hasok, Philipp, and all the participants of that seminar for helping me with these ideas, and additional thanks to Philipp for many other discussions about Dewey during my stay. The real spur for the current form of the chapter was an ongoing argument with Jacob Stegenga over issues of induction, robustness, discordance, etc. These issues were his before they were mine, and while I believe he is appalled at my conclusions, our discussions and his extensive feedback on various drafts of that chapter have been immensely helpful. I would also like to thank Eran Tal, who was a third party to many of those discussions who attempted to mediate between the two of us.

Chapter 4 began in part as an attempt to understand the temporal complexity of *judgment* according to Dewey as a response to Wayne Martin's (2006) account; the discussion of Kitcher began as a much briefer attempt to provide an example about how those ideas would impact philosophy of science. I have extremely fond memories of tramping all over Wivenhoe discussing his book and my chapter, among other things. My thanks to him for those discussions and his comments on my draft. I would also like to thank multiple audiences at UCSD, the Philosophy of Science Association, and the faculty of Arts and Humanities at the University of Texas at Dallas for opportunities to present and discuss this material.

I originally began Chapter 5 in response to Paul Hoyningen-Huene's invitation to speak at the Center for Philosophy and Ethics of Science (ZEWW) at Leibniz Universität Hannover. I pounded out most of the first draft at the house in Oxford that Jeremy Farris shared with several other Rhodes Scholars, otherwise known as the Fjord Institute in St. Ebbes-by-the-River. It was a truly wonderful environment to write in, and I had several wonderful discussions with Jeremy (as I always do).

While in Hannover and Berlin, I had several helpful discussions of this and related materials with Paul, Helmut Heit, Katie Plaisance, and Eric Oberheim; my thanks to Eric and Helmut for allowing me to stay at their homes during my trip. Early discussions of Giere with Craig Callendar and Feyerabend with Ryan Hickerson also were very helpful in spurring the ideas of this chapter. A slightly earlier draft of this chapter has been accepted for publication in *Studies in the History of Philosophy of Science*, and I am grateful for Giere's kind and encouraging response which will appear with it, and to Elsevier for permission to include the chapter here.

Chapter 6 was a term paper for Gila Sher's course on Richard Rorty, and I am grateful for her comments and encouragement. Helmut Heit was among those who read and commented on other drafts, for which I am grateful. I would like to dedicate that chapter to the memory of Richard Rorty, for while he comes in for heavy criticism in that chapter, I believe he was also one of the best and perhaps *the* most under-appreciated philosopher of the later half of the twentieth century. He made a real and important contribution to the discipline, and I hope that history will be kind to him.

The epilogue owes much to many people, and I hope it points in a fruitful direction for future research. My thanks to Nancy Nersessian for her encouragement in this project, and to P.D. Magnus for discussions of early versions of his paper on the topic (to which I respond). My thanks to Edwin Hutchins and the Distributed Cognition and Human-Computer Interaction Laboratory for discussions of distributed cognition and the opportunity to present early ideas on this topic, and to the organizers and participants of the conference on "Bringing together Philosophy and Sociology of Science" at the Vrije Universiteit Brussel, especially Benedikt Löwe and Hauke Riesch. Marta Halina has been especially helpful and supportive of this line of my work, and I am grateful for all of our discussions of d-cog and science studies.

On a personal level, it seems like such a long, strange road that's brought me here, with so many people to thank along the way. Let me return to the beginning:

my thanks to my parents, who always supported my academic and creative pursuits, my love of science and reading. My thanks to them also for not being *too* critical when I dropped computers for physics, and then physics for philosophy. My undying gratitude also to Bryan, Mark, and Cindy Kennedy, without whom I would be a much less kind and thoughtful person, and who also encouraged my intellectual pursuits all along the way.

All through grade school I participated in a program called Odyssey of the Mind, a creative problem-solving competition which combined elements of engineering and the arts. I owe a great debt to my many team-members, coaches and all the officials, volunteers, and creators involved in making that experience possible. They nurtured my creativity and my independent spirit, gave me a life-long interest in problems and problem-solving, helped me with a certain amount of fluidity moving between science, technology, arts, and humanities. I was a member of the Parkview High School Philosophy Club, which as I recall only discussed central topics of theoretical philosophy once or twice in four years, but our endless debates about politics, religion, literature, popular science, and various other matters certainly began my interest in the idea of philosophy. My thanks to the Highland brothers, Vivian Smith née McLean, and all the other participants, as well as to Sandy Peterson for sponsoring what must have been a fairly “dangerous” club for a school in the suburban south. My thanks also to Lynnette and the members of *Whispers of the Heart BBS* for providing a nurturing and spirited intellectual community at roughly the same time.

My greatest teacher in high school was Emily Beals, who I had through two and a half years of English. She taught me about Plato’s cave, about Emerson and Thoreau, about reading well, and, most importantly, about how to write well. It is hard to express just how important that training has been for my success of life and my love of what I do.

Such small things make such large changes in our lives. Thus, I owe a signifi-



cant debt to Benji Kmack for recommending *Zen and the Art of Motorcycle Maintenance* to me, a book whose influence on my thinking I can still make out to this day. I read it during my freshman year of college at the Georgia Institute of Technology, and I suppose it was my first exposure to “serious” topics in philosophy. I just happened to be reading it outside of one of the large lecture halls when Jennifer Hou spotted me reading it. I don’t think we really knew each other, but she came up to me to talk about the book, and ended up recommending I take a philosophy class with Professor Johnston, and so I owe her an even more significant debt. I don’t think I even knew there *were* classes in philosophy, and finding out, I was eager to jump right in.

Jon J. Johnston was a giant of a man, the greatest teacher and mentor I have ever known, and a true philosopher in the tradition of Socrates. Jon never published anything after his M.A. thesis and got tenure at a time when that didn’t matter. I became part of a small group that gathered around him to learn how to live the life of the mind. From Jon I learned ancient and modern philosophy, the existentialists (except Heidegger, who was verboten for his Nazism and his painful prose), aesthetics, the Asian philosophical traditions, Feyerabend, Popper, Ernest Gellner, and much more. I had countless lunches in his office, meetings in small groups at coffee shops and ice cream parlors around Atlanta. The intellectual contest between his Plato, Aristotle, Kant, and Russell and my Feyerabend and Rorty were among the most fruitful moments in my intellectual development. Jon helped me apply for special degree requirements in physics that emphasized philosophy and theory. He encouraged me to apply to graduate school. While at UCSD, I had many opportunities to visit him and give guest lectures in his classes, to talk to him about my work, and to convince him that *Art as Experience* is one of the finest works in all philosophy. Who I am and what I do I owe much to his influence. He taught me that philosophy is made of vaulting intellectual ambitions, not specialized, circumscribed intellectual puzzles. He taught me much more than philosophy.

My other teachers at Georgia Tech also had a significant impact on my development. To Bryan Norton, I owe my exposure to pragmatism; to Nancy Nersessian, my exposure to the philosophy of mind and social and distributed theories of cognition; to Andy Ward, my exposure to philosophy of science and the intellectually thrilling independent study that led to my quirky but apparently successful writing sample for graduate school applications. These were all the philosophers I had at my disposal, but they did very well by me. David Finkelstein, a physicist known amongst philosophers for his contributions to “quantum logic,” who is now bravely trying to work out the next paradigm shift in physics, taught me much about how to think about science philosophically and philosophy scientifically. His unusual pragmatic, process-oriented way of thinking about physics continues to stick with me, though my interest in the philosophy of physics has waned. David too had a certain inspirational intellectual ambition.

My fellow-students at Georgia Tech had almost as important an impact as any of my teachers; together with those students who loved philosophy, I formed the Georgia Tech Philosophical Society. Its meetings were some of the most valuable intellectual experiences of my career. Many thanks to Mr. Farris, Ms. Hou, Ms. Habeeb, Mr. Dennard, Mr. Peterson, Mr. Barnett, Ms. Beck, and all the others. I also enjoyed many discussions with my fellows in the GT Society for Physics Students. My gratitude to Bryan Kennedy and the Computer Science department for giving me the unbelievable opportunity to be a teaching assistant for two and a half years as an undergraduate. I am a much better teacher as a result, and I learned a lot from my fellow teaching assistants. My ironic appreciation to the systematic awfulness of much of the rest of that Institute, which convinced me that a career in the computer industry or in science itself would be a barren and soulless enterprise, even if it isn’t true.

Andy Ward suggested that I apply to UCSD; I will forever be thankful for the suggestion, and to the committee here that saw fit for whatever reason to admit

me. I am thankful for the almost complete dearth of interest in and support for pragmatism at UCSD; this shocking difference from my undergraduate experience led me to re-start the then defunct Pragmatism Reading Group which eventually led me to my passion for Dewey. I appreciate Craig Callender talking me out of doing philosophy of physics, though he may not know that he'd done so. Those faculty who criticized me for "heterodoxy for heterodoxy's sake" managed a nice bit of reverse-psychology, helping me see the poverty in much of contemporary philosophy, the need for reconstruction, and the virtues of my own views—as well as the need for a careful and thorough defense of them. I very much appreciate the encouragement of Paul Churchland and Nancy Cartwright, who saw creativity and daring where others saw a penchant for strange or discredited ideas. Paul encouraged all of my quirky ideas and unusual interests from the very beginning. I hope he has forgotten the topic of my very first term paper for him. While I have been at UCSD, Jenn Hou and Jason Loy helped me realize that I was so committed to philosophy that I wouldn't give it up for a big paycheck in an wonderful city. Megan McHugh helped me realize just how much I was willing to give up to keep doing philosophy.

I would be remiss if I didn't name those whose help along the way was less specific but still important: Anna Alexandrova, André Barbosa, Pedro Barbosa, Jeff Barrett, Bill Bechtel, Andrew Beck, Michael Bernstein, Marisa Brandt, Amanda Brovold, Patricia Churchland, Megan Clancy, Pete Coogan, Blythe Corgiat, Shannon DeGroff, Dale Dorsey, Yrjö Engeström, Brynn Evans, Beth Ferholt, Devin Flaherty, Erin Frykholm, Maria Hague, Mitch Herschbach, Monica Hoffman, Kristin Irwin, Charlie Kurth, Robert Lecusay, David Leitch, Eric Martin, Emily Matthews, James Messina, Evan Moreno-Davis, Ioan Muntean, Etienne Pelaprat, Catarina Pestana, Madeleine Picciotto, Alexis Rochlin, Mrs. Rochlin, Ivan Rosero, Kory Schaff, Aaron Schiller, Sharon Skare, Becky Stark, Craig Starnaman, Sandra Starnaman, Michael Tiboris, Stephen Tyndall, Eric Watkins, Nellie Weiland, James Wicks, Holly Wicks, and Jeff Yoshimi. Thanks to you all and to all of those I have forgotten to name.

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## VITA

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ABSTRACT OF THE DISSERTATION

**Science and Experience**  
**A Deweyan Pragmatist Philosophy of Science**

by

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Doctor of Philosophy in Philosophy

University of California, San Diego, 2009

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I resolve several pressing and recalcitrant problems in contemporary philosophy of science using resources from John Dewey's philosophy of science. I begin by looking at Dewey's epistemological and logical writings in their historical context, in order to understand better how Dewey's philosophy disappeared from the limelight, and I provide a reconstruction of his views. Then, I use that reconstruction to address problems of evidence, the social dimensions of science, and pluralism. Generally, mainstream philosophers of science with an interest in Dewey pay little attention to the body of scholarship on Dewey and tend to misinterpret or miss important features of his work, while Dewey scholars generally do not connect his work to the nuanced problems of the contemporary scene (with some notable exceptions). My dissertation helps to fill this important gap and correct common interpretive

mistakes by reconstructing and clarifying Dewey's philosophy of science and using it to resolve several contemporary problems.

Though his is the road less traveled, Dewey's views provide a good starting place for addressing current concerns. He worked towards a model of science that is both fully naturalistic and fundamentally oriented towards human practice, demands that have been strongly argued for but poorly assimilated by most mainstream philosophers of science. He treats scientific practice, and human thinking generally, as not only embodied but also socially and technologically embedded, and thus can be used to open up a dialogue with much of the social studies of science. He has an anti-foundationalist but structured epistemology, and he offers a way to navigate the narrow paths between an immodest and simplistic realism and the pessimistic extremes of anti-realism and social constructivism, a pursuit of interest to many major philosophers of science at present. Philosophy of science took a different path in the twentieth century, beginning with the "received view" of logical positivism that left many of the nuances of the original movement by the wayside. No aspect of that starting point has avoided disrepute in recent decades. I show that Dewey avoided the wrong turns of mid-century philosophy of science which are now blocking the way forward.

# Chapter 1

## Introduction

### 1.1 Whatever happened to American Pragmatism?

In “From *Wissenschaftliche Philosophie* to Philosophy of Science” (Giere, 1999, pp. 217–236), Ronald Giere poses a number of important and interesting historical and counterfactual questions. It is a historical platitude that Logical Empiricism, a movement that began in the early twentieth century intellectual heyday of Vienna with Moritz Schlick’s Vienna Circle, in the 1930’s emigrated west, mostly to America, where it was received by a friendly and sympathetic philosophical community. By 1960, Logical Empiricism had eclipsed American Pragmatism and become the dominant tradition in philosophy of science, and perhaps in philosophy generally. At the same time, it is part of the philosophical lore that American Pragmatism, the dominant philosophical tradition in North America in the 1930’s, is very much opposed to the core tenets of Logical Empiricism; furthermore, it is well known that the subsequent decline of Logical Empiricism—a long, slow process<sup>1</sup> announced already by Quine in 1951 and by Kuhn and Feyerabend in the 1960s—has been in part a move back towards Pragmatism.

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<sup>1</sup>The process is not altogether concluded, in fact (See Giere (1999, p. 235).



Against this background, Giere poses a pair of questions:

1. “How, between 1930 and 1960, did a dissident European movement advocating the replacement of much established German philosophy by *Wissenschaftliche Philosophie* transform itself into the dominant tradition for philosophy of science in North America?” (Giere, 1999, 219)
2. “How did a naturalistic pragmatism incorporating an empirical theory of inquiry get replaced by a philosophy that regarded induction as a formal relationship between evidence and hypothesis?” (Giere, 1999, 231)

It is also tempting here to pose a related causal question: “[H]ow much did the success of Logical Empiricism contribute to the decline of Pragmatism?” (Giere, 1999, 230). Though, as we shall see, Giere’s questions in some way belie the complexity of the historical issues here, nonetheless he poses a significant problem for those of us interested in the history of philosophy of science. Another key part of the story that will play an important role here is that Logical Empiricism did not simply remain constant through its change of geographic and social context; the development of philosophy from 1930 to 1960 includes a significant *transformation* of Logical Empiricism.

Ultimately, however, I am not so much interested in the purely historical issues. I regard the development of philosophy of science from 1930 to 1960, insofar as it really involves the decline of Pragmatism and the rise of Logical Empiricism to clear dominance, as well as the transformation of the philosophy of the Vienna Circle into the “received view” version of Logical Positivism circa 1960, as one of the greatest philosophical foibles of the twentieth century. I see the continuing influence of Logical Positivism in the agenda and method of philosophy of science as the main reason for the current impasse over issues of pressing concern, over the social dimensions and responsibilities of science, the role of values, and the merits of unity and pluralism, as well as the current denigration of the concept of scientific method. I’m interested

instead in, as Giere says, “establish[ing] connections with earlier traditions containing forgotten resources useful to the contemporary enterprise”(219). And I am thus somewhat more interested in a *counterfactual* question that Giere poses:

Imagine that the Social Democrats rather than the National Socialists had come to power in Germany in 1933 (and thus that World War II never happened). What would have been the fate of *Wissenschaftliche Philosophie* in Germany, Austria, and throughout the world? What would have been the fate of American Pragmatism? And what would now be the complexion of the philosophy of science in North America?(235–6)

One possibility is that the variety of philosophical positions available in 1930, and thus the flexibility of the discipline as a whole, might have been retained, and in particular, that Pragmatism might not have fallen into decline and disinterest. Even if Deweyan Pragmatism were not a live option in this alternative version of twenty-first century philosophy of science, it seems possible that it might well have had sufficient influence on the aims, agenda, and methods of philosophy of science that we would not be struggling so ineptly with issues of core importance to Dewey and his followers.

In this chapter, I aim to give historical and philosophical plausibility to this sort of counterfactual speculation. I will begin with a brief historical story about how Logical Empiricism came to America, how it was received by American philosophers, especially Pragmatists, and how it might have come to dominate and replace Pragmatism. So-called “external” factors loom large in this story, providing room for the suggestion that the historical development was a philosophical foible. I will attempt to briefly indicate the ways in which, from a contemporary perspective, Dewey and his allies were far more sophisticated than the Logical Empiricists who came to replace them. I will then show the ways in which Logical Empiricism, as it developed into the “received view” circa 1960, is responsible for the current impasse, and I will suggest the counterfactual history that we might imagine in order to get beyond that impasse.

## 1.2 The Transformation of Philosophy of Science, 1930–1960

“It is a matter of historical record,” Giere says, that both the Logical Empiricists and the American Pragmatists “viewed each other as philosophical allies” (230). How could this be so, given that the two groups seem to have such different philosophical programs? I will follow two distinct, though perhaps compatible explanations. First, there is a way in which the major differences we today perceive between Pragmatism and Logical Empiricism belie significant agreement and similarity of projects at a general level. According to Alan Richardson, both programs were part of a broader philosophical movement that we might call “scientific philosophy,” and even within this diverse program, these two groups shared significant aspirations for philosophy and an associated rhetoric of scientific philosophy as revolutionary, social engineering pursuit (Richardson, 2003). According to George Reisch 2005, Pragmatism and Logical Empiricism shared significant enemies in the neo-Thomists and other “enemies of science” (and scientific philosophy?), and thus, despite significant misgivings about the views of the Logical Empiricists on the part of Dewey in particular, the two groups joined forces in the “Unity of Science” movement for a variety of tactical as well as philosophical reasons.

In telling this story, it is tempting to revert to some features of a standard but problematic movement. It is tempting to talk as if Pragmatism were a unified movement, dominant circa 1930, and Logical Empiricism was a movement that migrated from Europe to America and quickly eclipsed Pragmatism. But as Richardson points out (2003, 4–5), it is not at all clear that Pragmatism was a *dominant* position in philosophy in 1930–1940; it is perhaps better to say that there were no dominant projects or programs, and if anything, the major division was between pro-science and anti-science philosophers of otherwise diverse views. Likewise, depending on what one means by “Logical Empiricism,” it isn’t clear that Logical Empiricism was

ever dominant, and if anything, it was not until the 1960s, a decade after Quine announced his “refutation” of the program, and when distancing oneself from “the received view in philosophy of science” became one of the main rhetorical strategies of philosophy of science. Nevertheless, since it is primarily the legacy of *Dewey’s* philosophy of science that we are concerned with here, and since he was always at odds to some degree with the various Logical Empiricists while nonetheless choosing to work together with them, the puzzle remains crisp.

According to Alan Richardson, “scientific philosophy” was a movement in the late nineteenth- and early twentieth-centuries that included such diverse philosophers as Helmholtz, Avenarius, Husserl, Russell, Carnap, Neurath, Dewey, and Heidegger (Richardson, 1997, 2002). Despite major disagreements and battles within the lines that Richardson would like to draw (e.g., the “somewhat shrill” exchanges between Dewey and Russell (Richardson, 2002, S43–44), it is clear that “scientific philosophy” represents a group that would have been thrown together in various battles against systematic metaphysicians, American and British Idealists, neo-Thomists, and others. While it doesn’t suit my purposes to discuss at length the common thread of scientific philosophy, especially since it is largely defined negatively, by what it criticizes (Richardson, 1997, pp. 418, 430), it is helpful to consider several points of general agreement that Richardson picks out:

1. “Philosophy, like science, had the aim of securing objective truth.”
2. “Philosophy, unlike the special scientific disciplines, had not been successful in achieving consensus on any of its issues and, thus, was doing badly given its aims.”
3. “Philosophy, therefore, had to learn from science regarding the means for achieving its aims.”
4. “Philosophy had to achieve the sort of community and habit of mind exhibited

by scientists in other disciplines; scientific philosophers required consensus and collaborative and piece-meal progress toward truth” (S40).

I have some misgivings about (1) as an accurate representation of Dewey’s views, but it is clear enough that he holds to some form of the other three doctrines, and perhaps some weaker version of (1) would also suffice. Agreement on these points left significant room for debate about the methods of science, how to bring scientific methods to bear on philosophy, the subject matter of philosophy, etc.

If shared allegiance to these basic principles were all that united Logical Empiricism and Pragmatism, it would seem weak ground on which to base an alliance. But on two major debates taking place within scientific philosophy in America up to 1930, the Pragmatists would find that the Logical Empiricists were on their side. In particular, to the question of “whether scientific philosophy was a revolutionary break from previous philosophy” both the Logical Empiricists and Dewey argued that it was, while other scientific philosophers like A.O. Lovejoy and Moris R. Cohen denied it. Richardson quotes from Dewey’s *Reconstruction in Philosophy* as evidence of the revolutionary nature of Dewey’s approach:<sup>2</sup>

The causes remain which brought philosophy into existence as an attempt to find an intelligent substitute for blind custom and blind impulse as guides to life and conduct. The task has not been successfully accomplished. Is there not reason for believing that the release of philosophy from its burden of sterile metaphysics and sterile epistemology instead of depriving philosophy of problems and subject-matter would open a way to questions of the most perplexing and the most significant sort? (MW 12:152)<sup>3</sup>

While Dewey doesn’t deny that philosophy has a characteristic set of problems (or problematic situations) that it responds to (the causes that bring about philosophy),

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<sup>2</sup>These claims about the revolutionary nature of Dewey’s philosophy ought to be somewhat mitigated by the fact that Dewey continued to engage with mainstream philosophy throughout his life. See Morgenbesser’s introduction to and Randall and Hook’s first contributions in (Morgenbesser, 1977)

<sup>3</sup> Quoted in Richardson 2002, p. S40

he argues that philosophy needs to take up new tasks quite different from the tasks of traditional philosophy, though some of the traditional philosophical skills are particularly well-matched to the task. Richardson shows that even relatively conservative Logical Empiricists like Moritz Schlick used revolutionary rhetoric to describe their aims:

I am convinced that we now find ourselves at an altogether decisive turning point in philosophy, and that we are objectively justified in considering that an end has come to the fruitless conflict of systems. We are already at the present time, in my opinion, in possession of methods which make every such conflict in principle unnecessary. What is now required is their resolute application. (Schlick “The Turning Point in Philosophy” [1930/1931] 1959, 54)<sup>4</sup>

On the second question, of the “social importance of scientific philosophy,” both the early Logical Empiricists and Dewey rejected the view of scientific philosopher as “pure philosophical theorist” using scientific methods in favor of the image of the “philosophical engineer” (S40). Dewey took his forward-looking and socially-minded stance towards philosophy from “the technological triumphs of science” (S42), and in doing so, he recommends an explicit social-engineering role for philosophy:

The experimental logic when carried into morals makes every quality that is judged to be good according as it contributes to the amelioration of existing ills. And in so doing, it enforces the moral meaning of natural science. . . . Natural science loses its divorce from humanity; it becomes humanistic in quality. It is something to be pursued not in a technical and specialized way for what is called the truth for its own sake, but with the sense of its social bearing, its intellectual indispensableness. It is technical only in the sense that it provides the technique of social and moral engineering. (MW 12:178–179)<sup>5</sup>

In part, the difference between Dewey and philosophers like Lovejoy and Cohen had to do with their different views of what is wrong with “unscientific philosophy.”

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<sup>4</sup> Quoted by Richardson 2002, p. S44

<sup>5</sup> Quoted by Richardson 2002, p. S42

Lovejoy and Cohen saw scientific philosophy as methodologically superior, and its opposite as weak, undisciplined, and contemptible. On the other hand, “For Dewey, the unscientific philosopher *has* power and plays a role in propping up an unjust social order... it is a barrier to the progress of humanity” (Richardson 2002, S43, my emphasis). This is particularly clear in the debates between Dewey and Russell. According to Richardson,

Russell considered that Dewey's pragmatism might be American commercialism in philosophical clothing, while Dewey remarked on his own restraint in not making the counter-suggestion that Russell's dry, technical philosophy might be the expression of a decadent English aristocratic sensibility. (S44)

And further, Dewey indicted Russell for espousing “notions such as the pure intellectual joy of disinterested pursuit of truth while doing nothing to make this joy available to more than a relatively few human beings” (Richardson 2002, S44).<sup>6</sup>

Richardson shows a close connection here too, between Dewey and the Logical Empiricists:

Otto Neurath, Philipp Frank, Rudolf Carnap, and others believed that traditional projects in metaphysics were not simply nonsense, but nonsense with a political agenda: talk of transcendent values served to confuse people, propping up illegitimate structures of political authority with stories that no one could understand. (S45)

Neurath even spoke about the superiority of “proletarian” science to “bourgeoisie” science when discussing the social and scientific value of overcoming metaphysics (e.g., in “Personal Life and Class Struggle,” quoted in Richardson 2002, S45). Logical Empiricism had a clear social agenda that included a revolutionary take on philosophy. Carnap, too, aimed at bringing scientific philosophy to the aid of social struggles; part of overcoming traditional metaphysics and epistemology was, for

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<sup>6</sup>See also Burke (1994).

Carnap, as for Dewey, a precondition for making philosophy that discipline which brought scientific tools to the aid of human problems and purposes (Richardson, 2003, 16–18).<sup>7</sup> In these these ways, “the logical empiricists were close kin to Dewey and his acolytes” (S44) in their agenda in the 1930’s.

There are more specific reasons, too, for the alliance between Dewey and the Logical Empiricists. For Reisch, the war of American scientific philosophers with the neo-Thomists looms large in the explanation of the cooperation between Dewey and the Logical Empiricists. Dewey believed that the Unity of Science movement “had to see itself as a response to science’s enemies” (Reisch, 86) rather than address only theoretical questions about relationships among the sciences. The neo-Thomists, led by Mortimer Adler and University of Chicago president Robert Maynard Hutchins, promoted a view of science as value-free and thus unfit for guiding culture.<sup>8</sup> Instead of scientific philosophy, the neo-Thomists called a return to the philosophy of St. Thomas as the guiding light for culture, education, and intellectual life (Reisch, 73–74).<sup>9</sup> All this came to a head in Adler’s “God and the Professors,” and a response in *Partisan Review* organized by Dewey’s student Sidney Hook, entitled “The New Failure of Nerve” (Reisch 76–78).

Effectively responding to these enemies, not only the neo-Thomists but “anti-scientific fascists in Europe” as well, was the goal of the Unity of Science in Dewey’s mind. In other words, not theoretical or epistemic but *socio-political* unity was the point, and hence the title of Dewey’s contribution to the *Encyclopedia*, “Unity of

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<sup>7</sup>See also Friedman (1996).

<sup>8</sup> This would be the source of Dewey’s view that the Logical Empiricist position on values was a dangerous tactical error.

<sup>9</sup> Reisch quotes a recollection from Carnap’s autobiography from a department seminar at the University of Chicago, where Adler “declared that he could demonstrate on the basis of purely metaphysical principles the impossibility of man’s descent from”brute,” i.e. subhuman forms of animals. I had of course no objection to someone’s challenging a widely accepted scientific theory. What I found startling was rather the kind of arguments used” (Carnap 1963a, 42; as quoted in Reisch, 74).



Science as a Social Problem” (Dewey 1938, LW 13:271–280)<sup>10</sup>, where he addresses significant concern about

active opposition to the scientific attitude on the part of those influenced by prejudice, dogma, class interest, external authority, nationalistic and racial sentiment, and similar powerful agencies. Viewed in this light, the problem of the unity of science constitutes a fundamentally important social problem. (LW 13:274)<sup>11</sup>

Dewey emphasized the need for the movement to be “flexible, open, and democratic” (Reisch, 2005, 86) rather than setting out “in advance a platform to be accepted” (Dewey 1938, LW 13:275; Reisch *ibid*). Neurath was in agreement, “Always eager to fend off traditional philosophy and its pretensions to be queen of the sciences” (Reisch *ibid*).

Besides the specific disputes between scientific philosophers and neo-Thomists, there was a great degree of camaraderie between American philosophers and the representatives of Logical Empiricism. Neurath in particular was well-received by many of the intellectuals in New York City (Reisch, 65–67), both for his congenial socialist political views and his scientific-minded but open and pluralistic philosophical views and attitudes. The subtleties of Neurath’s views have been brought to light in recent years, and they paint a picture of someone much more at home in the 1930’s American philosophical scene than in the dry philosophical views portrayed by Ayer or captured in the “received-view” version of Logical Positivism (Uebel, 1991; Cartwright et al., 1996). Neurath was practically worshiped by Ernest Nagel, he won over Dewey to the *Encyclopedia* project, and he made close and enduring friendships with Sidney Hook and Horace Kallen (Reisch, *ibid*.) The Unity of Science movement found a natural home in 1930s North America.

The main tension between Dewey and the Logical Empiricists, one that Richardson may downplay a bit more than he should, is “the proper philosophi-

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<sup>10</sup> See discussion in Reisch, pp. 85f.

<sup>11</sup> Quoted in Reisch (2005, p. 86)

cal account of values”(S45). It is here where logical empiricism and pragmatism clearly come apart. Logical empiricism draws a clear distinction between “descriptive” and “normative” theories, and regards the latter as metaphysically confused, to be better accounted for in non-theoretical, non-cognitive terms. Richardson takes this as evidence of logical empiricism’s “demarcationist” rhetoric. Dewey’s account of the normative, on the other hand, displays “imperialist” rhetoric, attempting “to bring scientific rigor into all areas of philosophical concern”(S46) including moral theory and axiology.

This last point is particularly damaging to Giere’s claim that Quine represents a move back in Dewey’s direction. Quine certainly belongs to the very broad category of “scientific philosophy” which subsumes logical empiricism and pragmatism. While he also shares some theoretical commitments to pragmatism (e.g., a rejection of the internal/external and analytic/synthetic distinctions, perhaps, and a more thorough anti-foundationalism), “With Quine, a theoretical commitment to pragmatism lost both its practical dimension and its social consequence”(S47), and thus it seems that Quine is no closer to Dewey than to logical empiricism. Further, “pragmatism” for Quine is a particular claim about the nature of confirmation and belief (viz., that the “rules of verification do not wholly determine the choice of what to believe”(Richardson 2003, 17)), whereas for Dewey pragmatism has much more to do with *doing* than belief or knowledge.<sup>12</sup> Dewey’s pragmatism amounts in part to an “insistence that an adequate philosophy both understand and provide means for human agency”(Richardson 2003, 18).

Another point of tension was Dewey’s rejection of reductionism. In Dewey’s first contribution to the *Encyclopedia*, he did not only advocate flexibility and democracy in the Unity of Science movement; he also advocated anti-reductionism. When Dewey wrote,

But the needed work of co-ordination [of the sciences] cannot be done

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<sup>12</sup>See also Shook (2002).

mechanically or from without. It, too, can only be the fruit of cooperation among those animated by the scientific spirit. Convergence to a common center will be effected most readily and most vitally through the reciprocal exchange which attends genuine co-operative effort. The attempt to secure unity by defining the terms of all the sciences in terms of some one science is doomed in advance to defeat (Dewey 1938, 34; Reisch 87)

This trampled on Carnap's work on the theoretical unity of science via definability of terms (e.g., see Carnap's "Testability and Meaning").

But Dewey had an unshakeable sense "that the categories of [e.g.] sociology and biology cannot be 'reduced in the sense in which the English reader naturally understands the word to physical categories (i.e. categories of physical science)'" (Dewey to Carnap, 30 Dec 1937; Reisch 88). He didn't see the use of a "thing language" or physicalist "slang" in science, "because science crucially involves 'operations' and behaviors" (Reisch 89). Further, these both "crucially involve valuations and value propositions" (ibid), which the thing language does not permit. If anything, "a behavior or operational language" had a much better shot (Dewey to Neurath, 17 August 1938; Reisch 89).

Especially in the case of social science, the attempt to speak the language of natural science was a mistake, "doomed in advance to defeat" in part because of the necessary role played by values and value terms.

None of these disagreements was purely a matter of technical philosophical issues, of the first-order philosophical theses themselves. "In Dewey's eyes, both of these run-ins with his logical empiricist editors involved his concerns about science's enemies and how to keep them at bay" (94). They were tactical concerns. Only embracing values "as a core component of unified science" could allow the movement to diffuse the neo-Thomist critique and allow scientific philosophy "a credible, influential voice in intellectual and popular debate about the course of contemporary culture" (Reisch 95).

In Dewey's mind, Reisch claims,

If Adler and Hutchins successfully fooled the world into believing that science was technical and value-free, that is, then they could more easily persuade the world that Thomism (or some other nonscientific, rationalistic system) had to be embraced as a source of values and guidance for contemporary life. And in that case, both the New York philosophers and the logical empiricists would be on the losing side in the war over science. (Reisch, 2005, 95)

And so it was tactically crucial to reject the emotivist and non-cognitivist tendencies in the Logical Empiricist treatments of values, and the associated tendency towards physicalist reductionism.

After Dewey's death, his ideas about science were fairly quickly forgotten. The tale is recounted in detail by George Reisch, who argues that the socially-engaged philosophies of science like that of Dewey and of the left-wing logical empiricists like Neurath and Frank was eclipsed by a purely formal, logicist, disinterested version of logical empiricism of the sort expressed in the late work of Hans Reichenbach. Reisch places the blame on the Cold War—the new “analytic” mode of philosophy was safer to practice in McCarthy's America than the frankly socialist philosophy of Dewey and Neurath. Giere also points to the experiences of World War II and their impact of Reichenbach—in the context of justification, there was no “Jewish science” or “German science,” only theories and evidence. We might also look to Bertrand Russell's (probably unintentional) campaign of misinformation about Dewey that culminates in the penultimate chapter of his *History of Western Philosophy* devoted entirely to refuting bad misreadings of Dewey, or one might look at the massive influence of W.V.O. Quine, who was politically quite conservative, over the whole discipline, starting in the mid-fifties. Whatever the contributing factors, very few philosophers by 1960 were defending anything like Deweyan ideas.<sup>13</sup>

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<sup>13</sup>As Reisch recounts, somehow Sidney Hook came to the conclusion that the best way to put Dewey's philosophy to work in the '50's was to become a McCarthyite red-baiter for other intellec-

### 1.3 The Current Impasse as Legacy of Late Logical Empiricism

The tradition we inherit in philosophy of science today has little to do with the dynamic ideas of Neurath or Dewey, and is greatly influenced by the set of ideas we have come to call “the received view.” Whatever the status of this tradition in the minds of contemporary philosophers, it continues to have a basic hold on our thinking. Education in general philosophy of science largely proceeds in terms of setting out the received view (perhaps by reading E. Nagel, Feigl, or Reichenbach) and rehearsing the main objections to it (from Hanson, Kuhn, Feyerabend, etc.), and then figuring out what features we can save (i.e., how to recover good sense from these “radical” detractors). Insofar as philosophers of science are no longer satisfied with the logical analysis of scientific language, or with the rational reconstruction of scientific episodes in terms of formal confirmation theory, the received view offers little interest; however, the major critics fail to provide a comprehensive alternative. Philosophers of science now struggle with a variety of issues that go beyond the analysis of science as such to looking at the context in which science operates and the other areas of human life to which it relates. The place and status of social and political values and critique in science, evidence for use and application, and the relation of science to social policy are examples of the sorts of problems that many philosophers of science are wrestling with today. But as they do so, various presuppositions inherited from late logical empiricism creep in to frustrate the analysis.

From this point of view, the most pernicious and problematic assumptions that we’ve inherited from the tradition are:

1. That science is *or should be* essentially value-free, because values conflict with

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tuals. Philosophers of science like Richard Rudner and C. West Churchman defended something like Deweyan ideas about the scientific status of values and the role of values in science, as discussed in Douglas (2009).

or corrupt the ends and means of science.

2. An expressivist or irrationalist theory of value.
3. An empiricist theory of evidence that takes evidence as the relatively-fixed justifier of ideas.
4. The dualism between the processes of discovery and justification.
5. An undue obsession with formalisms, especially the formalism of mathematical logic.
6. A lack of attention to the character of science as a *practice*.
7. An assumed discontinuity between the practice of science and the other areas of human life.

While many or perhaps all of these assumptions would be rejected outright by many philosophers of science today, they nonetheless function as a kind of “default” model of science. Thus, for example, while many philosophers of science discussing how theories are tested will explicitly deny any straightforward empiricism, those working in other areas might implicitly assume some basic form of empiricism. In several of the arguments I will discuss throughout this book, I will uncover such assumptions and show how they vitiate attempts to make progress.

Even the more radical critical reactions to this problematic model of science are of little more use to contemporary philosophers of science than the model itself. Some critics, especially sociologists of science, reject the value-free image of science, but they largely retain the second clause of the claim: values still have a corrupting potential, but the potential is everywhere actualized. Science is thoroughly value-laden, and thus thoroughly corrupt. Even the ideal that science *should* be value-free is hopeless naiveté, which could at best be a cover-up for the real power and ideology underlying science. The failure of simple epistemologies of science has led not only to

skepticism and relativism about scientific knowledge, but also skepticism about the very topic of scientific method.<sup>14</sup> When philosophers of science move their attention to practice and the context of discovery, they also mostly eschew general projects, and engage in micro-studies of particular cases. The new philosophy of science tends towards overspecialization and lowered ambition, which is fine so far as it goes, but is useless for those hoping to solve the sorts of problems I’ve been discussing.

Take Kuhn as an example, one of the most important and revolutionary philosophers of science in the latter half of the twentieth century. He rejected the value-free ideal, empiricism, the discovery/justification dualism, and formal logical analyses. He championed a return to looking at science as a practice. But what do his positive views amount to? Even on the most charitable interpretations, Kuhn is an ontological (Hoyningen-Huene, 1993) and epistemological (Doppelt, 1978) relativist. Scientists practice in different worlds from each other and the rest of us, and they work largely on puzzles internal to the values and concerns of their own paradigms. While Kuhn promises a return to practice, it seems like he provides only a historicized version of Carnapian neo-Kantianism. For all the power of his critique, Kuhn spent much of his subsequent career working out the least helpful and interesting aspects of his view, going down the rabbit-hole of semantics, defending incommensurability against philosophers of language. He talks about “values,” but his epistemological values are just the old epistemological rules, which are no longer taken to work algorithmically, but in a more fuzzy fashion.<sup>15</sup>

In contemporary discussions of evidence in philosophy of science (chapter 3), the problematic nature of the received tradition is often quite clear. Empiricism and logicism (including the new probability-based formulation of logicism known as

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<sup>14</sup>On the latter, see Feyerabend (1994).

<sup>15</sup>“It is, after all, no accident that my list of the values guiding scientific choice is, as nearly as makes any difference, identical with the tradition’s list of rules dictating choice... however, each must first flesh out the rules, and each will do so in a somewhat different way even though the decision dictated by the variously completed rules may prove unanimous” (Kuhn, 1977).

Bayesianism) reassert themselves at the worse times. While lip-service is paid to the fact that science and the use of evidence within are *practices*, no philosophical underpinning is given, and so no interesting conclusions can be drawn. Where the value-free ideal holds sway, further problems develop, especially when one tries to apply our knowledge about evidence to problems of evidence for application and evidence for policy.<sup>16</sup> Again, when one turns to the role of *social* values in science, and the social dimensions of and restrictions on science, our old model of science only causes frustration (chapter 4).<sup>17</sup> Even philosophers of science who directly insist on the importance of paying attention to practice and purposes seem unable to offer much in the way of a framework for thinking about these things (chapter 5).

## 1.4 Back to Dewey: A Counterfactual History

Imagine that we could turn back time and begin again in the early days of philosophy of science, and this time, logical positivism would not ascend to such a powerful status, and that Dewey's pragmatism had remained throughout the course of the twentieth century an important part of the background and tradition of philosophy of science.<sup>18</sup> Perhaps the National Socialists never came to power, and so the Vienna Circle never emigrated. Perhaps Dewey's *Logic* had been better received, or had more able and more persuasive interpreters and defenders, and thus philosophers had been better inoculated against an over-enthusiasm for the use of formal-

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<sup>16</sup>The problems this ideal causes in the realm of environmental policy are explored in depth in various works by Bryan Norton 1991; 2005.

<sup>17</sup>Douglas (2009) also returns to the history of philosophy of science to help address these problems.

<sup>18</sup>My aim as laid out in this section was conceived independently of, but shares much with, the aim of Steven Shaviro's forthcoming book on Whitehead, *Without Criteria*: "My aim in *Without Criteria* has been a limited and specific one. I began this book counterfactually, with the "philosophical fantasy" of a situation in which Whitehead, rather than Heidegger, "had set the agenda for postmodern thought." I have therefore focused upon those aspects of Whiteheads metaphysics that might especially make a difference in how we understand the world today" (Shaviro, 2009).



mathematical logic in philosophy. Perhaps Quine had been captured by Whitehead's later rather than his earlier work, or Russell had been less influential. How exactly we set up this *What If?* story is somewhat immaterial. The question I'm interested in is, what would philosophy of science in this possible world (Call it Earth-2) look like?

Many philosophers of science will get itchy at this sort of talk about possible worlds. Let me assure you that the conceit I'm suggesting owes more to speculative fiction than to Lewisian metaphysics. Imagine I am an ambassador from an alternate reality, an *Elseworld* that branched from your own in 1938, just after the publication of Dewey's *Logic*. I am here to show you how current issues in the field of philosophy of science would be treated by certain philosophers in my own world, how major texts would be received. Our problems are not so different; we, too, face a social and political milieu in which understanding the role of science in a democracy, the scientific constraints of politics and the political constraints on science are of major importance. We have resources that you have forgotten, however, and an ongoing tradition that is used to treating these sorts of questions. Rather than talk about the history of philosophy in my own world post-1938,<sup>19</sup> I will refer only to texts that are available to you, and so I will have to spend a little more time on basic Dewey exegesis than would be usual in a major work in the field where I'm from. (Though such work is common enough for any graduate student in philosophy back home!) Nevertheless, I hope to show you that the way we've proceeded on Earth-2 offers a very useful alternative to your own tradition.

Let's not belabor the fantasy any further. While Dewey's analysis of modality has not received much attention,<sup>20</sup> I share Dewey's view that talk about *possibility* is

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<sup>19</sup>For some reason it turns out to be quite difficult to transport non-living artifacts, especially books and computers, between these worlds. It is a pity not to be able to show you the Carnap<sub>E2</sub>-Heidegger<sub>E2</sub> correspondence, Quine<sub>E2</sub>'s work on the logic of concrescent processes versus the logic of eternal objects, or the Churchlands<sub>E2</sub> work on the consequences of distributed-cognition theory for philosophy of mind, epistemology, and ethics.

<sup>20</sup>Sleeper (1986) is a particularly helpful exception to the rule.

not just a bit of philosophical machinery, but is an important imaginative tool aimed at understanding *possible* changes to *our* world. I engage in this fantasy as one way of showing the value of the kind of philosophy-cum-history project I am engaged in, here. I want to treat Dewey, not as a philosopher with strange views worthy of mere antiquarian interest, but as a live contributor to the tradition of philosophy of science, with important views that could form the historical and conceptual basis for a mature philosophy of science today. Since Dewey has been very nearly forgotten by the tradition, and since he has come to be associated with some figures who are problematic at best, I cannot tell a straightforward tale of historical influence. Thus, I tell a sci-fi tale of possible historical influences, not for the fun of it, but in order to spur a certain reorganizing and reconstituting of our discipline.

The rest of this project will be free from this conceit, though the reader is encouraged to think of it if it helps, and to dismiss it fully if it confuses or frightens. In the following chapter, I will lay out an interpretation of Dewey's philosophy of science, which I hope will simultaneously constitute an original and significant contribution to Dewey scholarship, as well as the theoretical core to be used in subsequent chapters. The next three chapters cover substantive issues and texts in contemporary philosophy of science, arguing that a Deweyan approach avoids various wrong turns and provides a compelling alternative. As already mentioned, chapter 3 covers debates about evidence, taking us from the older problem of the experimenter's regress to the hot topic of evidence-based policy; chapter 4 comes at the science-policy interface from the other direction, addressing the political constraints on science laid out by Kitcher (2001). Chapter 5 responds to the perspectivist views of Giere (2006), bringing the latter-day Paul Feyerabend in as a supporting player. The final substantive chapter responds to a powerful objection from within the pragmatist tradition (in the actual world)—Richard Rorty's arguments against a pragmatist approach to epistemology, logic, and scientific method in favor of a "hermeneutic" approach. The epilogue presents an important prolegomena to future work in science studies, in a

way that Giere argues for and Dewey would have found congenial, though no explicit mention of Dewey occurs there.

## Chapter 2

# John Dewey's Philosophy of Science

### 2.1 Introduction

The time is ripe for Dewey's philosophy of science. Foundationalism is untenable in anything but the most attenuated and empty forms, while the various coherentist alternatives are equally unpalatable. It is hardly deniable that in some sense data or observations are theory-laden, but then we are left with the puzzle about how such things could serve as evidence. It is widely believed that philosophy of science must understand the social dimensions of science and the role of values in scientific inquiry. The problems with giving either a purely descriptively adequate account of what scientists *actually do* or with giving an a priori, normative account of what they *ought to do* are well known, and on top of all that, some philosophers of science are calling for a further requirement to be *socially relevant* as well as getting the descriptive and normative features right.

Not only did Dewey address all of these concerns in a way that is novel from the point of view of the contemporary milieu, he did so in the course of a systematic

and compelling philosophy, as I hope to show in this chapter. Furthermore, he did so in a way that can shed significant light on contemporary concerns and do a better job than some of the most important approaches from the last several decades. The claim that Dewey had a systematic philosophy of science may seem unlikely, given that Dewey never published a systematic work in the philosophy of science; however, I believe that philosophy of science was such a pervasive concern of Dewey's that it features in the vast majority of his works. Because of its thick connections to his more general reflections on logic, knowledge, and inquiry on the one hand,<sup>1</sup> and the more specific reflections on education, politics, morals, etc. on the other hand,<sup>2</sup> he simply never regarded it as appropriate to treat philosophy of science as a *self-contained* matter of concern. As he once said,

The place of science in life, the place of its peculiar subject-matter in the wide scheme of materials we experience, is a more ultimate function of philosophy than is any self-contained reflection upon science as such. (*Context and Thought*, LW 6:19–20)

This is one of the guiding ideas of this project.

The claim that that Dewey's philosophy of science is of continuing contemporary relevance may also seem unlikely, given that his major works on the topic which I will discuss were all written over half a century ago; on the other hand, I believe that the continuing relevance and power of his views betray both the depth of Dewey's insight but more importantly the folly characteristic of the discipline of philosophy of science in the decades following Dewey's death.

The main commitments or features of Dewey's view can be described as (a) anti-skeptical fallibilism and antifoundationalism, (b) situationalism-contextualism, (c) problem-solving inquiry, and (d) the integration of social and value dimensions

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<sup>1</sup>As captured in his *Studies in Logical Theory* (1903), *Essays in Experimental Logic* (1916), *The Quest for Certainty* (1929; LW 4), *Logic: the Theory of Inquiry* (1938; LW 12), and *Knowing and the Known* (with Arthur Bentley, 1949; LW 16)

<sup>2</sup>Especially in works like *Democracy and Education* (1916; MW 9), *Freedom and Culture* (1939; LW 13), and *The Public and Its Problems* (1927; LW 2).

of science. In this section, I will very briefly describe each of these features in order to give a basic sense of Dewey's framework. In the next sections, I will give the background of Dewey's philosophy of science and then a more in-depth interpretation of these and other major features.

Dewey's philosophy is fallibilist and anti-foundationalist in that it regards inquiry as being bound to no *ultimate* fixed points. We might see this as following up on Peirce's dictum: "Do not block the way of inquiry" (EP 2:48).<sup>3</sup> Given the limited position that we are in, taking anything to be permanently fixed and unverifiable prior to inquiry may prevent us from finding our answer. On the other hand, we must take some things as *tentatively* fixed in order for inquiry to get anywhere (Dewey calls such things "operationally *a priori*" in the *Logic*, LW 12:21). We cannot doubt everything at once, because we must take something for granted, and in particular we should not doubt what we cannot find reason to doubt. This is the anti-skeptical position that Dewey shares with Peirce: while we must always take much for granted in order to get anywhere, no particular thing is beyond doubt.

The case of sensation is of course a special one. One might be a fallibilist as I've just described, while nonetheless (on anti-skeptical grounds?) regard the evidence of the senses as generally reliable pieces of immediate knowledge on which to build a system of knowledge. This fallibilistic foundationalism would be unpalatable to Dewey, however, as he is unwilling to regard any knowledge as immediate. For Dewey, there is no such thing as immediate knowledge; rather, assertions about sense-experience, such as "this spot is red," insofar as those assertions are something that can play a role in inquiry, assert a *judgment* about experience, they don't merely report the experience itself, since judgments are always mediated by fallible presuppositions and require certain skills.<sup>4</sup>

Dewey regards all inquiries, and all human activities whatever, as arising

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<sup>3</sup> "The First Rule of Logic" (1898).

<sup>4</sup> Cf. Sellars' account of perception.

from, taking place in, and only comprehensible in relation to a *situation*. A situation is a complex state of affairs including purposeful agents and the physical and social environment that their activity is engaged with. When we talk about “the situation of women in contemporary culture,” of the situation of anthropogenic global warming, or the situation of the AIDS epidemic, we come near to the sort of thing Dewey meant by “situations.” Each has a “whole individual presence for anyone for whom they are indeed concrete situations” (“Introduction” Burke et al., 2002, xvi) All scientific inquiries are also situational; since they respond to particular situations they don’t provide *universal* pictures or descriptions of the world; rather, they provide the means for solving particular problems (though these problems can be of quite wide scope). This shows a serious limit on the applicability of results. One clear example is the conflict that arises between medical research and medical treatment. Medical researchers focused on the problem of identifying and eradicating a particular disease might come to one conclusion, while patients, who take certain side effects of the treatment more seriously, might rightly regard the treatment to *not* be an adequate solution to their problem.

Medical researchers might also run into problems because the context of the laboratory trial might be sufficiently different from the real-world applications as to constitute different situations (this concern is captured in familiar terms as the problem of “external validity” or of “relevance of evidence”). Or, one might take features of atoms to be fixed facts for the purposes of microbiology, while those same claims are highly problematic and contested within some field of physics. What counts as fixed well-enough for getting along varies based on the particular purposes at hand, the situation in question.

One of the most important features of situations for Dewey’s theory of inquiry is that inquiry is prompted by qualities or features of the situation which we might generally call “problematic.” In the course of our activities we are faced with problematic situations which present barriers to unreflective, habitual activity. Inquiry

is our attempt to reflectively cope with these difficulties and return to coordinated activity. In sum—*inquiry is problem-solving*. This slogan will do, so long as we understand that drawing a problem out of the disturbed and discoordinated situation is part of inquiry itself, not external to it. This is a rather schematic definition of inquiry and its aim—it leaves open more particular values and goals, what counts as a solution, how evidence is selected, etc., and all the better, since many of these things will differ in different situations. What it does tell us is that all inquiry, science included, is concerned primarily with coping with problems and transforming situations so as to resolve them, not with disinterestedly describing or modeling of the World. Further, it recommends that our philosophy of science analyze the parts of inquiry in terms of their functional role in leading to resolution.

Dewey's picture of inquiry is one in which social and value dimensions loom large. Situations as Dewey defines them are not solipsistic, but things that can be shared by a group of agents defined by some shared interest. Far from being an impediment to objective inquiry, the social dimensions of scientific knowledge contribute essentially to its power and rationality. The fact that we can pool our resources towards the exercise of social forms of intelligence is crucial for Dewey in both his philosophy of science and his epistemological argument for democracy.

Because science is embedded in our lives and activities, as well as larger social systems, and because it is a practice of its own distinctive character, science is deeply integrated with matters of value. Because science is a practice, it has a distinctive normative structure, and thus it has its own set of values. Further, because science is concerned primarily with manipulating the world in order to gain knowledge that will solve problems, it is essentially involved with not just *theoretical* or *factual* but *practical* judgment. And since the process of problem-solving necessarily takes place on the background of personal and social lives and concerns, it is likewise colored by our broader values as well. This is not cause for concerns about the objectivity or validity of science; rather, science as a tool of problem-solving can actually aid us in



reflecting on and reevaluating what is really *valuable*.

## 2.2 Background

### 2.2.1 Lived and Historical Context

Dewey's philosophy arises in an important historical moment.<sup>5</sup> Dewey was born in 1859, the same year that Darwin's *On the Origin of Species* was published. He got his B.A. in 1879, the same year that William Wünderst founded the first laboratory of experimental psychology. Dewey's early influences in philosophy were German idealism—Kant and Hegel—and the liberal Congregationalist and social gospel tradition in Christianity. Each of these left a “permanent deposit” in Dewey's thought, especially Hegel's philosophy. As he said, looking back over his philosophical influences,

The form, the schematism, of [Hegel's] system now seems to me artificial to the last degree. But in the content of his ideas there is often an extraordinary depth; in many of his analyses, taken out of their mechanical dialectical setting, an extraordinary acuteness. Were it possible for me to be a devotee of any system, I still should believe that there is a greater richness and greater variety of insight in Hegel than in any other single systematic philosopher. (ED 1:18, LW 5:154)

There has been significant debate about the nature of this “permanent deposit” of Hegel in Dewey's thought.<sup>6</sup> Without entering in to this significant and important issue between Dewey scholars, it seems uncontroversial that Dewey accepted many of Hegel's criticisms of prior philosophers, especially Kantians, empiricists, and any manner of “dualism” or strict opposition. Dewey's philosophy also retains the dynamic and historicist qualities of Hegel's system, as well as its contextualism (though

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<sup>5</sup>Westbrook (1991) gives one of the best accounts of Dewey's life. Boisvert (1998) provides an accessible overview of Dewey's philosophy, in which his ideas are placed in their historical context. (Boisvert also makes interesting use of literary examples throughout.)

<sup>6</sup>See Shook (2000); Good (2006b,a); Garrison (2006).

Dewey's contextualism doesn't become Hegel's universal holism). Perhaps most radically (and most difficult to interpret) is Dewey's allegiance to the idea that the process of knowledge *creates*—or modifies—its objects. Dewey's re-interpretation of this idea in an experimentalist and non-idealist mode presents one of the most challenging and difficult features of his philosophy.

Even during the heyday of Dewey's Hegelianism, his ideas were greatly impacted by the work of Charles Darwin and the emerging field of empirical psychology.<sup>7</sup> Engagement with Darwin's ideas deepened the historicism and anti-dualism that Dewey was already taking from Hegel. What Dewey saw in Darwin was not simply an argument for a naturalistic picture of biology generally and humanity and particular. In "The Influence of Darwin on Philosophy" (MW 4:3–14), Dewey argued that Darwin's great insight was in showing how the traditional concept of species as fixed and final should be replaced by one of species as having an origin and development. What Darwin did for species, Dewey thought ought to be done for all philosophical ideas and philosophical problems. Hence Dewey's rejection of "inquiry after absolute origins and absolute finalities in order to explore specific values and the specific conditions that generate them" (MW 4:10). In this quest, Dewey liberally employed psychological and sociological analyses and a genetic-historical method to the concerns and conceptions of the philosophy of his day. He doggedly attempted to tie philosophy to the vital context that could give it life, and prevent it from falling into sterile abstraction.

It is important to recognize the incredible impact on Dewey's thinking of his first wife, Alice Chipman Dewey.<sup>8</sup> Dewey met Alice early on in his teaching career at the University of Michigan, where she was his student and neighbor. Alice was an intense and intelligent woman who was especially responsible for pushing Dewey in two directions: away from organized Christianity "to a barely Christian social

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<sup>7</sup>These two accomplishments (rather than physics, say) were ever after to be the paradigmatic cases of science for Dewey.

<sup>8</sup>See Westbrook (1991, 34–6)

gospel” (Westbrook, 1991, 35) in which *democracy* rather than God was the main focus, and away from abstract thinking focused on the history of philosophy to an active engagement in concrete and live human affairs.

Though it has sometimes been overplayed, the publication of William James’ *Principles of Psychology* in 1890 was no doubt a sea change moment for Dewey. Dewey had published his own poorly-received *Psychology* three years earlier, an attempt to work out his philosophy of *experimental idealism*. It was James’ “*biological* conception and mode of approach” (ED 1:20, LW 5:157, my emphasis) and his conception of life as dynamic and active rather than structural, static, and mechanistic that fully thrust Dewey out of Hegelian abstraction and onto the path of his mature views. Further, “the objective biological approach of the Jamesian psychology led [Dewey] straight to the perception of the importance of distinctive social categories, especially communication and participation” (ibid.). The “cultural naturalism” that Dewey refers to in his *Logic* (LW 12:28) has its germ in Dewey’s reading of James’ psychology. Important to Dewey’s development at this time also was his Michigan colleague George Herbert Mead, who had trained under German psychologists like Wünder. Mead and Dewey collaborated for many years on working out a social psychology of the sort that Dewey thought followed from James.

The formative details discussed so far largely precede Dewey’s move from Michigan to the University of Chicago in 1894. In Chicago, Dewey headed up the department of Philosophy, Psychology, and Pedagogy (he remained head of the latter when it was split off into its own department a few years later). Dewey took on a particularly important project in his years in Chicago, the founding of an experimental elementary school, the famous “Dewey School” or “Laboratory School.” This was both a functioning school and an experimental laboratory, where Dewey attempted to apply the methods of experimental science to the crucial field of education, by trying out promising new educational methods. This is one of the key examples in action of an idea that Dewey was to develop and defend throughout his career: that

the methods of scientific inquiry, broadly understood, could be fruitfully applied in solving social problems.

Also in Chicago, Dewey met Jane Addams and discovered her social settlement work at Hull House. Settlement houses were an important feature of progressive era urban social work, and Addams was one of the leaders of the American settlement house movement. Here, too, Dewey encountered a kind of experimental practice of social reform. One of the important features of Addams' approach to the social work was her thorough avoidance of technocratic or patriarchal social engineering. Hull House was not a matter of the better-off reforming the worse-off using their superior skills and resources. Addams and her partner, Ellen Gates Starr, opened Hull House with "very little by way of specific directions for what the settlement would be other than a good neighbor to oppressed peoples" (Hamington, Winter 2008). They opened the door to the community and asked what they could do to help. Hull House became an "incubator for social programs" (ibid.), usually responding to the initiative or needs of those they intended to help, rather than imposing (religious, political, ideological) ideals from the top-down. Addams helped Dewey to give up his Hegelian conviction that open (even violent) social conflict was a necessary part of the dialectic of social progress, to reinforce his commitment to the social values of communication, participation, and cooperation.

In 1904, Dewey left Chicago for Columbia. At Columbia, Dewey led a more traditional philosophical life. He avoided administration, and while he participated in some important social moments (the Trotsky investigation, the founding of the NAACP and the ACLU, etc.), he was much more heavily involved in the work of academic philosophy. His arrival coincided with the founding of the famous in-house *Journal of Philosophy, Psychology, and Scientific Method* (today known simply as the *Journal of Philosophy*), and from the time of its inception until his death in 1952, he published over one-hundred separate items in the pages of that journal. It is here that he worked out the full intellectual force of his ideas, through interactions

with colleagues like F.J.E. Woodbridge and students (later colleagues themselves) like John Herman Randall, Jr., Ernest Nagel, and Sidney Hook.

These are the main features of Dewey's life and historical context that had a formative impact on his thinking. I have discussed key features of his late life in the previous chapter. Now it is time to discuss the main features of his view, especially as the impact his thinking on science.

## 2.2.2 Guiding Principles

Dewey attacks fundamental problems in epistemology, logic, and philosophy of science, but he does so *not* in the manner of careful elaboration of and argument for epicycles of established philosophic theory, nor by cautious and piecemeal revision of familiar doctrine, nor by carefully elaborating history or scientific data and attempting to draw philosophic conclusions from it. Dewey sees cracks in the structure of our philosophical discussions, difficulties so systematic that the best way forward is to shake the thing down to the ground, clear the rubble away, and begin again. Dewey doesn't simply reject everything that comes before; he sees valuable building techniques in thinkers as diverse as Plato and Aristotle, Hegel and Darwin, Mill and Emerson, and he takes much inspiration from William James and C.S. Peirce as well. He also sees much wisdom both in our traditional or common sense ways of solving problems and in science. Yet he uses their insights to start anew.

There are several guiding orientations of Dewey's philosophy that should be pointed out from the start. The first is *naturalism*. This is not the reductive naturalism of Churchland or Quine. Nor does it even have much to do with the relationship of science and philosophy. Rather, Deweyan naturalism is simply the view that man and his activities are *natural events*, that our abilities and practices are *continuous* with those of other organisms in a way very generally like Darwin says. Though Dewey never gives an explicit analysis of his principle of continuity (or its connec-

tion to Darwin), its importance in his work is pervasive. Thomas Alexander has dedicated a significant portion of Ch 3 of his book on Dewey to the topic. The clearest discussion comes in Ch 2 of Dewey's *Logic*:

The primary postulate of a naturalistic theory of logic is continuity of the lower (less complex) and the higher (more complex) activities and forms. The idea of continuity is not self-explanatory. But its meaning excludes complete rupture on one side and mere repetition of identities on the other; it precludes reduction of the "higher" to the "lower" just as it precludes complete breaks and gaps. The growth and development of any living organism from seed to maturity illustrates the meaning of continuity. (LW 12:30)

Saying that, e.g., logic is continuous with biological functioning means neither that it is reducible to biology, nor that it comes from some entirely separate realm; it grows out of prior activities just as the oak tree grows out of the acorn. Thus, on Dewey's naturalism, nothing we can rely on or talk about in any practice, including philosophy, stands outside of, apart from, or in contrast to nature.

The second basic orientation is *immediate empiricism*.<sup>9</sup> Unlike classical empiricism, immediate empiricism is not so much an epistemological theory or framework as a principle of respect for experience in all of its manifestations. Immediate empiricism cautions us to give weight not just to the experience of cognition or knowing, or scientific experience, but also to our experience of family life, of coping with everyday problems, of work and leisure, of art and religion, of technology and wilderness, of love and loss. Dewey goes as far as to say that immediate empiricism postulates that "things... are what they are experienced as" (MW 3:158). This incautious, or at least difficult to interpret statement really means that, in philosophy

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<sup>9</sup>The name is probably an unfortunate one, since *empiricism* is generally taken as an epistemological doctrine, whereas *immediate* empiricism is a quasi-metaphysical position. William James' "radical empiricism" is a very similar view, and Dewey may have been following James. "Radical experientialism" is a name that would help a little bit, since it avoids the unfortunate use of "empiricism." Dewey sometimes talks simply of "immediatism," though this is also unfortunate in the course of his epistemology, since he rejects the idea of immediate *knowledge* (though not, as we shall see, of immediate experience).

at least, we cannot privilege any one type of experience over the others *tout court*, nor can we make sense of one type of experience as being any more “real.”

These two orientations together form what Dewey calls prominently *empirical naturalism*. This doctrine figures especially in *Experience and Nature*. Naturalism tells us to regard experience as itself a feature of nature, not something that stands apart from it. And being immediate empiricists about experience itself tells us that experience involves not only neurons or minds, but also our whole environment: coffee cups and books and trees and other people. Human experiences and affairs are *of* nature as well as taking place *in* nature. To quote Dewey at length:

[E]xperience is *of* as well as *in* nature. It is not experience which is experienced, but nature—stones, plants, animals, diseases, health, temperature, electricity, and so on. Things interacting in certain ways *are* experience; they are what is experienced. Linked in certain other ways with another natural object—the human organism—they are *how* things are experienced as well. Experience thus reaches down into nature; it has depth. It also has breadth on an indefinitely elastic extent. It stretches. That stretch constitutes inference. (*Experience and Nature*, LW 1:12–13)

And in a later clarification of his views, Dewey writes:

To me human affairs, associative and personal, are projections, continuations, complications, of the nature which exists in the physical and pre-human world. There is no gulf, no two spheres of existence, no “bifurcation.” For this reason, there are in nature both foregrounds and backgrounds, heres and theres, centers and perspectives, foci and margins. If there were not, the story and scene of man would involve a complete break with nature, the insertion of unaccountable and unnatural conditions and factors. To any one who takes seriously the notion of thoroughgoing continuity, the idea of existence in space and time without heres and nows, without perspectival arrangements, is not only incredible, but is a hang-over of an intellectual convention which developed and flourished in physics at a particular stage of history.

*It is not pragmatism* nor any particular philosophical view which has rendered this conception questionable, but the progress of natural science.

One who believes in continuity may argue that, since human experience exhibits such traits as Santayana denies to nature, the latter *must* contain their prototypes. (“Half-Hearted Naturalism”, *J Phil* (1927) LW 3:74-5)

In this picture, experience does not sit apart from and more or less accurately reflect nature or a part of nature. Nor does it sit merely at the surfaces of things. We are always already deeply engaged with many things in many different ways, and that is just what experience is.

The final orientation that I will mention here is *pragmatism*. It might be strange to refer to this as just one feature of his thought, rather than a name for the whole, but Dewey (and Peirce and James, too) always regarded it as *one* feature of his thought, rather than the whole (just as “empiricism” is one feature of the complex and otherwise quite different views of Locke, Hume, Mill, Russell, and Carnap, for instance). For Dewey, pragmatism amounts to the tendency to analyze philosophical ideas in terms of their *function* in purposeful and embodied human practices and life-experience; hence, his project of analyzing logic and knowledge in terms of our actual practices of inquiry (or knowledge-making), rather than in terms of their essences or abstract, timeless character. While this might be a non-standard definition of pragmatism, I think it accords well with how Dewey thought of that commitment, and it was certainly something he more-or-less shared with Peirce and James.<sup>10</sup>

Finally, it is worth remarking on Dewey’s style. Because of the fundamental way in which Dewey wishes to rework philosophy, there is little that can be taken for granted and much which must be carefully understood. Philosophers like Peirce and Heidegger have attempted to facilitate this by developing a technical vocabulary. Dewey does not do this. Because he hopes to speak to a wide audience, and not necessarily only to philosophers, and because of a deep belief in the wisdom of common sense over philosophical sophistication, he attempts to speak in plain English. This is highly misleading to philosophers who bring their own baggage to his

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<sup>10</sup> Though these things are notoriously difficult to pin down. See A.O. Lovejoy, “The Thirteen Pragmatisms”; Robert Talisee, “Two Concepts of Inquiry.”



terms, and he provides few signposts to make the differences clear. I will try to do better, but it is important from the outset, as we've already seen with "experience," and we will come to see with "situation," "fact," "hypothesis," etc., not to assume too much about what Dewey meant by these terms. Further, because it is important to rethink much philosophical bedrock, it is more important to Dewey to set out a coherent framework in concordance with his basic commitments than to provide airtight arguments for or against specific points. Likewise, it will be sufficient for my purposes in this work if I can set out a clear, coherent framework that provides compelling ways of dealing with the problems that now concern us, rather than arguing each step of the way against current approaches or arguing for each component of the view independently from the usefulness of the whole.

### 2.2.3 Dewey's Philosophical Projects

Dewey's philosophy of science cannot be fully understood and appreciated if it is entirely cut off from his other philosophical interests. Some of these interests start from his very early Kantian and Hegelian days, and drove him into philosophy in the first place. Others accrued due to some important experiences in Dewey's life, such as his work at the Dewey Laboratory School in Chicago. I am inclined to think there is something right in the views of both the *continuity* interpretation, that Dewey's fundamental thinking is continuous from his idealist to his naturalist periods, and the *rupture* interpretation, who see significant differences between the earlier and later periods. It seems to me that some of Dewey's main projects and positions from his idealist period survive into his late work, though some are dropped and more accrue throughout his life.

**Freedom and value.** One of Dewey's major projects, from the very beginning, is an attempt to, through ideas and action, ensure human freedom and make room for human value. Traditional philosophical theory to a large extent, and

some areas of natural science, as well as many types of religious and political powers, threatened our freedom and value.

**Democracy.** Another main goal is a sustained defense of democracy, not as a procedure or institution, but as a way of life. There are deep connections between the scientific spirit and the democratic way of life.

**Education.** From his work at the Dewey school, and perhaps before, Dewey was concerned to improve education, to make it more scientific, to make it compatible with freedom, value, growth, our social natures, and the democratic way of life.

**An ethical, public science.** Dewey sought not only to make morals, politics, and education more scientific, he also sought to make science more moral and more responsible to the people. One large component of this was his attempt to bring the lessons of science into everyday life. Another was his explicit recognition of the values and interests driving science.

With these preliminaries finally out of the way, I will now go on to outline what I take to be the main features of Dewey's philosophy of science. As Don Howard has suggested,<sup>11</sup> Dewey never set out in one work to provide a philosophy of science, but philosophy of science was nonetheless an abiding concern of his work. There are many places to look for clues to his philosophy of science, including his writings on the sciences of psychology and education, his discussions of the relation between science and society/politics, his theory of values and valuation, his attempts to provide a more scientific basis for ethics, and so on. I will focus primarily on his works on logic and epistemology, his theory of inquiry, which embody his attempt to synthesize (and make widely available) the most general lessons of scientific and common sense inquiry. This must be supplemented with more specific remarks about the nature of science in contrast to common sense, though the emphasis will largely be on their continuity.

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<sup>11</sup> "Progressivism, Pragmatism, and Science: John Dewey's Theory of Science," unpublished talk, presented at PSA 2008. Slides online at ([http://www.nd.edu/~dhoward1/Dewey's Theory of Science.pdf](http://www.nd.edu/~dhoward1/Dewey's%20Theory%20of%20Science.pdf))

### 2.2.4 The relation of logic to science

Dewey, not unlike contemporaries such as the logical positivists, often discussed philosophy of science in relation to “logic.” This is one *prima facie* reason to ignore him today, since the close connection of discussions in logical theory and philosophy of science now seems a quaint and mistaken conceit of mid-twentieth century analytic philosophy. Ever since Kuhn, it has become harder and harder to insist that science can be analyzed primarily by the use of logic.<sup>12</sup> The imposition of formal logical methods in the reconstruction of the practice of science is seen now as a poor tool for the job, perhaps even to have *hindered* the progress of philosophy of science, and we now travel a hard road of trying to analyze scientific practice, the social dimensions of science, the role of values of science, and all the core areas of philosophy of science by different means. Looking to Dewey’s writings on logic for inspiration in philosophy of science may thus appear to be a dangerous bit of recidivism.

Dewey differs from his contemporaries by *denying* that logic is *imposed* on science from the outside. Logic just is, for Dewey, a theory of inquiry. While philosophers like Russell and Carnap believed that logic was to be developed on its own and then used as a tool to deal with problems in philosophy, Dewey believed that there was a more complex relationship between reason and practice. In the case of science, Dewey had to fight the “well recognized distinction between methodology and logic, the former being an application of the latter” (*Logic*, LW 12:12). This common assumption of logic’s independent status from any of its uses, and that scientific method (or philosophy of science in general) is simply applied logic, is precisely the untenable stance we have come to reject. That Dewey also rejected it is clear, but he did not take this to mean that logic is thus unrelated to philosophy of science:

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<sup>12</sup>The present-day inheritors of this tradition being the Bayesians, who still hold that science is profitably analyzed not according to the formal apparatus of the predicate calculus but rather the probability calculus.

But it may be noted that the assertion *in advance* of a fixed difference between logic and the methodology of scientific and practical inquiry begs the fundamental question at issue. The fact that most of the extant treatises upon methodology have been written upon the assumption of a fixed difference between the two does not prove that the difference exists. . . In any case, the *a priori* assumption of a dualism between logic and methodology can only be prejudicial to unbiased examination both of methods of inquiry and logical subject-matter. (*LW 12:13*)

What logicist philosophers of science and their contemporary critics seem to share is the assumption that logic and methodology are distinct from one another, and where they differ is on the question of whether methodology is an application of logic, or whether logic is largely irrelevant for methodology.

Scientific method is not simply the application of logic, on a Deweyan view, and neither is it irrelevant to logic. Rather, logic and scientific method exist in a dialectical relationship; they mutually inform one another. At base, Dewey's theory is that:

[A]ll logical forms (with their characteristic properties) arise within the operation of inquiry and are concerned with control of inquiry so that it may yield warranted assertions. (*LW 12: 11*)

Dewey agrees with the logicist that "Inquiry in order to reach valid conclusions must itself satisfy logical requirements" (*LW 12:13*). What he denies is the "easy inference from this fact to the idea that the logical requirements are imposed upon methods of inquiry from without" (*ibid.*). Rather, the logical requirements on inquiry come from within inquiry itself, because of inquiry's self-corrective processes. Dewey would point, as our anti-logicist contemporaries might also, to the fact that improvement in scientific methods came from within science, rather than from developments outside. The new methods of epidemiology were not produced by formal logicians or even statistical mathematicians, but rather by researchers like John Snow, on the ground, trying to deal with epidemics. The history of science is replete with methodological

innovations, and they come by and large not from the *a priori* pronouncements of philosophers, but from developments in the sciences themselves.

Relativism and skepticism also threaten Dewey's position. How can inquiry be, at one and the same time, judged by a standard and the source of that standard? Dewey's answer is to point to the *historical* character of the standards,<sup>13</sup> to the fact that while there are standards that apply to a particular inquiry, that inquiry is also embedded in a continuum of inquiry:

The developing course of science thus presents us with an immanent criticism of methods previously tried. Earlier methods failed in some important respect. In consequence of this failure, they were modified so that more dependable results were secured. Earlier methods yielded conclusions that could not stand the strain put upon them by further investigation. It is not merely the *conclusions* that were found to be inadequate or false but that they were found to be so because of the methods employed. Other methods of inquiry were found to be such that persistence in them not only produced conclusions that stood the strain of further inquiry but that tended to be self-rectifying. (*Logic*, LW 12:13-14)

## 2.3 Anti-skeptical fallibilism

Nothing is so fixed and obvious as to be impervious to challenge by future inquiry:

While the direct use of objects, factual and conceptual, which have been determined in the course of resolving prior problematic situations is of indispensable practical value in the conduct of further inquiries, such objects are *not exempt* in new inquiries from need for reexamination and reconstitution. (*Logic* LW 12:143)

There are no facts, no theories, no knowledge, no forms of inference, no categories, no concepts, no terms that are *a priori* unable to be revised in the attempt to solve

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<sup>13</sup>See Colapietro (2002); Seigfried (2002)

a problem or resolve a doubt in inquiry. What Dewey offers is a very strong form of fallibilism: there are no materials of cognition, no tools or products of inquiry which are set from the begging as indubitable and unchangeable. This is not to say that we have no absolute fixed points whatsoever. That we experience, that we engage in activities, that we enjoy and suffer, these are not something that we can coherently doubt, however we attempt to capture them intellectually. Our experience is what it is, and though there is a sense in which inquiry can change our experience (by reconstructing the world that we experience in the future, or by re-interpreting the experience of the past), it cannot make us doubt what we have or are experiencing, because these are not things that can be used directly by inquiry, but rather the experiential-practical background in which all inquiry takes place. What can be doubted, rather, are our ways of describing or understanding our experience, as well as our theory of experience and even this fallibilistic epistemology itself.

This degree of fallibilism begins to sound dangerously like skepticism—if nothing is indubitable, then everything is uncertain, and thus we can't *really* know anything. We can't be certain of fallibilism. We can't even be sure that we're even making any sense with what we're saying right now! We can't even be sure we're saying, or that we're here now, or that we're us! These worries play upon an ambiguity in terms like “indubitable,” “certain,” “doubtful,” and “uncertain.” It is true that to be reflexively coherent, we must be fallibilists about fallibilism, and even the meaningfulness of the doctrine of fallibilism, but Dewey shows us how to avoid falling into skepticism by constructing an antiskeptical fallibilism. In any case, Dewey's fallibilism is not arrived at *a priori*; it is a bet based on examining previous inquiries:

The history of science also shows that when hypotheses have been taken to be finally *true* and hence unquestionable, they have obstructed inquiry and kept science committed to doctrines that later turned out to be invalid. (LW 12:145)

Can we be certain of anything? It depends on what one means by “certain.” If being certain means being completely indubitable and unrevisable in the future, then no; it is in this sense that Dewey engaged in a sustained critique of the “quest for certainty.” If certainty is rather an attitude we have towards some bit of knowledge, then we can be *and are* certain of many things, in that we have and can see no reason to doubt them. As Peirce has shown us,<sup>14</sup> there is nothing more we can ask of belief; once we have inquired into the matter and no longer have any reason to doubt something, we find that we cannot but accept it. It is not just belief that we require justification for, but we must also have a reason to doubt. Furthermore, while we might come to doubt any particular belief, we most certainly cannot doubt all our beliefs at once. To even make sense of doubt requires a stable background of presently unquestioned (though not forever unquestionable) beliefs.

## 2.4 Anti-foundationalism without coherentism

One might think that it is a direct corollary of Dewey’s type of fallibilism that he is an anti-foundationalist. If we’re fallibilists even about empirical facts and observation-statement, then these things can’t act as firm foundations for grounding other knowledge. Yet, it *cannot* be that obvious a corollary, by dint of the fact that, while almost no one these days believes that we can ground our knowledge on absolutely certain foundations, many go on to conceive of the theory-evidence relation in basically foundationalist ways. That is to say, while fallibilism is about how no part of our system of knowledge can be held with *complete and permanent* certainty, the question of foundationalism is a question about the nature and direction of the *support relation* that structures our system of knowledge. I shall call a view “fallibilist foundationalism” if it espouses fallibilism about data, yet nevertheless regards the data as fixed in relation to theory, and regards justification as going *from*

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<sup>14</sup> e.g. in “The Fixation of Belief”

data *to* theory.

Perhaps one reason why fallibilist foundationalism is so common is that the obvious alternatives are highly unsatisfying. Perhaps the best alternative that receives common attention is epistemic holism, or justificatory coherentism. Quine's "web of belief" framework is one of the most famous of such views. Quine regards all beliefs, be they perceptual beliefs, beliefs of common sense, scientific theories, or principles of logic and mathematics as linked together in a justificatory web. This requires some fundamental connections of coherence between the beliefs. New evidence coming in that doesn't easily mesh with the standing web can cause a number of reactions in attempt to render the web once again coherent. As one option open in the process is simply rejecting the evidence, it constitutes a more thoroughgoing fallibilism about evidence, and a clear anti-foundationalism.

This view is unsatisfying in a variety of ways. Most importantly, it brushes over the obvious differences in scientific practice between data, evidence, and experiment on the one hand, and speculation, theory, and hypothesis on the other. These things don't play the same sort of role in actual inquiries (but neither do they play the role foundationalists attribute to them). Furthermore, there is a significant worry that a simple coherentist model of justification will be overly conservative. Even if we can explicate a holist model which doesn't just tend to insulate basic beliefs from criticism and reject anomalous data (a dubious proposition), it is not clear what keeps such a model from falling into a stable state far below the optimum. And since the only pieces in the game for the holist are beliefs of different sorts, none of which has any "special access," then it isn't clear how the whole system gets the right sort of traction on the world in the first place.

As we've seen in the previous section, Dewey explicitly and at length rejects the idea that there is any such thing as "immediate knowledge," i.e., knowledge that we get without any mediating processes of inquiry. Not only are our observations or data fallible, but they are also themselves *constructed* or *mediated* by processes of



inquiry. Dewey's theory takes plays from both the foundationalist and the coherentist playbook. From the former, he takes a view that facts and theories really play quite different functional roles in inquiry, and he innovates by saying it is the pragmatic distinction based on taking these roles than makes something a fact or a theory, rather than something being qualified as essentially a fact or a theory that qualifies them for these roles. Like the coherentist, Dewey allows that facts can be revised or rejected on the basis of not meshing well with theory, though ultimately he will point towards situational and pre-cognitive, qualitative factors as the judge of both (i.e. providing traction), rather than having only the clash of different beliefs.

## 2.5 Inquiry defined

On Dewey's account, the most "basic conception of inquiry," and therefore the most in need of clarification and explanation, is "as determination of an indeterminate situation" (*Logic*, LW 12: 3). It is an iterative process of gathering facts, refining hypotheses, and experimental testing in order to solve a problem. Or, to indicate that problem-formulations aren't fixed and that their determination is an important part of inquiry, we should say that the goal is rather to resolve some perplexity or problematic situation. It is an iterative process in that it requires successive attempts to refine all of the elements—facts, hypotheses, problem-statements, solutions—in light of the others until a settled conclusion is reached. Processes of inquiry are active processes of an embodied human agent; perplexities and problems arise, not out of intellectual considerations, but out of experienced difficulties in navigating situations and successfully conducting affairs. They are resolved by transforming the relations between agent and environment in a way that removes the difficulties.

Dewey's key definition is:

Inquiry is the controlled or directed transformation of an indeterminate

situation into one that is so determinate in its constituent distinctions and relations as to convert the elements of the original situation into a unified whole. (*Logic*, LW 12: 108)

The importance of the term “situation” will be discussed in the next section. An *indeterminate* situation is one that is marked by a certain kind of perplexity or difficulty, one which is unclear, uncertain, ambiguous, doubtful, or precarious with respect to *what is going on* and *what is to be done*. While Dewey is keen to explain the impetus for inquiry in a way that is non-subjective, located in a situation rather than in the mind, his pragmatism leads him to analyze it in these terms because they make clear that a disruption of human *practice* is at issue. Once we recognize that the perplexity or the indeterminacy of the situation should be resolved by undertaking inquiry, we call it a “problematic situation” or simply a “problem” (problems or problematicity, then, being strictly speaking all and only what is open to inquiry).

As in C.S. Peirce’s doubt-belief schema for the fixation of belief, all genuine inquiry must address a genuine doubt or a genuine problem. For Peirce, this meant that the impulse to inquiry had to be a state of doubt characterized by a certain feeling of uncertainty or difficulty (phenomenological component); it had to act as an impediment to action (practical component); and it had to lead to a genuine struggle after new belief (epistemic component). All of this is compatible with doubt being a personal mental state, which was Dewey’s greatest worry about Peirce’s theory. For Dewey, the impulse to inquiry comes from a *situation*, not an individual or a mental state. The situation is characterized by a “pervasive qualitative character” of doubtfulness, precariousness, uncertainty, indeterminacy, unsettledness, etc. This overall aesthetic quality or mood that characterizes the situation stands in for the phenomenological component of Peirce’s “doubt.” Dewey retains Peirce’s practical component as well; indeed, the situation is in part defined by the interactions or transactions between agents and environments that characterize human practices, and the doubtfulness that characterizes an indeterminate situation is a result of

some discoordination or disequilibrium in those interactions.

It is important to recognize, however, that scientists do not passively wait around for genuine problems or doubts to arise on their own:

Here is where ordinary thinking and thinking that is scrupulous diverge from each other. The natural man is impatient with doubt and suspense: he impatiently hurries to be shut of it. A disciplined mind takes delight in the problematic, and cherishes it until a way out is found that approves itself upon examination. The questionable becomes an active questioning, a search; desire for the emotion of certitude gives place to quest for the objects by which the obscure and unsettled may be developed into the stable and the clear. The scientific attitude may almost be defined as that which is capable of enjoying the doubtful; scientific method is, in one aspect, a technique for making productive use of doubt by converting it into operations of definite inquiry... Attainment of the relatively secure and settled takes place, however, only with respect to *specified* problematic situations; quest for certainty that is universal, applying to everything, is a compensatory perversion. One question is disposed of; another offers itself and thought is kept alive. (*The Quest for Certainty*, LW 4:182)

So while inquiry transforms *some* indeterminate situation into one that is settled and secure, there is no reason to think that science as a whole is moving from less to more uncertainty, from more to fewer problems. New conditions and new results may spur new problems, and in any case, scientists positively go hunting for new problems to attack.

Inquiry is a *transformation* in the sense that the resolution of a problematic situation or perplexity in general requires not just a change in the inquirer's state of mind or beliefs, but a change in the situation itself. Again, a pragmatist and empirical naturalist orientation leads Dewey to regard a mere change of mind, leaving everything else unchanged, as nonsense. Even when no physical modification of an environment is necessary to resolve a problem, changing the beliefs, attitudes, and habits of the inquirer changes the relations, interactions, and activities that characterize the situation.

Inquiry is *controlled or directed* in that it is a conscious and reflective attempt to determine and resolve the problem, and also to the extent that it does so in light of normative constraints on inquiry (where the nature and extent of those norms will be discussed below). The resolved or settled situation is *determinate* in the sense that the constituents of it which were at the beginning unclear or ambiguous and the possibilities of action within it which were once uncertain are now settled and clear. In the ideal case of inquiry fully carried out, the situation is so fully determinate that it is a *unified whole*, in the sense that it all hangs together, that it presents a fully unambiguous field for action.

## 2.6 The importance of the situation

It is crucial to understanding Dewey's theory of inquiry to understand that inquiry takes place in and is directly concerned with a *situation*, and what the import of that is. A situation is not an objective, perspective-free spatiotemporal region. Neither is it a subjective theatre of appearances. It is rather, Dewey says, an "environing experienced world" (*Logic*, LW 12:72–3). It is not merely the space-time worm of a particular agent plus some parts of their environment, neither of fixed nor variable extent. Situations can be shared by many people, they are overlapping and multiple. The situations I inhabit roughly involve everything that is involved in my affairs, and the individuation of situations will accord with how I characterize my practices, with my purposes in doing so. Situations have a complex structure: they include a foci, a foreground, background, and horizon. They include both discretely discriminated objections and the background upon which discriminations take place. Nevertheless, a situation is experienced as a whole: "A situation is a whole in virtue of its immediately pervasive quality" (LW 12:73)

Dewey attempts to clarify the concept of a "situation" in response to a letter from Albert G.A. Balz:

“Situation” stands for something inclusive of a large number of diverse elements existing across wide areas of space and long periods of time, but which, nevertheless, have their own unity. This discussion which we are here and now carrying on is precisely part of a situation. Your letter to me and what I am writing in response are evidently parts of that to which I have given the name “situation”; while these items are conspicuous features of the situation they are far from being the only or even the chief ones. In each case there is prolonged prior study: into this study have entered teachers, books, articles, and all the contacts which have shaped the views that now find themselves in disagreement with each other. (LW 16:281-2)

Consider another example: as I sit attempting to characterize John Dewey’s philosophy of science, the foreground of my situation includes my computer, the texts I’m looking at, my notes and my thoughts about how best to explain his views. The focus of the situation is the very text I’m composing. In the background, there are a variety of other texts I’m not at the moment thinking of, a community of Dewey scholars, philosophers of science, and others who are the potential audience of the text, but whom I am not attending to directly at the moment. The horizon of the situation, which at the moment is pretty obscure to me, might be the borders of those things that are and are not relevant to constructing my interpretations and arguments.

This situation, the stage on which my dissertation is constructed, is one that I’ve been in for almost two years now, and I will be in it until the project is complete. But I’m also in many other situations during that time: when I’m teaching a class or fixing Pat’s computer, there are other situations for those activities. The plight of student workers at the University of California, the difficulties of the southern California housing market, and my developing romantic relationship are all *situations* that I have had to navigate in these times.

## 2.7 Normative force

Dewey doesn't always clearly distinguish apparently descriptive points about the psychology or behavior of inquirers or scientists and the normative recommendations about how inquirers *ought* to behave. This isn't merely confusion on his part, however. It is due primarily to his own particular conception of normativity:

Up to this point, it may seem as if the criteria that emerge from the processes of continuous inquiry were *only descriptive*, and in that sense empirical. That they are empirical in one sense of that ambiguous word is undeniable. They have grown out of the experiences of actual inquiry. But they are not empirical in the sense in which "empirical" means *devoid of rational standing*. Through examination of the relations which exist between means (methods) employed and conclusions attained as their consequence, reasons are discovered why some methods succeed and other methods fail. . . *rationality* is an affair of the relation of means and consequences, not of fixed first principles as ultimate premises. . . (*Logic*, LW 12:17, emphasis mine)

Norms for Dewey are neither a priori edicts, nor are they ultimate ideals. Rather, they are something like the principles of engineering or medicine. Engineering principles are not categorical imperatives or eternal ideals, they are rather the rules you follow when you want to build a sturdy bridge or a working car. Medical principles likewise tell you what you ought to do if you want to get well and stay well. Obviously, neither are derived a priori, but rather from looking at actual medical treatment, and none of them are infallible either, as they can be rejected if they fail to work, and they can be revised in the course of further application. They are not ultimate or eternal ends, but *ends-in-view* which might later be seen as means towards further ends. In the case of his theory of inquiry, he provides recommendations for inquirers about how to proceed in order to more easily reach more stable and warranted solutions, drawn from looking at many cases of past inquiry that were and were not successful. This is not merely an instrumental characterization of rationality, either, because,

In framing ends-in-views, it is unreasonable to set up those which have no connection with the available means and without reference to the obstacles standing in the way of attaining that end. (LW 12:17)

Dewey again gives as a picture where means and ends are *dialectically* related, and one in which epistemology (or the theory of inquiry) is *explicitly* recognized as itself a work-in-progress rather than a set of fixed, eternal truths.

Dewey is giving us a picture of inquiry ideally carried out. Inquiries are real phenomena, and some of them are carried out in the way Dewey claims. The logical-methodological principles that Dewey sets out are meant to articulate the habits and practices involved in successful inquiries (LW 12:20); they are “formulations of conditions, discovered in the course of inquiry itself, which further inquiries must satisfy if they are to yield warranted assertibility as a consequence” (LW 12:24). Such principles (sometimes called “postulates” by Dewey) are “not arbitrary or mere linguistic conventions”, since they “must be such as control the determination and arrangement of subject-matter with respect to achieving enduringly stable beliefs” (LW 12:25). They are also not “externally *a priori*” in the sense that “they are not given and imposed from without” (ibid.), but rather discovered and developed in the course of inquiry.

Insofar as Dewey has captured widely applicable lessons from past inquiries, then his i“nquiry into inquiry” provides enough normative force both to make positive recommendations to inquirers, and also to provide critical resources for the analysis of inquiries. Of course, the final test of the validity of the principles is “determined by the coherency of the consequences produced by the habits they articulate” (LW 12:20), that is, the general efficacy or fitness of the principles in producing conclusions that stand up and can be fruitfully developed by further inquiry.

## 2.8 The Pattern of Inquiry

One of Dewey's most important and, from the contemporary scene in philosophy of science, most controversial claims is that all inquiry has a pattern in common:

It was held throughout these chapters that inquiry, in spite of the diverse subjects to which it applies, and the consequent diversity of its special techniques, has a common structure or pattern. . . (LW 12:105)

Dewey falls somewhat short of saying that all science or all inquiry shares a common *method*, and the distinction between a method and a pattern must lie in a degree of specificity or determinateness. In at least one traditional way of thinking about a method, a method determines with a high degree of specificity a set of steps to be completed, a procedure to be followed, in order to produce the desired result. For Dewey, specifying a pattern means specifying a set of *functional* relationships *to be satisfied*, though not an algorithm or set of temporally arranged steps for doing so. Both tell an inquirer what they *ought* to do to produce a satisfactory result, but a method provides a much more restrictive set of instructions, whereas Dewey provides a more general set of guidelines. The restrictiveness of this way of thinking about “method” proves *too* idealized to capture the fruitful ways of engaging in inquiry, the degree of creativity and slipperiness involved.<sup>15</sup>

There are two major categories of materials and processes in inquiry: *existential*, those that refer to the fixed conditions of the situation, and *conceptual* or *ideational*, those that refer to possibilities for *action* in the situation. The collection of facts or data through processes of observation and the statement of a problem are the main existential activities. The suggestion of hypotheses, theorizing, modeling, and reasoning about alternatives are the main conceptual activities. The two are

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<sup>15</sup>Certain conceptions of “method” bring it much closer to Dewey’s sense of “pattern.” I suspect that many of the arguments that took place for and against method involved a lack of clarity on just this point, about how restrictive a method might be.



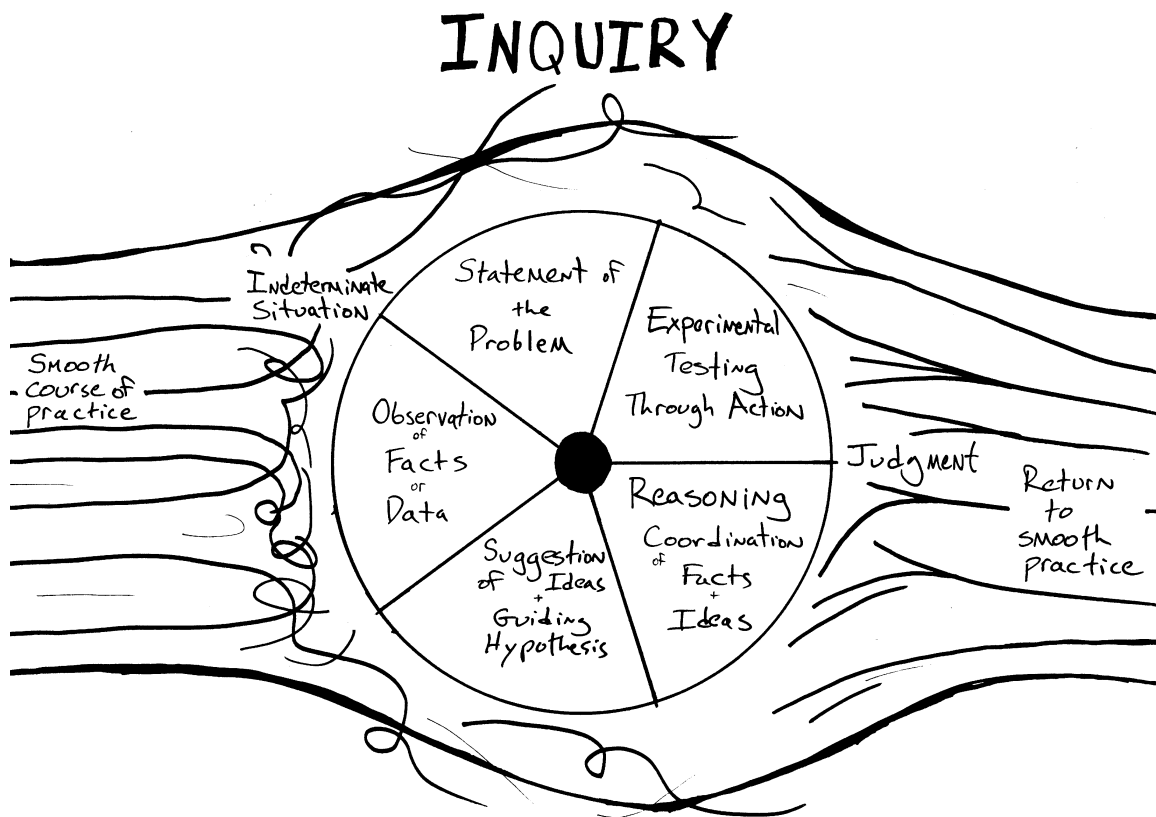


Figure 2.1: The Pattern of Inquiry.<sup>16</sup>

brought together in experimental testing of a hypothetical problem-solution, and in the final judgment that closes an inquiry. (See Figure 2.1.)

### 2.8.1 Facts and problems

Of the functions and factors of inquiry, *instituting the problem* is probably the most important. For Dewey, all inquiry is centered around a problem and its solution. As we've seen, inquiry doesn't happen if there is no problem, if there is nothing wrong in the situation that provokes inquiry. In formulating a coherent,

antiskeptical fallibilism, Dewey sided with Peirce in thinking that one could not doubt settled convictions or conceptions without a positive reason to do so. If no doubt arises, if we are settled and certain, then inquiry cannot arise, and beliefs will not be changed.

We do not begin inquiry with an already set problem, but with some implicit or poorly expressed perplexity. To emphasize that the location of the perplexity is not simply in the mind of the inquirer, but in the whole situation, Dewey often calls the perplexity a “problematic situation.” A *problem*, or a problem-statement, is an explicit formulation of the source of the perplexity, i.e., it states what the difficulty is and which factors contribute to it. It is hard work to get the problem right: “A problem well-put is half-solved” (LW 12: 112). And in fact, we should say also that a problem perfectly-well-put is entirely-solved. That is, we can never quite set a statement of the problem in stone until we’ve found its solution, since it could always be that to find the solution we might need to reformulate the problem.

The foremost function of facts in inquiry is determining what the problem is, though that is not their only role. It seems to some present-day pragmatists, and philosophers of science influenced by the debates about theory-ladenness and related worries, that talk of “facts” in any serious way doesn’t belong in philosophy of science. In Dewey’s scheme, facts play a very serious role, though his concept what a fact is for Dewey departs significantly from the traditional one. For Dewey, facts are *context-dependent*. Something is a fact if it plays a certain role or performs a certain set of functions in an inquiry; in other circumstances, it might not be taken as a fact at all.

Facts are constructed, for Dewey. They are not “out there”, not some metaphysical object or state which propositions correspond to, but they are a type of proposition. Facts could also be called “data,” and sometimes Dewey uses this term as well. Facts are not *given* in experience, rather they are *taken*, which is to say that they are ways of *taking* experience to be such-and-such a way. They are attempts to

make certain features of experience explicit in a pellucid way, and as such are subject to selective interest and interpretation. Not only do facts make explicit what is already there, but in many cases in science, we are concerned to *construct* phenomena that exist nowhere in “nature,” such as electric circuits, digital computers, or even balls rolling down near-frictionless inclined planes.<sup>17</sup>

If we focus carefully on the functional role of facts within an inquiry, however, Dewey’s account looks somewhat more familiar. Facts capture the fixed conditions with which inquiry must cope. They provide the resources for locating and formulating the problem of inquiry. The facts also suggests certain hypotheses for solving the problem (e.g., determining that we have an oil fire rather than some other sort suggests a different method of solution be tried). Likewise, once a hypothesis has been suggested and elaborated, further examination and determination of the facts can help test the hypothesis and suggest acceptance, rejection, or further refinement.

Since transformation of a problematic situation (a confused situation whose constituents conflict with one another) is effected by interaction of specially discriminated existential conditions, facts have to be determined in their dual function as obstacles and as resources. . . No existing situation can be modified without counteracting obstructive and deflecting forces that render a given situation confused and conflicting. . . Nor can an objectively unified situation be instituted except as the *positive* factors of existing conditions are released and ordered so as to move in the direction of the objective consequence desired. (*Logic*, LW 12: 493)

Facts are not the *ultimate* test of a hypothesis, however. There is no foundational relationship here, and facts and hypotheses are symmetrical with regard to their revisability. Instead, facts and ideas co-evolve, sometimes facts suggesting new ideas or revisions, sometimes reasoning through ideas suggests further operations of observation, re-statement of the facts, or even rejection of some data as spurious. The ultimate test of both is the transformation of the situation to resolve its

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<sup>17</sup>Dewey’s description of facts is not unlike a generalization of Cartwright’s *nomological machines* in this way. See Cartwright (1999, Chapter 3).

problematicity.

Another important point to make is that facts for Dewey are not always singular or particular matters. Nor are there anything like “atomic” or “basic” facts that transcend particular inquiries. Even high-level models of data or inclusion-exclusion relationships of high-order kinds can count as facts. In both cases, what the facts express are about fixed conditions which inquiry must deal with: that the data show a certain general trend, or that whales are mammals rather than fish, provide constraints that must be dealt with. Of course, what we can go on to infer from any set of facts is another matter: while facts capture fixed conditions, hypotheses capture potentialities or possibilities of future development of the situation. They tell us what we can expect, and how we can act in order to direct the situation towards a better issue.

This leads us to the last key point, Dewey’s subtle take on the theory-observation distinction. Clearly, the examples of curve-fitting and the whale-mammal-fish relationships bring up concerns about what has usually been called theory-ladenness. Dewey would agree that these kinds of facts, and indeed any statements of facts whatsoever, rely on categories and concepts developed by inquiry and involved in theories. It has become a commonplace in some quarters<sup>18</sup> that facts and theories must “speak the same language” in order for them to be relatable to each other. Nevertheless, though facts depend on what we might call “theoretical concepts,” there is nevertheless a clear and important distinction between facts and theories, namely that they occupy distinct and opposite functional roles within an inquiry. Facts are responsible to actual conditions, and theories are responsible to possibilities. And ultimately, both are responsible to something that is not at all theory-laden, the pre-cognitive felt perplexity that must be resolved, and the situation that must be transformed.

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<sup>18</sup> For example, so-called Kantian epistemologists, probably Kant himself, and those concerned with conceptual content all recognize this as a problem/feature of reason.

## 2.8.2 Ideas and solutions

As already mentioned, coordinate with the role of facts in inquiry is the role of hypotheses or ideas (which Dewey also sometimes called “concepts,” “conceptual contents,” “meanings,” and “theories”). In other words, “idea” is Dewey’s generic term for whatever conceptual-theoretical materials play a role in inquiry. Ideas are proposals or suggestions for solving a problem, as when we might say, “I have an idea!” when a solution occurs to us. Whereas facts capture the observed conditions of the situation, ideas capture the future possibilities inherent in it, both in terms of what we can expect to happen and what operations we might perform to affect the course of events. Facts are what is present, where as ideas indicate what is possible. Ideas are originally suggested (as possibly relevant or applicable) by the determination of facts and the formulation of the problem. They are then worked out through processes of reasoning, checked against further facts, tried out in experimental applications, and ultimately evaluated on the basis of their “functional fitness” (*Logic*, LW 12:114) in bringing the problematic situation to resolution.

While it might be easy to see how some set of practical hypotheses might work in the way Dewey suggests, it is difficult to see how a scientific theory can be understood as a solution to a problem, or how its content essentially involves operational interventions and their consequences.<sup>19</sup> One thing that theories do is express connections between events or things. To use some outmoded examples, if all ravens are black, then I expect that if I have found a raven, I will also find it to be black, and if it is the case that if there are low, dark clouds there will likely be rain, then I can expect that if I see such clouds, I can expect it to rain. These show the possibilities or connections inherent in present situations, and these connections can be put to operational use in solving the particular problems at hand. Further,

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<sup>19</sup> Though, see Pearl (2000) and other manipulability or interventionist accounts of causation for reason to think that at least all (causal) laws of nature have to do with interventions and means-consequences relations.

theories of a higher level of complexity and abstractness than these old philosophers' toy examples can be seen as more general mechanisms of generating more specific suggestions. That is, while a hypothesis like, "If you have a headache, you should take aspirin," if it is fairly adequate, will solve a wide variety of problems, a comprehensive medical knowledge of many features of the human body can generate an even greater variety of solutions.<sup>20</sup> If we regard the generation of these more specific ideas as the main function of theories, then we can see how even high-level theoretical systems can fit the general role of working towards the solution of the problem.

### 2.8.3 The Coordinate Development of Facts and Ideas

It is crucial to Dewey's theory of inquiry, and it is a great innovation on prior accounts, that facts and ideas *develop* in inquiry in coordination with one another. The key to successful inquiry is getting the right sort of fit between the facts, or the terms of the problem, and ideas, or problem-solutions. In summarizing his view, Dewey brings this element to the forefront:

Inquiry is the directed or controlled transformation of an indeterminate situation into a determinately unified one. The transition is achieved by means of operations of *two kinds* which are in functional correspondence with each other. One kind of operations deals with ideational or conceptual subject-matter... The other kind of operation is made up of activities involving the techniques and organs of observation. (LW 12:121, emphasis mine)

Dewey thought that the enduring insight of Kant was his recognition of the need for perceptions and conceptions to work together. Kant made an error, according

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<sup>20</sup> Though we should be cautious: greater unity or comprehensiveness is not always a virtue; it can often trade off against more specific and disconnected but yet more effective knowledge (a point made repeatedly by Feyerabend in many contexts; see e.g. "Notes on Relativism" (Feyerabend, 1988)). Further, it might well be the case that an overlapping set of incompatible, comprehensive theories of the human body might also be more effective, despite the difficulties in adjudicating contradictory hypotheses generated from them.

to Dewey, in thinking that perceptions and conceptions “originate from different sources” and require an activity of “synthetic understanding” to bring them together. But they need not have a third activity to bring together, because determination of fact and development of ideas work in concert, controlled by and with an eye towards the other. As Dewey says,

In logical fact, perceptual and conceptual materials are instituted in functional correlativity with each other, in such a manner that the former locates and describes the problem while the latter represents the possible method of solution. (LW 12:115)

So it is not clear that we can conceive of an activity of perception or of concept-formation apart from the other, since each is defined by a functional relationship with the other. Furthermore, both come from the same, not different, sources.

Both are determinations in and by inquiry of the original problematic situation whose pervasive quality controls their institution and their contents. Both are finally checked by their capacity to work together to introduce a resolved unified situation. (LW 12: 115)

Finally, it is crucial to note that, “As distinctions they represent logical divisions of labor”(LW 12: 115); that is, the distinction between percepts and concepts, facts and ideas, is a functional one. No proposition is a fact or a theory in-itself, for all time, in all inquiries. The distinction between concepts and percepts is not, in other words, ontological, but logical (functional). Whether something is counted a fact depends on what role it plays in the inquiry at hand. Ideas and facts exist *as* ideas and facts relative to each other, and to the problems and purposes of the inquiries in which they play a role.

It is crucial to understanding the correlative role of facts and ideas that, as Dewey says, “it is recognized that both observed facts and entertained ideas are operational”(LW 12:116). It would otherwise seem mysterious, according to Dewey, since facts, which locate and describe the problem, are “existential,” that is, they deal with

the fixed conditions of the present situation as revealed by activities of observation, while ideas, referring to potentiality and possible solutions, are “non-existential” (not referring to features of the actual situation). That ideas are operational means that they “instigate and direct further operations of observation”(LW 12:116) and they provide proposed plans for action that will “bring new facts to light and to organize all the selected facts into a coherent whole”(ibid) in a way that leads to a resolution of the problem at hand.

That facts are operational is less clear. In part, it simply indicates that there are no ultimate, “inquiry-independent” facts, but rather that what are selected as facts are relative to purposes and problems. In particular, whether some putative fact is continued to be treated as so in inquiry depends entirely on whether it fulfills its function to both suggest relevant solutions to the problem and to test those solutions’ adequacy.

It is important to remember that neither facts nor ideas are absolutely given. Some facts may be settled prior to the inquiry at hand. The facts are taken, not given, as conditions for the inquiry, and the factual conditions are revisable within the inquiry in light of new information; the facts are a matter of “selective emphasis” for the purpose of resolving a particular problem (LW 1:31-2). Even when the facts stand as unquestioned conditions or as evidence, this should be understood to have logical, not metaphysical import; it is only a matter of their functional role in inquiry. Ideas are suggested by the determined factual conditions and by the creative efforts of the inquirers; they begin life as suggestions that simply spring up or occur to us in a flash (LW 12: 113–4). The two work together to move inquiry towards resolution.

Though Dewey never uses the term, we could easily describe the relationship of facts and ideas in an inquiry as “dialectical.” The two develop in constant dialogue with each other. Any significant development in the determination of facts immediately either prompts new ideas or development of the working hypotheses already at hand. Any development of a working hypothesis suggests new observations and



tests. But it would be misleading to call the two elements dialectical in the sense of two separate *sources* or *faculties* in dialogue. As Tom Burke says, in his introduction to the critical edition of Dewey's *Essays in Experimental Logic*:<sup>21</sup>

We have to make distinctions... but... We must not posit two different but mysteriously linked faculties where sensation, apprehension, perception, observation, and experimentation are essentially separate from thought, understanding, judgment, cognition, comprehension, calculation, and computation. Placing experience over and against reason in this way is a fundamental flaw in modern epistemology and contemporary cognitive science. It only gives rise to interminable debates concerning various priorities and dependencies among the elements and operations of these two separate faculties. (*EXL*, xviii)

On Dewey's view, by contrast, facts and ideas, perception and reason, are both types of experience, or activities that occur within the course of experience. They do not have separate sources or consist of separate realms of existence; they are distinctions drawn within the course of reason. There is no reason on this view to regard their interaction as mysterious or problematic. This is the core of Dewey's critique of epistemology: once one adopts a functional logic, those problems that have so long exercised modern epistemology, which arise due to recurrent and pernicious dualisms of mind/body, internal/external, etc., dissolve.

It is also interesting that this sort of pragmatism makes nonsense of empiricist nominalism, since particular matters of fact depend on theoretical generalizations as much as generalizations depend on particulars. It is only due to a confusion of what Burke describes as "the status of being logically primitive or elementary" with being either psychologically or metaphysically primitive that doctrines such as nominalism, sense-data empiricism, or logical atomism make sense.

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<sup>21</sup> Referred to hereafter as *EXL*.

### 2.8.4 Reasoning and Conceptual Frameworks

Reasoning is a crucial feature of inquiry for Dewey; without it, warranted judgment and thus knowledge is impossible:

When a suggested meaning is immediately accepted, inquiry is cut short. Hence the conclusion reached is not grounded, even if it happens to be correct. The check upon immediate acceptance is the examination of the meaning as a meaning. (LW 12: 115)

In other words, a grounded conclusion must be checked by *reasoning*, which Dewey characterizes in the following way:

[Reasoning] consists in noting what the meaning in question implies in relation to other meanings in the system of which it is a member. . . . If such and such a relation of meanings is accepted, then we are committed to such and such other relation of meanings because of their membership in the same system. Through a series of intermediate meanings, a meaning is finally reached which is more clearly *relevant* to the problem in hand than the originally suggested idea. (LW 12:115)

These remarks on the nature of reasoning clearly implies the existence of a conceptual framework and seems to imply a form of conceptual holism. So a suggested hypothesis is reasoned through by tracing the conceptual structures to which it is related. For example, suppose that that a physicist, when studying some electromagnetic repulsion phenomenon, encounters an anomalous result. Suppose furthermore, that she hypothesizes that there is a gravitational effect at work. Our physicist will then reason through the variety of connections between the electromagnetic relations she thought would be at work, the theory of gravitation, and what connections gravitational and electromagnetic phenomena may have (including, perhaps, the high-level theoretical principles of Newton's laws and the rules about balancing forces, or yet more abstruse connections in more modern physics). Through this process, then, the hypothesis is refined into one "which is more clearly relevant to the problem in hand" because

It indicates operations which can be performed to test its applicability, whereas the original idea is usually too vague to determine crucial operations. In other words, the idea or meaning when developed in discourse directs the activities which, when executed, provide needed evidential material. (LW 12:115)

In other words, it is developed,

until it receives a form in which it can instigate and direct an experiment that will disclose precisely those conditions which have the maximum possible force in determining whether the hypothesis should be accepted or rejected. Or... indicate what modifications are required in the hypothesis... (LW 12:115–6)

It is important to point out that Dewey's innovation here is not in having anything strikingly original to say about the procedures of reasoning, which ought to be fairly familiar, but rather about the functional role of processes of reasoning in advancing inquiry.

### **2.8.5 The Necessity of Experiments**

Solving any problem requires operations of making and doing. Ideas cannot be tested by a purely passive collection of facts through observation. Active interventions of an experimental nature provide necessary evidence about the prospective effectiveness of solutions. Even an entirely conceptual subject-matter (like mathematics) requires that new ideas be “tried out,” that attempts be made to see how things would be different if new ideas and theories were put into operation. Even a scientific pursuit that seems completely passive and observational, such as stellar astronomy, depends on laboratory work on things like spectral emissions in physical chemistry in order to progress.

If the problems of inquiry arose merely from personal states of doubt, then the manipulation of purely personal states of mind would (in principle) be sufficient

for solving problems, and thus sufficient for inquiry. Experimentation would then be a merely accidental feature of science, one possible method for arriving at new observations, themselves only useful as means to forming and justifying further beliefs of a more theoretical nature. Indeed, it would seem that traditional epistemology and philosophy of science have often been committed to just such a view.

From the point of view of the history and practice of science, this seems totally naive. It is surely the case that the experimental method, which involves not only observation but active intervention, is one of most crucial innovations of science, as close as most anything comes to being an essential differentia of modern science. And we would expect nothing less, from Dewey's point of view, for resolving a problematic situation requires putting a solution into practice.

Quine is wrong:<sup>22</sup> science is not about systematizing the stimuli that excite our sensory organs. Sensory stimulations are not the data of science. Facts are, and their content deals with existential constituents of the world, and, except for the most mundane of facts, those can't be determined without intervening in the world. Systematization is also not the goal of science. The resolution of problem is. Problem-solutions involve changes in our ways of acting in the world, and those can only be properly tested by means of controlled interventions. This pragmatic standpoint that recognizes science as an activity of live, embodied creatures trying to cope with an environment, that the significance of scientific questions come from disruptions to that activity, is able to faithfully capture the significance of experimental methods, whereas more traditional, more empiricist theories of science are not.

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<sup>22</sup> By the way, all the same arguments apply to the Humeanism of David Lewis, just as well, swapping out "sensory stimulations" for "the mosaic of spatio-temporally distributed intrinsic qualities." (Lewis, 1986)

### 2.8.6 Final Judgment and the Close of Inquiry

Inquiry ends in judgment. If the operations of inquiry have run their course to the fullest, if the ideas suggested by the facts of the situation have been reasoned through carefully, if they have been used to gather new facts relevant to their proposed problem-solutions, and if they have been thoroughly tested by experimental operations, then the final judgment with which the inquiry closes can be called a “warranted assertion” and has the property called “warranted assertability.” This notion is inquiry-relative; what is warranted on the basis of inquiry in a particular situation may no longer have that status some time down the road, in a different situation. But so long as inquiry is done well and the judgment actually succeeds in resolving the problematic situation from which the inquiry began, then it is fair to call the judgment warranted.<sup>23</sup>

An interesting feature of judgment that Dewey emphasizes again and again, and which will receive more attention in the conclusion, is that judgment is an active affair that *changes* the situation. It has what Dewey calls “direct existential import.” An inquiry establishes new *objects of knowledge*.<sup>24</sup> That doesn’t mean that it creates existences out of thin air; rather, it means that it adds new meanings and connections to the qualities, events, things of our experience, and that these will be taken up in our activities in different ways as a result. We pick things out and use them differently, exercising selective emphasis. Thus, inquiry which uncovers iron as a new object, or alters the object by uncovering important new properties or potentialities in it, changes not only how we see, think of, and value the parts of the world that we now pick out as “iron” (though it does do that). It also alters the face of mountains, the ore that we extract from them, and creates a whole new set

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<sup>23</sup>Ultimately, though, warranted assertability looks forwards, to the future, and whether the results can be asserted in the future. In the continuum of inquiry, forms and techniques of inquiry are explicitly formulated to allow judgments to have relevance and staying power beyond the immediate situation they develop in response to.

<sup>24</sup>The meaning of this idea will be more fully explored in the conclusion of this work.

of industrial practices and practical use-activities in which this quite revolutionary material plays a role.

Judgment also alters the background conditions of a situation. In order for us to establish reliable patterns that we may use, we must to large extent control the situation so that only the relevant factors are available. Not only must agriculture learn how to encourage the growth of plants, it must learn to eliminate factors that interfere, such as competing plants, parasites, and animals that would eat those plants. Chemistry and materials science must refine materials so that they fit neatly into the categories that we know how to deal with; not any rock or piece of earth will do. Using principles from physics in application relies just as much on eliminating factors that physics doesn't know how to deal with as it does applying the laws and principles of its theories to specific situations. Part of the tenacious difficulty that plagues quantum computing, and will keep it from having any practical payoff for years to come, is the difficulty in regimenting the environment around the qubits so that the rather strict requirements for applying quantum mechanics obtain, and doing so for a sufficiently long time with sufficiently many qubits such that useful computations might be performed.

## 2.9 The role of values

In the early twentieth century, the ideal of value-free science was advocated by two groups: logical empiricists and others who had been negatively impacted by the effects of ideology on science and philosophy in the 1930's and '40's, and religious philosophers like the neo-Thomists who wanted to deny the relevance of science for philosophical (especially moral) matters and thus make room for more traditional ways of life in the face of modern science. Both of these groups were, as we've seen in the previous chapter, opposed by Dewey and his followers. On the one hand, the dichotomy between science and values and the reductionism about values that Dewey

perceived in the work of Carnap and others echoed similar views in Russell, which he regarded as a product of a kind of aristocratic aloofness that was bad for both philosophy and science. More importantly, he saw it as a major tactical error for the agenda that he shared with the logical empiricists: if the supporters of science claimed that it was value-free, then values would end up in the realm of the anti-scientists, and scientific philosophy and the scientific image of humanity would be lost. He saw neo-Thomism as an extreme anti-naturalism, trying to hold on to the vestiges of beliefs that had been outmoded since Darwin.

The fight against the ideal of value-free science never really stopped, though it is certainly much more prevalent today than it was just after Dewey's death, and perhaps more than ever. However, until recently, the major defenders of the role of values in science have shared a premise with the defenders of value-freedom that Dewey would have rejected. Both sides have generally taken values to be more or less *fixed* or *given*, fixed points with which science must begin (or avoid). The late logical empiricists and their descendants regarded values as being bound up with ideology at worst, and thus pernicious political forces, or with personal preference at best, and thus pernicious subjectivism. Similarly, feminist philosophers of science have until recently regarded values as part of the "background assumptions" that influence choices otherwise underdetermined by evidence, and so sexist versus feminist background values might make the science turn out very differently. But these values are always something brought in from the outside.

Dewey thoroughly rejected both of these views about the relation of science and values, and the theory of values implicit in it. For Dewey, it is clear that inquiry is a practice that is "socially conditioned" (LW 12:27), and thus that cultural interests and values will shape the problems inquiry addresses and the standards for solution. In part, this is because inquiry must begin with the symbolic resources and practices of the current culture, and because those symbolic resources and cultural practices are themselves normatively-laden. But inquiry also has "cultural consequences." Ac-

cording to Dewey, “every inquiry grows out of a background of culture and takes effect in greater or less modification of the conditions out of which it arises” (LW 12:27). On reflection, this obvious fact—that solving problems and gaining knowledge must modify the conditions of the culture in which it takes place—makes nonsense out of the idea that values are imposed on inquiry from outside. Because inquiry leads to action, because it changes habits and culture, because it reshapes practice, the idea that values are fixed in the social world prior to inquiry and unchanged in the course of inquiry is a non-starter.<sup>25</sup>

According to Dewey, logical theorists have completely ignored a *prima facie* distinct species of judgment, judgments of practice. Such judgments are not, according to Dewey, reducible to mere descriptive judgments. They have a distinctive logic.<sup>26</sup> Judgments of practice are crucially judgments about *what we shall do*; they are necessary when the course of action is not obvious, but confused or indeterminate. Judgments of practice crucially involve *evaluation*, of both means and ends, of possible courses of action and their consequences. Judgments of practice are future-oriented; though they depend on an adequate evaluation of the present circumstances, they are judged entirely on the basis of their consequences. To wit, their verification is identical to their truth. If the new course of action pays off, if it leads to the return of unproblematic action, the judgment of what to do was correct.

Suppose, to use an example of Dewey’s, that I need to buy a new suit, because mine are all worn out, perhaps, or I don’t have one that is nice enough for a certain occasion. If the choice is absolutely clear, then there is no need for judgment; I will

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<sup>25</sup>This argument assumes that we’re talking about actual personal or socio-cultural values, rather than (say) absolute moral values. I have doubts about whether such values exist, but that is a matter for meta-ethics. Suffice it to say that until the ethicists deliver on this possibility, what enters into actual inquiries that must be accounted for are actual human values.

<sup>26</sup> See *Essays in Experimental Logic* Ch 14, “The Logic of Judgments of Practice”; *Logic: the Theory of Inquiry* Ch 9, “Judgments of Practice: Evaluation”; Jennifer Welchman, “Logic and Judgments of Practice,” *Dewey’s Logical Theory: New Studies and Interpretations*. The following discussion owes much to Welchman’s insightful discussion, though attempts to go further in the spelling out the implications for philosophy of science.



be carried on by some unreflective habit. Suppose this is not the case. The very nice, stylish, and durable suits are all too expensive. I may have to trade off style versus durability, or I may have to choose a less formal or fancy suit. I am pulled in different directions; apparent values conflict. I must reevaluate the options with an aim to *integrate* the competing values. Values accrue to objects or features in the course of inquiry and reflection as means towards coming to a decision of what to do.<sup>27</sup> I think about the consequences of buying a cheap suit that will fall apart in a year, or how this or that style will reflect on me. I think about whether buying on credit is an acceptable means of payment, and how that affects my price-limit. And so on. In the end, I evaluate the different suits and I make a judgment as to my course of action, and purchase some particular suit. If the financial impact is not in the end too dire, and the suit wears well, takes me smoothly through formal occasions, and gets a few compliments and no derisive remarks, then I will say that I made the right decision. If things go less well, I may say that I judged falsely.

It may already be clear to you, though, that Dewey does not, in the end, regard judgments of practice as a distinct kind of judgment, but rather as part of, or even the, fundamental kind of judgment.

The net conclusion is that evaluations as judgments of practice are not a particular kind of judgment in the sense that they can be put over against other kinds, but are an inherent phase of judgment itself. (LW 12:180)

There are, of course, *relatively* more or less practical judgments, where evaluation plays a major or minor, explicit or implicit role. But all descriptive and scientific judgments, in what Dewey would call the “eulogistic” sense of judgment, require evaluations, where evaluations are understood not as the application of some antecedent values but the reflective *judgment* that something has some value. It is not just a prizing, but an appraisal.

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<sup>27</sup>Strictly speaking, evaluation like all inquiry requires that it be made public, rather than purely personal.

His non-reductionist picture of the relationship between evaluative and descriptive judgments recognizes deep interconnections between the two kinds of judgments. Practical judgments depend crucially upon descriptive information. I need to become aware of the causal antecedents and consequents of actions and events, whether certain actions are possible, which reactive attitudes (pleasure, satisfaction, pain, annoyance, etc.) I have towards various things. But on the other hand, I can never *directly infer* practical judgments from any quantity of descriptive fact. While Dewey would not be inclined towards full reductionism here, since the functional distinction between “descriptive” and “evaluative” judgment is quite useful in many contexts, talk about “pure” descriptive or theoretical reasoning is so misleading that he would ultimately have us drop it.

It is because all inquiry has to do with transforming a problematic, indeterminate situation into a unified one, which requires an active modification of one’s circumstances, that all judgment is (in part) a judgment of practice. This is in part because “the primary object of our attempts to understand the world is not to describe it but to *manage* it” (Welchman, 39). A problematic situation is, as we have said, a disruption of some practice that matters; resolving it requires a judgment about how to reconstitute the practice. As a practice, science has a specific normative structure governed by values that help scientists determine “how they ought to pursue their inquiries, what they may count as evidence, and what they are entitled to believe in specific situations” (Welchman, p. 28). The selection of data is an active and evaluative enterprise. Inquirers must decide which instruments and techniques to use, which operations of observation to perform, which data to select as relevant, and which tolerances to set and errors to control. All inquiry requires experiments, which are at base operations of making and doing. There is no phase of inquiry in which values play no role. But these values don’t remain fixed. In many parts of inquiry, explicit judgments of value, or evaluations, are required. And indeed, the inability to make good judgments about value would severely impair the ability to

do science well. Values in scientific inquiry are not fixed and ultimate ends, but ends-in-view that are held tentatively or “experimentally.”

Clearly, the values we begin with influence to a large degree which inquiries we pursue, as well as their outcomes. Feminist critics have made the point quite compellingly with regards to sexist values in certain areas of research. Dewey was especially bothered by the way the values of laissez-faire capitalism and corporate interest, as well as nationalistic interests in war-making, had influenced the recent course of scientific inquiry. We might point as well at the influence of the interests of wealthy nations in guiding biotechnological research, as Kitcher does. But Dewey believed, in the spirit of the Enlightenment though against many of his and our contemporaries, that science had the power to transform values. Indeed, within its limited sphere, every judgment is a transvaluation of prior values.<sup>28</sup> But Dewey also believed in the ability of science to influence values more generally.

Science cannot, however, deliver values from on high. Science does not discover values amongst the furniture of the world, as it might discover electricity, nuclear fission, or species. Nevertheless, in the course of science certain values are tried out. Deference to Biblical authority, for example, has played out poorly as a value, whereas democratic deliberation between different voices and gathering of evidence via interaction with nature have paid off well. And science has also developed a peculiar spirit, one that might serve as a model for deliberation about values. If we were to bring into the public sphere to some degree the spirit of fallibility, the ability to withhold judgment in order to reason and gather evidence, the requirement to appeal to evidence rather than belief or passion, and to the experimental method, the haphazard movement of policy and social values might improve significantly. Having paid off well in the successful sciences, we might try to bring these lessons

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<sup>28</sup>Dewey remarks in characteristic wit about Nietzsche that, “Nietzsche would probably not have made so much of a sensation, but he would have been within the limits of wisdom, if he had confined himself to the assertion that all judgment, in the degree in which it is critically intelligent, is a transvaluation of prior values” (“The Logic of Judgments of Practice”, EEL 196).

over *mutatis mutandis* to social problems.

## 2.10 Summary

Dewey's philosophy of science analyzes science in an anti-skeptical, fallibilist, anti-foundationalist fashion, argues that scientific inquiry is situational or contextual, aimed at solving problems that arise in those situations, that it is inherently social, and that it is value-laden. His main contributions include an analysis of the genesis and importance of scientific problems, a novel theory of facts and hypotheses that is neither foundationalist nor simply coherentist, his defense of anti-reductionist contextualism, and his integration of science with social factors and values. On Dewey's view, science is the practice through which we create intellectual tools for coping with a precarious and problematic world.

## Chapter 3

### Inquiry and Evidence

### From the experimenter's regress to evidence-based policy

Failure to institute a logic based inclusively and exclusively upon the operations of inquiry has enormous cultural consequences.

- John Dewey<sup>1</sup>

#### 3.1 Introduction

Several problems in the contemporary discussions of evidence—the experimenter's regress, concerns about discordant evidence, worries about the importance of “robust” evidence from different types of sources, and questions about “evidence for use” as distinct from evidence for theories or hypotheses—arise because the parties to these discussions ignore the temporal dynamics of inquiry, or the various functional roles of evidence within that dynamic process, or both. While inattention

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<sup>1</sup> *Logic*, (LW 12: 527)

to these matters seems almost endemic amongst philosophers, the problem has also come to infect certain of the social and medical sciences as well as policy-making. Scientists and policy-makers now find themselves in quandaries about how to rate evidence and how to combine it from multiple sources. Setting a framework of “evidence-based policy” is one of the latest practical problem about evidence to arise at the interface of science and policy.

While I will insist that many problems as currently understood are pseudo-problems better dissolved than resolved, they nonetheless are serious matters of concern in the sense that they are blocking the road of inquiry. They are also serious in that they are related to underlying problematic situations in scientific methodology, but the terms in which such problems are now raised, and the usual solutions to them, are attempts to find an easy certainty where only hard and fallible scientific research will do.

Put differently, what appear to be serious *philosophical* and *scientific* problems of evidence itself are rather problems with our current *models* of evidence. Rather than solve these problems, I suggest we *give up* our problematic model with its associated normative confusions and descriptive distortions, and replace it with an alternative model that is more adequate on all counts. The main purpose of this chapter, then, is to show how a model which emphasizes the temporal dynamics of inquiry and the functional roles of evidence within scientific problem-solving practices dissolves several apparent problems of evidence in the contemporary literature, and to argue that the considerations that can help provide a better resolution of a problem like the experimenter’s regress, which does not obviously connect to values or policy, can lead naturally to better ways of dealing with evidence-for-use and evidence-based policy. From the point of view of this model, some of what looked problematic can be seen as a necessary or even mundane feature of scientific research, while other problems that seemed to call out for logical or epistemological solutions actually require only the continued carrying out of the often hard and

time-consuming processes of inquiry.

I will begin with a brief discussion of what I take to be the main features of the problematic, non-dynamical model of evidence and inquiry (the “ $E \Rightarrow H$  model”). Then I will set out the main feature of the alternative model of evidence, based on the pragmatist model of inquiry developed in chapter 2. I will provide an illuminating example and then show the major lessons of the inquiry-model for our philosophical treatment of evidence. In the following section, I will address several key problems of evidence in the recent literature: theory-ladenness, the experimenter’s regress, discordance, robustness, and evidence for use. I will show just how impossible a situation the current model of evidence puts us in, while on the other hand showing how the inquiry-model dissolves or quite easily solves these problems. In the final section, I will address what I take to be a serious problem of evidence even on the inquiry-model: forming a sound framework evidence-based policy.

## 3.2 Evidence and the Pattern of Inquiry

### 3.2.1 The $E \Rightarrow H$ (Non-Dynamical) Model

I will here briefly try to describe the main features of the problematic but common model of evidence influencing the current discussion. The model is non-dynamical in the sense that it doesn’t depend on any important or interesting way on the temporal complexity of inquiry. I call it the “ $E \Rightarrow H$  model” because it defines evidence according to a single function, the “support” relation it has to hypotheses, theories, claims, etc. Positivist and Popperian models from the middle of the twentieth century are clear specifications of this model, as are some Bayesian discussions of evidence.

In all these cases, “support” is an abstract relation that some set of evidence (beliefs, propositions, measurement records, etc.) hold to some further hypothesis or

claim, whether the nature of that relation be logical, statistical, or formal in some other sense. Given a set of evidence and some hypothesis, we should be able to identify whether that set supports the hypothesis, and perhaps how much (at least well enough to rank-order hypotheses on the basis of the evidence). Further, we can always ask *at a time* what the evidence supports, and there is always a determinate fact of the matter (though we may not know what the answer is). Evidence is that which justifies, and at a fundamental level it must be more certain, more justified, more secure than that which it justifies. That is, support is a one-way relation from evidence to hypothesis. Usually, evidence must also be independent of that which it justifies, lest the justification be illegitimate because circular.

While this may appear to be a caricature to some, and many people would explicitly deny one of more of the ideas contained in this model, I nevertheless believe—and will show in several cases—that in its basic outlines, this model captures the basic background framework for most contemporary discussions of evidence, and I will show how these ideas structure some key discussions of problems of evidence from the last several decades. Part of the problem is a lack of recognition of the *existence* of a theory or model at work in philosophical discussions at all. It is quite easy to default to an ingrained model when one isn't aware of the existence of the model in the first place. Such models are the source of our claims about what is “obvious” or “almost true by definition” about evidence, but they are nonetheless optional.<sup>2</sup>

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<sup>2</sup> This is one of the great contributions to philosophy of Dewey and Richard Rorty, to show that philosophy, like science, gets at the world through sophisticated but optional and replaceable *theories* or *models*, and that often what we need is not to answer certain questions or solve certain problems but to replace the theory in which that question or problem is stated. An importantly related idea is that of “metaincommensurability,” i.e., incommensurability at work in discussions of philosophy of science based on different metaphysical and meta-philosophical background assumptions, as has been discussed by Oberheim and Hoyningen-Huene (1997).



### 3.2.2 Dynamical models

The temporal dynamics of inquiry have received scant attention. While it is popular nowadays to talk about science in terms of “practice,” few have explored the impact that taking the praxical side of inquiry seriously for understanding the unfolding of science in time.<sup>3</sup> I am aware of only two detailed (types of) models of the temporal dynamics of science. One is the class of models developed by Kuhn and his followers (and here I include critics of Kuhn, such as Laudan or Lakatos, who provide different but related models at a similar scale). This type of model discusses the career of large-scale theories, traditions, or research paradigms that govern entire disciplines or sub-disciplines over a large span of time. However, these models are sufficiently large-scale and long-term that they are not useful for addressing current concerns in the literature on the nature of evidence. These current questions deal not with the evolution of theories over the long run, nor the revolutionary replacement of theories or paradigms. The questions at issue are far more local, having to do with the role of evidence in single controversies within a discipline or paradigm.

Another major model of inquiry on offer is the pragmatist model introduced by C.S. Peirce and further articulated by John Dewey. This model works best at the more local level of particular scientific inquiries, though it has some applications at the larger scale. I do not take it that there are important conflicts between Kuhnian models of scientific development and the pragmatist theory of inquiry. As far as I can see, they are complementary in broad outlines, though neither hangs on the success or failure of the other. I will not attempt a full defense of the pragmatist model here,<sup>4</sup> though I will attempt to make it as plausible as possible and provide an illuminating example. Later in the chapter I will analyze various problems of the nature of evidence in light of the model, and show how they are dissolved. The

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<sup>3</sup> Wayne Martin comes close in *Theories of Judgment* when he argues that the temporal complexity of *judgment* has been ignored, though in the end he has little specific to say about what this temporal complexity looks like.

<sup>4</sup> See Chapters 1 and 2, as well as Hickman (1998).

burden then falls on those who would insist on the reality of the problems to show a much more compelling model of inquiry in which the problems still arise, or to argue conclusively against the model here provided.

### 3.2.3 The Inquiry-Model

In the main outlines, the pragmatist model of the dynamics of inquiry<sup>5</sup> can be described by a number of interlocking phases:

1. *Inquiry begins with a felt perplexity.* There are many types of perplexity, but they are not in general a mere state of ignorance on the part of the inquirer. Rather, the objective state of the science—which may include theoretical frameworks and concrete models, techniques of observation and sets of data, methods of prediction and expectations of inquirers, and so on—is confused, indeterminate, or tensional. There are conflicting tendencies within the situation of the field at the present time, a major discoordination, and this requires investigation. (Contrast here the smooth application of some theory or technique to a case with immediate success.)
2. *The Institution of a Problem.* The situation must be assessed in order attempt to *formulate a problem-statement* that adequately captures the given perplexity. *Operations of observation* must take place in order to arrive at a statement of the problem, which evolves as the inquiry develops.
  - a. We engage in a type of meta-observation, a taking stock of the relevant state of the science at the time.
  - b. We engage in operations of observation of the subject-matter in question.

Notice here an important difference with one common story about science. Scientific inquiry does *not* begin with a set problem or question at which science is

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<sup>5</sup> See Chapter 2 for more thorough discussion and Figure 2.1 for an illustration of this model.

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 stated ahead of time; the statement of the problem is a phase of the inquiry itself, and it evolves as the inquiry is pursued and more adequate and sophisticated observations are made.

3. *Suggestion of Hypotheses.* The first pass at determining the factual conditions of the situation and the terms of the problem *suggest hypotheses* for solving the problem. These hypotheses may sometimes be free-standing, relatively *sui generis*. In a mature science, this is the less usual case. Hypotheses are often related to more general theoretical-conceptual schemes. There are probably many types of relationships here. What is important is that the theoretical materials from which the hypothesis originally springs must be developed in accordance with observations of the current situation, what we might call the facts of the case, so that the hypothesis generates concrete operations that can be executed in order to solve the problem.
4. *Coordination of Observation and Hypothesis.* A reciprocal process of *coordination* and improvement of observed facts and theoretical-hypothetical ideas is undertaken. There are several phases of this process which depend on each other and need not proceed linearly.
  - a. 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.
  - b. New observations are made in response to the evolving body of hypotheses and theoretical ideas, to answer questions posed by them and fill in information needed to specify the relevant features of the ideas.

- c. From the set of putative evidence (“factual propositions” as Dewey would say) 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.
  - d. The statement of the problem is refined to reflect the changing understanding of the situation and the evolving set of hypothesis.
5. *Experiment.* A series of 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.”
6. *Judgment.* The objective and final product of inquiry is a judgment. Inquiry continues until one of the hypotheses is adjudged to be the most warranted amongst the alternatives, and the alternatives have been more or less ruled out. To put it differently and more prospectively, the inquiry proceeds until a point of resolution so settled that the conclusion can be used as a reliable means to further inquiries. A judgment of warrant is a judgment about the adequacy of the hypothesis to solving the problem. Such a judgment is impossible without to some degree undergoing this process of inquiry (otherwise, it would be merely a reflexive response), and it is only a judgment in the eulogistic sense of “judgment”<sup>6</sup> if the process of inquiry is exhausted to the point that, from one perspective, no doubt remains about the hypothesis, and, from another perspective, the conflicting tendencies of the situation have been resolved and coordination has been restored (at least, for the moment). A judgment that satisfies the conditions of good inquiry and can be used as a settled means to future inquiry has the property of *warranted assertability*.

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<sup>6</sup> An ideal, of course, that most if not all actual inquiries only approximate.

This is obviously an idealized picture of the conduct of inquiry. It is no *a priori* imposition, however; both Peirce and Dewey were students of the history of science and participants in the science of their day, and this deeply informed their writings on the nature of science and inquiry. Theirs is a *normative-explanatory* model, attempting to capture, explain, and make available the lessons of successful inquiries past. The proof of this model is in its power to give us a more successful understanding of the uses of evidence and to resolve or dissolve problems of evidence that arise. First, we will look at an illuminating concrete example of inquiry, in order to show what lessons for understanding evidence this model provides.

### 3.2.4 Snow on Cholera

Consider the work of John Snow on the transmission of cholera.<sup>7</sup> The basic outlines of the problematic situation are clear: cholera is a terrible disease, fatal in nearly all cases at the time. The nineteenth century saw many epidemics of the disease, beginning in Asia and later in Europe and America. It is tempting to say that the *problem* itself is clear from the beginning: how is cholera communicated, and how can its transmission be prevented or contained? While the idea of contagious diseases was not new in the middle of the nineteenth century, when Snow was at work on cholera, it was neither fully accepted nor clearly distinguished from views identifying disease as a punishment for sin. To regard some diseases as communicable, and to identify cholera as one such, is already to be well into the inquiry. Understanding the exact nature of the problem is especially difficult because the transmission of cholera didn't follow the expected pattern of the prominent "effluvia" theory of contagion,

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<sup>7</sup> My discussion here is taken from Goldstein and Goldstein (1978, pp. 25–62) who draw heavily on Snow's own manuscripts. Parenthetical references are to their discussion. They admit on their own that the case study is far from a complete history of Snow's investigation (See vii–viii). Nevertheless, they lucidly explain the case in a way that exhibits many of the features in the model discussed above. Since the purpose here is illustration rather than induction, much less to give an account of Snow's research, I trust you will forgive my schematic and incomplete gloss on Goldstein and Goldstein's incomplete account.

according to which disease was transmitted by emanations or exhalations from the sick patient into the surrounding air. Cholera tended to be concentrated amongst the poor, and almost never infected the doctors who tended to the sick. This was taken as evidence that the disease was “a just punishment for the undeserving and vicious classes of society”(26). To regard the problem as one fixed prior to inquiry would be to take as fixed many things that are at first unsettled.

Snow begins by collecting a variety of general and fairly pedestrian facts (29):

1. Cholera began fairly localized in India, where Europeans first encountered it in the late eighteenth century and spread rapidly from there in the early nineteenth century.
2. Cholera travels along channels of and at the speed of human interaction, always appearing first at the sea-ports of new islands and continents, and it never attacks those sailing from countries free of the disease until they enter the port or come ashore in a place where the disease is found.

He then moves to more specific cases (30–1):

3. Mrs. Gore’s son, who had been living and working at Chelsea, came home ill and died of the disease. His mother, who attended him, caught ill the day following his death, and was dead the day after. No other deaths from cholera in the area took place.
4. John Barnes died after having contact with the clothes of his sister who had died from the disease, whose personal effects had been sent to him after her death.
5. Mrs. Barnes, who contracted the illness from her husband, was attended by her mother, who contracted the disease after washing her daughter’s linen.

And so on. All of this clearly suggests the idea that *the disease is communicable*. Further, it might naturally suggest the most common explanation of the transmission

of disease, *the “effluvia” theory* already mentioned. Already the cases of John and Mrs. Barnes suggests some difficulty with this explanation, since John Barnes was never exposed to his sick sister, and Mrs. Barnes mother was healthy while she was in the presence of her daughter, only contracting the disease after contact with her linens. Further evidence tells against this hypothesis (31):

6. It is not always the case that someone who spent time in the same room with the patient, or attending to them, is likely to contract the disease.
7. One need not ever come near to the patient to contract the disease.
8. Other diseases such as “the itch,” syphilis, and intestinal worms are transmitted by vectors other than air.
9. The pathology of the disease begins with intestinal symptoms, rather than any symptoms of systematic infection such as fever.

The final two pieces of evidence suggest another hypothesis: *The disease spreads by some infected matter “ejected” from a cholera patient being accidentally ingested in sufficient quantity, and whenever this accidental consumption of infected matter is likely, the disease is highly likely to be communicated* (33).

This hypothesis suggests some further observations. If it is valid, you’ll find that certain people who come near to the patient do not get cholera (as we’ve seen), and further that they avoided it by way of habits of cleanliness that would prevent them from accidentally ingesting any cholera evacuations. Indeed, this is clearly the case with doctors:

10. Doctors do not generally contract cholera from their patients, while persons who attend to the patient in a more personal way, with less concern for cleanliness, are more likely to contract the disease. (33)

Reasoning through the implications of the hypothesis, we can see that there are several reasons that people of different social classes would have different risk of contracting the disease: they “perform different functions around the sick”, live in different conditions, have different lifestyles and personal habits concerning cleanliness and quantity of human contact (33). Further general observation tends to bear out the hypothesis (34). One bit of evidence raises a puzzle, however:

11. Cholera does sometimes spread to the rich despite the absence of the vectors of direct communication present in the case of poor laborers.

In other words, rich folk live in much less cramped environments, tending not to “live, sleep, cook, eat, and wash” in the same space (34). They do not usually tend intimately to sick persons, or if they do so, they wash carefully and constantly. It seems very unlikely that the illness would spread between family members in such circumstances, and it rarely does. Nevertheless, rich people do contract the disease in some cases. Snow did not take this to invalidate the hypothesis, however. Rather, he supposed a further specification of the hypothesis in these cases that would provide the appropriate kind of transmission vector: *cholera can spread through the water supply* (35), and further cases support this hypothesis.<sup>8</sup>

Having worked out the implications of the hypothesis and found corresponding facts is not, however, where Snow stopped. At this point, his hypothesis is surely plausible, but not firmly established. The next phase requires experimental application of the hypothesis to real situations in order to test its adequacy. This goes beyond merely collecting observations about cases of cholera, either individually or in bulk. Experiment is *not*, as many have supposed, just a special way of generating

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<sup>8</sup> It isn't clear from Snow's reports what the order of inquiry was supposed to be in this case. It is possible that awareness of certain cases where the main difference between those who got sick and those who didn't was presence or absence of a water supply tainted by toilet water suggested the hypothesis to Snow. It is also possible that the hypothesis suggested certain kinds of evidence to look for, or that one or two cases suggested the hypothesis, which in turn suggested a search for like cases.



further observations. In many ways, and in many cases, the procedures may look very similar. Certainly, techniques of observation are part of the experiment, and experiments may even produce data that are fed into the recursive process of coordinating facts and ideas, but the function is nonetheless very different. The functions of *observation* are to fix the conditions of the problematic situation and the terms of the problem, as well as to suggest and refine hypotheses. The function of an *experiment* is to put the hypothesis *into practice*, in a limited and controlled fashion, in order to determine its efficacy in solving the problem.

Snow engaged in at least two experiments, neither of which was entirely satisfactory from the point of view of the model under consideration. His first experiment was with the Broad Street water pump in 1849. In this case, by first observing the circumstances of a certain outbreak of cholera, he was able to determine, based on his hypothesis, the probable cause of the outbreak in the pump on Broad Street. He was able to determine that use of water from that pump was a common cause of most cases of the outbreak (37–39). Likewise, he was able to determine that amongst the groups in the area who were mostly *unaffected* by the outbreak, all had avoided, for one reason or another, use of the pump (39–40). He then made an experimental intervention by convincing government officials to remove the handle from the pump to prevent its use. Unfortunately, removing the pump-handle failed to produce any significant effect on the number of new cases, and this is likely because the epidemic had pretty much subsided by the time of the experiment. So, while there was plenty of supporting evidence for the pump as cause of the outbreak (including indirect evidence of the contamination of the water by sewage), the experiment failed to be conclusive (40–41), because the intervention failed to have any appreciable effect on resolving the problem due to the fact that cases of cholera were already in rapid decline for other reasons.

Snow's second experiment was what is sometimes called a "natural experiment" (42). There were no actively controlled circumstances, nor were there even

any active interventions. Instead, a “natural experiment” is one in which the natural course of events is such as to be *as if* one had set up an experiment to test the results. In the London cholera outbreak of 1853–4, Snow was able to find a very distinctive pattern in the deaths resulting from cholera according to which of two water companies in London supplied the house with water. Snow’s study had two parts. First, using what we would today call “retrospective study design,” Snow began with a district of London in which houses were supplied by two different water companies—Southwark & Vauxhall or Lambeth—in fairly random mixture. He then looked at all of the reported cases of cholera in that district, determining that of the 44 deaths, 38 were supplied by the Southwark & Vauxhall Company. In the second study, using what we would today probably call a “prospective design,” Snow looked at all of London by water company, and discovered that the rate of deaths from cholera in houses supplied by the Southwark & Vauxhall Company was an order of magnitude larger than either those supplied by the Lambeth Company, or among houses supplied by neither (some third party, local well, etc.). Snow argued that the connection between houses and water companies was quite randomly distributed with respect to the relevant factors (two neighbors were even in some cases supplied by the two different companies). This affords a significant test of the hypothesis: it seems difficult to deny that water supply has in this case had a significant effect on incidents of the disease, or that the “act” of avoiding the contaminated water supply significantly reduced the risk of contracting cholera (42–46).

While Snow performed no active interventions in this case, it still plays an experimental *role*. It is not any particular technique that makes the experiment, and Snow need not even have engaged in any direct intervention. What matters is the function it performs, the way that the experiment is *taken up* in the process of inquiry: as an application of the hypothesis to the situation. Nevertheless, one would prefer a more active application of the hypothesis to the problem of cholera, based on the model I’ve described, because this would serve more directly as a test of the ability

of the hypothesis to act as a problem-solution, to move the problematic situation towards resolution. A careful analysis of past events can be of very significant use in the course of an inquiry. But ultimately, experimental inquiry looks forward, towards a transformation of the situation from problematic to settled, rather than backward at what has come before. Hence, when possible, we prefer an active intervention that changes present conditions.

Though he offered further evidence for his hypothesis, Snow never produced such a test. He did provide further support for his theory, however. He rejected certain apparent counter-evidence by providing reasons to regard it as either irrelevant to, or explicable in a way that was compatible with, the main hypothesis. He combined reasoning and observational evidence to provide arguments for rejecting alternative hypotheses. And he described analogous suggestions for other diseases, whose causes were both known and unknown. All of these fit well within the model above, under heading (4): the reciprocal coordination of factual and hypothetical materials. Snow uses observations to help select and refine a hypothesis, and he uses a guiding hypothesis to discriminate putative data, in a reciprocal process that arrives at a tight fit between fact and hypothesis.

The final part of Snow's monograph on cholera is the most crucial, from the point of view of our model, though I suspect it has rarely been regarded as so by other commentators.<sup>9</sup> In the last section, Snow provides a list of twelve recommendations for how to prevent the spread of cholera, based on his two hypotheses, plus some further reasoning about possible cases. For example:

1st. The strictest cleanliness should be observed by those about the sick...

3rd. Care should be taken that the water employed for drinking and preparing food... is not contaminated with the contents of cesspools, house-drains, or sewers; or, in the event that water free from suspicion

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<sup>9</sup> Goldstein and Goldstein, for example, include it in a section near the end of their paper entitled "Applications to Other Problems" (51ff), and treat it as something of an afterthought.

cannot be obtained, it should be well boiled. . .

11th. To inculcate habits of personal and domestic cleanliness among the people everywhere. . . (53–4)

And so on. These recommendations are crucial to the eventual acceptance of Snow’s explanation. The fact that the problem was resolved as far as Snow himself was concerned is relevant to inquiry from a purely personal point-of-view, but for a truly scientific inquiry, social dissemination and understanding, according to the inquiry-model, are crucial to judgments of the warranted assertibility of the hypothesis. Further, no amount of convincing argument or “decisive proof” provided by a scientific manuscript can be the ultimate measure of a scientific judgment’s warranted assertibility. Claims to warranted assertibility must be judged, on the one hand, by others taking the results to be so settled as to provide a steady resource for further inquiry and, on the other hand, by the success of future applications, such as the ones suggested by Snow in this final section. It is the success of these further applications that are the “decisive experiments” that justify Snow’s view, rather than any alleged proofs that Snow claimed he had produced.

### 3.2.5 Evidence on the Inquiry-Model

Before closing the discussion of the inquiry-model, there are a few things that need to be emphasized. First, it is important to notice the very different roles that evidence plays in the course of an inquiry. In many contemporary accounts, evidence is, if not mono-modal, at least mono-functional: all evidence serves as a test of a theory or hypothesis, and it confirms or disconfirms it, or renders it more or less plausible, probable, or credible. In the model of inquiry I’ve been discussing, however, evidence is not only multi-modal, but serves a variety of purposes. *Observational* evidence helps locate the problem (1–2);<sup>10</sup> it provides information about the conditions of the problematic situation (3–5); it *guides* speculation and hypothesis-formation (3–5); it

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<sup>10</sup>Parentetical numbers refer to items from the case study in the previous section.

helps us *eliminate, specify, clarify, or improve* our original hypotheses (6–11). *Experimental* evidence serves not only to generate further observational evidence, but also serves as a tentative application of a developed hypothesis to check its consequences for future action and inference (the Broad Street pump experiment and the study of water supplies). In every case, it is not some abstract or formal relation between the evidence and the hypothesis by which the evidence serves to justify the hypothesis. The formal and symbolic is only one side of evidence. It is rather a very concrete process of transforming a perplexity into a resolution that evidence is instrumental towards, and which ultimately justifies any final judgment of the inquiry.

Second, it is important to recall that the model at hand is an idealization in several senses. It is idealized in that it is *simplified*: it does not even pretend to capture every important element of scientific practice. It is nonetheless a useful idealization: the clarity it lends to particular cases such as Snow's, and more importantly the ease with which it resolves or dissolves a variety of puzzles about the nature of evidence that plague contemporary discussion (which I will discuss below), will demonstrate its usefulness. It is also an *ideal* model, that is, it makes some modest *normative* claims. It hopes to capture something of the lesson of successful inquiries of the past. The model is about the best (the ideal) way to carry out inquiry. It is ultimately an interpretive model: individuating inquiries is a tool of the inquirer into inquiry, and the divisions need not be clear within primary inquiry, to the inquirers themselves.

Third, the model makes no claim that science is generally or usually a large-scale movement from less to more certainty. The ubiquity of problems in scientific research suggests otherwise, certainly. Perplexities arise in many ways: from failed application, from new evidence garnered elsewhere, from theoretical-aesthetic worries, and as by-products of other inquiries. Scientists positively go hunting for problems to work on; by searching for potential problems, they secure in advance new ways of coping with the world and stabilizing practices that could otherwise become

unsettled in tragic fashion. Nevertheless, something like what is described in the model in question, it is claimed, goes on once they fix on some perplexity and set to work on it in a fashion that tends to lead to success.

Hopefully I have at least been able to garner some plausibility for the model. It is all I need for what follows. If I have grossly oversimplified the nature of scientific evidence with this model, all for the better, since an even more complex account of the development of inquiry in time and the variety of evidential functions will serve my purposes just as well, if not better. Let us move, then, to some putative philosophical problems of evidence.

### **3.3 Some Problems of Evidence Dissolved**

#### **3.3.1 Theory-Ladenness and the Experimenter's Regress**

There are two distinct but related worries that people have about evidence. Many people regard the impact of theory on evidence as having problematic consequences. Discussions around the work of Norwood Russell Hanson, Thomas Kuhn, and Paul Feyerabend raised significant worries about whether their arguments for the theory-ladenness of observation undermined the importance of empirical evidence. These worries continue today. For example, Robert Hudson (2000) believes that if we cannot make room in our epistemology for direct perception, unmediated by theory or concepts, then we can never escape the “hermeneutic circle” and find some independent ground for our knowledge-claims unsullied by the question at issue. The same worry can be put about the need to interpret “raw data” before it can become “data” or “facts” (Culp 1995, p. 439).

In a related vein, philosophers like Sylvia Culp (1995) worry about and attempt to solve the problem of the “experimenter’s regress” raised by H.M. Collins (1975, [1985] 1992). Rather than a concern about how theoretical frameworks in-

fect data, the experimenter's regress is a worry about how our expectations about results and our assumptions about certain techniques lead to circularity. According to Collins, good data is regarded as the product of a good experimental technique, but the test of an experimental technique is whether it produces the expected data. For example, Collins looks at the case of gravity wave detection experiments ([1985] 1992, pp. 79ff). He argues that,

What the correct outcome is depends upon whether there are gravity waves hitting the Earth in detectable fluxes. To find this out we must build a good gravity wave detector and have a look. But we won't know if we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until... and so on *ad infinitum*. (p. 84)

We have here a tight couple between the technique we use to gather data, the validity of the data itself, and our expectations about what data we expect to find. The "experimenter's regress" has two forms for Collins: a practical and a philosophical form. In the practical form, it presents a problem for scientists who must find a way to break the circle in order to resolve a dispute. In some cases, like the case of the TEA-laser that Collins discusses earlier in the book, the circle is broken by some practical result, e.g., the laser actually performs. In the gravity-wave case, no easy external criterion is available. Collins shows how variously interacting arguments about calibration, results, instrument sensitivity, assumptions about the data, the existence of the waves, etc. eventually led to the kind of "control on interpretation" that breaks the circle.

But from a philosophical point of view, this doesn't settle the problem. It is not on the basis of some conclusive evidence that the circle gets broken, but rather,

the definition of what counts as a good gravity wave detector, and the resolution of the question of whether gravity waves exist, are congruent social processes. (p. 89)

And further,

I am arguing here that just as the process of deciding whether gravity waves had been detected was coextensive with deciding which set of results was to be believed, so the detailed *nature* of gravity waves was settled at the same time. Different decisions about the quality of the experiments would have gone hand-in-hand with different decisions about the nature of gravity waves. (p. 100)

Since these decisions are made as a package, it is the *contingent, social* process of negotiation and decision-making that “break” the regress. The solution to the problem is thus a “sociological” rather than a philosophical solution (pp. 145ff), since experiments and evidence cannot do so. This leads to a form of *relativism* (p. 1) which holds that science studies should “treat descriptive language as though it were about imaginary objects” (p. 16) since it depends on contingent decisions, which different “networks of science and of society” (p. 130) would have made differently.

Let us step back and think about the ideas about evidence in play. Something like the following picture, suggested by Culp (1995, p. 439–40), is surely right: we set up an observational/experimental apparatus and run it. At one level, it merely produces brute happenings of a certain sort. We must then interpret those happenings, take them up as a certain item of fact, and, metaphorically speaking, teach them to speak the language of the theory, in order to see how they bear on the theory. (Of course, these “interpretations,” according to the defenders of theory-ladenness, take place at the level of seeing itself, not afterwards.) This interpretation is never independent of theory, neither the theory of how the apparatus works nor the theory in question. Further, thanks to the experimenter’s regress, it is not only because we have a background *theory* informing our observation that data is infected, but more basic expectations about what data should look like and which techniques are reliable lead to a problematic circularity between data and technique (Culp 1995, 438–9). All of this presents a problem: we are left wondering how interpretations of experiments that *themselves* presuppose controversial theories, including parts of the theory in question, can serve as solid ground to support our theories; we are left



wondering how claims about the reliability of a detector, which *themselves* presuppose controversial assumptions about what counts as “competent” data, including assumptions about the existence of the object in question, can serve as solid ground to support detection-claims.

But notice how, from the point of view of the model of inquiry we’ve been discussing, several parts of the story have been left out. For one, it mentions only one direction on the two-way street of the coordination of factual and conceptual materials. Contra Culp’s supposition, we don’t only teach evidence to “speak the language” of theory. We also teach the theory to speak the language of observation; that is, we must develop our hypotheses so that they have operational consequences, that they may *direct* activities of observation. This too is an “interpretation,” if you like, of the theory, but it is very different from the process of interpretation that Culp discusses (not to lessen the importance of that phase, either). Collins’ and Culp’s shared way of setting up the problem presupposes that theory is inert, and experiment must be constructed or interpreted in a way that meets it. But theory and experiment must meet in the middle.

Further, they construe the function of evidence extremely narrowly. Evidence is taken to be exhausted by its function of supporting a hypothesis. But this is a narrow and relatively minor function of evidence within the course of inquiry. Observation serves to help institute the problem and indicate the fixed field of the situation, it suggests hypotheses for solution, helps elaborate or clarify hypotheses. Experiments put hypotheses to work in tentative application, trying them out as solutions to a problem. It is undeniable that in some sense, theories “produce” their own evidence. But this is only a problem if evidence serves only to justify theory, and theory is justified only by that body of evidence it produces. To the contrary, producing (not predicting) some events is the *point* of a theory; it is the adequacy of the consequences produced to solving the problem, along with its usefulness in attacking *new* problems and supplementing *new* inquiries, that are the ultimate test

of the theory. A theory which *failed* to produce its own evidence, i.e., failed to produce any new phenomena, would be inert, useless, and unjustifiable. It would be impotent to solve any problems.<sup>11</sup>

An important part of the problem of the experimenter's regress is the issue of *calibration*.<sup>12</sup> Early attempts to detect or measure some previously unobserved or unquantified phenomenon are faced with a problem of how to calibrate, lacking any other techniques to check against. We have only theoretical expectations about what the phenomenon should be like to guide us.<sup>13</sup> Later attempts are faced with the problem that their calibration depends on previous measurements which themselves were not calibrated in a standard way. In both cases there is a troublesome regress; in the earlier cases, we accept the measurement because it gave us the kind of results we expected—but then, it is hardly independent evidence for those expectations. In the later cases, we accept a measurement because it accords with our previous techniques in overlapping domains—but then, it is neither independent evidence for the reliability of our prior techniques, nor ultimately for our theoretical predictions.

But the question we must ask is, “What is this experimental evidence *for*?” Under the impoverished model of theory-evidence relationships that regards the sole role for evidence to be either adding or removing support from a hypothesis (in context-free fashion), the experimenter's regress is a serious concern. If evidence lacks independent plausibility, it cannot stand as support in the way this simple model would hope. This problem has been addressed in a variety of ways by different authors. Godin and Gingras (2002) have suggested that the “experimenter's regress” amounts to just the classical problems of skepticism, and thus that we should get around it in the same way that we get around skeptical worries in epistemology

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<sup>11</sup> Though it is important to keep in mind that the range of problems and the diversity of ways that phenomena are produced exceeds what is commonly called “practical” in the narrow sense.

<sup>12</sup> See Franklin (2007, §I.B.1)

<sup>13</sup> Hasok Chang's work on temperature (2004) show in that case that there is ultimately a set of unchallenged expectations that inform what counts as an acceptable measurement technique. Basic assumptions like linearity, single-valuedness, etc. are inescapable (pp. 90–2).

generally. Godin and Gingras alternatively describe the way out as “mitigated skepticism” (141), “pragmatism” (140, 141), and “community and argumentation” (144), but it all amounts to the point that we can arrive at scientific consensus, but this consensus is only “pragmatic and time-situated” (146) and never amounts to absolute justification. In some sense this must be right, but it won’t dissolve the problem for most philosophers, since the problem is *internal* to the traditional  $E \Rightarrow H$  model, once the facts of theory-dependence and the experimenter’s regress are accepted. Collins, if Godin and Gingras are right, has merely shown us that the classical and modern problems of skepticism apply to the  $E \Rightarrow H$  model. Just as “From the point of view of classical logic, there seems to be no way out of the skeptical regress” (Godin and Gingras 2002, 141), from the point of view of the traditional  $E \Rightarrow H$  model of evidence, there is no way out of the experimenter’s regress. One must either elaborate or replace this model to avoid these skeptical problems.

On the other hand, the pragmatist model of evidence being defended here provides a very different answer to the question of the purpose of evidence. Evidence has a variety of functional roles within an inquiry, the main goal of which is the resolution of the perplexity which spurred the inquiry. In general, then, the experimenter’s regress will not present any difficulty, since all that matters is that the evidence fulfill its role *well enough* for the purposes of solving whatever problem presents itself. Genuine problems of inquiry set the conditions of their own solution. They do not “go away” because some external standards of “objectivity” or “justification” are satisfied. Only a transformation of the situation to remove the original discoordination or difficulty will suffice. So long as we find a way to combat the disease and increase the life and vitality of people, it doesn’t matter that the experimental techniques have a variety of dependencies on the experimenter’s expectations. Since experiment is *not* merely a procedure for producing neutral evidence, but *rather* a way of *making* and *doing* that puts the hypothesis *into practice*, there is a test of the experimental evidence, together with the hypothesis, that is independent

of expectations *per se*. Expectation cannot prevent a bridge from falling down, nor can it cure disease, nor can it even reconcile the incompatibility between quantum mechanics and relativity theory. The germ of this solution exists in Collins' own discussion, i.e., in the case of the TEA-laser. What Collins ironically misses is that the question of the existence of gravity waves is not itself a context-free, abstract question, but rather part of a social process of dealing with a problematic practice (a perplexity), and the concrete factors of that situation provide the conditions for adequate solution, just as the narrowly *practical* function of the laser provide the conditions for adequate solution in that case.

An alternative solution to the problem posed by the experimenter's regress is to appeal to the *robustness* of evidence. We need not have full independence of evidence from our expectations. Rather, what we need is evidence from a *variety* of different kinds of sources that are independent from each other and that still support the same conclusion. Evidence from a single source that seems to support the conclusion but only does so due to being calibrated to that way would be problematically circular. A variety of different types of evidence, developed independently from each other at different times and places, which all seem to support the conclusion but in fact are just the product of our expectations, so the argument goes, would be a miracle. The truth of the conclusion is the better explanation.

The strategy is an appealing one. Suppose you want to build a bridge to carry a train across a ravine. All the individual wooden boards at your disposal are inadequate to carry the weight of the train. One could either give up on the possibility of using wood to support the train, or one could try to figure out if a large enough collection of boards, arranged in a very particular way, might do the job. In Culp's argument, she fully admits that no particular bit of evidence can be theory-free, that it doesn't even make sense to talk of uninterpreted, bare "happenings" as evidence. Nonetheless, since she is committed to the metaphor of support, she attempts to find an arrangement of evidence that can serve as a fixed-enough support. A set of

evidence can be a foundation for theoretical knowledge if it is *robust*—if it comes from a variety of sources that are theoretically independent of each other.

This argument fails to meet the challenge posed by the experimenter’s regress, however. At least three difficulties arise, one empirical and two epistemological. (Compare to Jacob Stegenga’s “three easy problems” for robustness.) The first is the difficulty of finding *really* independent sources of evidence. The history of the development of experimental techniques is replete with a variety of cross-calibration techniques. Hasok Chang’s (2004) discussion of the development of the modern thermometer shows the complex interdependencies of various new techniques for measuring temperature (see especially Chapter 3). Early errors propagate into later techniques and take a long time to disappear entirely, as in the case of measurements of the charge of the electron,<sup>14</sup> because of the preponderance of cross-calibration. True independence may be difficult to determine (Stegenga, 3–4).

The second problem, which springs from the first, is that robustness doesn’t really solve the problem of calibration. For any particular measurement technique, there are two cases: either it is calibrated according to existing techniques, or it isn’t. In the former case, the possibility of independent techniques of measurement is seriously endangered. Furthermore, the question of how those pre-existing techniques were themselves calibrated must be examined. In the latter case, it would appear that all we have to go on to judge the results provided by the technique is the very expectations we hope to support. A variety of different types of evidence, all calibrated by reference to the same set of expectations also lack the independence required by the argument.

It may be that the original types of measurement, though originally calibrated in a suspect way, are calibrated with respect to different, independent sets of expectations. While problematic in those original circumstances, in a *present* case, they may be sufficiently independent *from one another* to provide robust, adequate evidence

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<sup>14</sup> This case is vividly recounted in Richard Feynman’s essay, “Cargo Cult Science.”

in the case at hand. Even supposing that this case passes the empirical test of independence discussed above, a larger epistemological question about whether we ought to rely on the evidence remains. Perhaps we ought to regard it as a miracle that a variety of such evidence purportedly supports a single conclusion, but why should we think for one moment that the truth of that conclusion explains the apparent miracle, given the story of evidence now on offer? A variety of methods, calibrated under highly suspicious circumstances, apparently providing no real support in the case of their original development, now all happen to agree on one conclusion. Do we have any reason to believe that this coincidence has anything to do with the truth of the conclusion? Not without some prior reason to think that the methods, taken individually, track the truth in even a modestly reliable fashion, i.e., that the methods track *some* signal, and don't just produce noise. But it is *precisely* the lack of such a reason in the case of individual techniques that leads to the demand for robustness in the first place.

The final problem is the nail in the coffin for the prospects of solving this problem through the appeal to robustness. In order to have truly independent sources of evidence, it is crucial that the measurement techniques not be calibrated to one another, lest the bias in one creep in to the other. The sources must be multi-modal, and they must be incommensurable, in the sense of not having any inter-modal standard of comparison (the existence of such a standard strongly implies mutual calibration, unless it is a merely hypothetical standard). If they are incommensurable in this way, however, we're left with a major worry: if we have no standard of comparison between the types of evidence, how can we say determinately that they support the *same* conclusion (Stegenga, forthcoming 2009)? If the interpretive framework at hand is the theory in question, of course, then it is easy to see how different bits of evidence support the same conclusion. But then the evidence isn't really independent in the way that Culp demands. Suppose, then, that the evidence, that is, "raw data" plus interpretation, are all independent from one another. How

do you determine the relevance of each to your hypothesis?

This question may be practically answerable in a relatively loose and informal way, when all of the evidence seems to tell in favor of, or is at least consistent with, the hypothesis. But what if the evidence isn't so concordant?

### 3.3.2 Discordant Evidence

In recent work, Jacob Stegenga (forthcoming 2009) has discussed the problem, raised by Franklin (2002) of discordant evidence, that is, the problem of how to address diverse, multimodal evidence which appears to pull towards *different* conclusions. For example, Stegenga discusses the case of the transmission of influenza. Clinical evidence such as patterns of transmission suggest that the flu is transmitted only by contact. On the other hand, mathematical models and some case studies suggest that it is quite likely that the influenza virus is spread through the air. Given the (as I've argued, necessary) lack of any meta-standard for balancing diverse evidence, difficult decisions must be made about which set of evidence is more relevant in this case. The problem of discordance not only raises doubts about the value of robustness, but raises a clear problem for scientific methodology itself: if evidence of different types conflict, what are we to do when making decisions where evidence is required?

When evidence is of one type only, fully commensurable, problems of discordance do not occur. There may be disagreement between results, but these can be chalked up to error, noise, or a problem with the technique. It is a basic assumption of a measurement technique that it provides consistent results within its margin of error (Chang 2004, 90–2).<sup>15</sup> Further, when different techniques are commensurable,

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<sup>15</sup> Of course, more sophisticated measurement techniques than thermometers may produce evidence that appears less consistent, and statistical analyses must be applied to make sense of the results. But then, I would say that what functions as “evidence” in this process are not the individual data-points that are fed into the analysis, but the original process of analysis itself. My thanks to Jacob Stegenga for reminding me of this complication.

as in the measurement of temperature with a wide variety of thermometers (Chang 2004, Ch. 2 & 3), it is common practice to calibrate the techniques so that they give consistent results when their areas of functioning overlap. When techniques are multi-modal and incommensurable in the way that *robustness* requires, however, the problem of discordance arises. Franklin (2002) suggests that robustness can *solve* this problem, but, as Stegenga argues, it is the multi-modal requirement of robustness that causes the problem. In cases where evidence is not multi-modal in the sense that robustness requires, no difficult problem of discordance arises. In cases where evidence aims at robustness, discordance will often arise, and cannot be erased by gathering further evidence.

Appealing to robustness alone, the best one can do is increase the amount of evidence pointing in one direction. This fails as an adequate solution to the problem of discordance, however, as it fails to address what Cartwright (forthcoming 2009) and Stegenga (forthcoming 2009) term “the problem of relevance.” When multi-modal, incommensurable evidence disagrees, it matters not only what the *quantity* or even the *quality* of the evidence is. It also matters which evidence is more *relevant* to the problem at hand. In the epidemiological case mentioned above, much of the controversy depends on one group believing that the clinical evidence is more relevant, while others think that the models and case studies bear more importance. This goes beyond mere precision and validity. The question is, given a hypothesis, which evidence bears more directly on its truth or falsity.

If, as I’ve suggested the adherents of robustness must admit, the hypothesis and all the different types of evidence must come from independent conceptual backgrounds, and thus to some degree “speak different languages,” then the problem of relevance is of utmost importance. We must be able to determine how some piece of evidence bears on some hypothesis where there is no simple way to plug them in to a probabilistic formula nor a deductive syllogism.

But as soon as we state the problem this way, it seems utterly insoluble. If



there is no common ground between putative pieces of evidence, or between evidence and hypothesis, how can they be reconciled? Without standards for mutual comparison of the type that allow cross-calibration, what way is there to settle the differences?

One possibility is to find a formal meta-standard for comparing evidence and determining its relevance that is independent of and blind to the background assumptions in question. Such standards are in place for so-called evidence-based policy which only look at experimental design (RCT, case-control study, etc.), but these fail to really capture relevance (see section on EBP below). In general, such standards will always fail because the problem of relevance depends on the *content* of the evidence and hypothesis, not just the formal aspects. Furthermore, I think we should generally be suspicious of such formulae; the attempt to find a simple algorithm or recipe for reconciling various types of evidence amounts to an attempt to solve a difficult task faced by all research in one fell swoop. In all likelihood, this is simply a problem that must be solved in the course of each inquiry, on its own terms, and cannot be eliminated by philosophical sophistication.<sup>16</sup>

Another option is to say that while no formal methods of reconciliation are available, good scientists will nevertheless be able to see how to determine the relevance of the evidence to a hypothesis. It is a creative, skillful activity, and while no explicit rules can be articulated, the tacit knowledge available to practitioners allows them to make good judgments about relevance. While this must to some degree be correct, it is an inadequate answer to the problems of relevance and discordance. First, it is difficult to normatively assess tacit knowledge and skilled judgment. There is a difference between what judgments scientists are *justified* in making and simply what a scientist or group of scientists *in fact* does, but it isn't clear how to distinguish the two if scientific judgment is so inarticulable. Second, this doesn't address

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<sup>16</sup>Such attempts at short-cut solutions are a vicious but near-pervasive feature of philosophy, especially epistemology. More on this below.

the way that disagreements about relevance and how to resolve discordant evidence are pervasive in scientific controversy. If skillful judgment can resolve the problem, then why is there so much disagreement on just this matter? Finally, this answer presupposes an illegitimate individualism in its understanding of the scientific process. Ultimately, it is not individual scientists who have the last word on scientific debates. Rather, science is a social phenomenon, and these matters must be decided on a larger scale than individual judgment. Skill and tacit knowledge surely play a role in how science gets done, but settling disputes over relevance of evidence must take place at a more explicit level.

What we must do is reject “robustness” altogether (in the very specific sense that defenders like Culp are forced to accept). The call for evidence that is independent from the hypothesis in question and a set of evidence each independent from the rest is an impossible requirement. Without some shared background and structures of commensuration, without the ability to coordinate hypothesis and evidence, there is no way to push inquiry forward.

Which is not to say that there are never difficulties of determining the relevance of some data, or that there are never problems of inconsistency or incongruity between evidence. Discordance can be a real problem, not for epistemology but for scientific inquiry itself. What is problematic in the way that some philosophers approach evidence is that they hope to solve this problem once-and-for-all with some formal method or meta-standard that obviates the need for further research. Franklin’s (2002) instincts are right when he suggests that the problem of discordance can be solved by gathering further evidence, but this answer fails if we understand it either in terms of robustness or in terms of the traditional models of evidence. Looking at the problem of discordance from the point of view of the inquiry-model, resolving discordant evidence just amounts to resolving the inquiry in question.

At the beginning of an inquiry, we expect discordant evidence. If the evidence was at first blithely all in agreement, there would be no problem for inquiry to resolve.

The situation would already be wholly determinate (at least in the relevant respects), and so we could simply apply our theory and move on. Discordant evidence is part of what sets the problem for inquiry in the first place. When Snow first began to study cholera, there was evidence that pointed towards it being an ordinary, communicable disease, but there was also evidence that many people exposed to the disease, such as doctors, rarely caught it, and others never exposed to cholera patients nevertheless caught the disease. How then is the disease transmitted and how can it be contained? Discordance will also naturally arise in the mediate phases of inquiry, and it is a driving force for the improved articulation of both the hypothesis and the data. Snow was able to explain with his non-effluvial hypothesis why doctors rarely contract the disease. But then he had to explain the occasional epidemics in which the rich and poor alike became infected. This drove further articulation of the hypothesis (transmission via water supply) and suggested new observations or experiments (track or intervene in the distribution of water).

Discordant evidence is part of the problem of inquiry, because it sets the problem and is part of the mediate phases of inquiry in which the attempt to coordinate evidence and hypothesis is not yet complete. It is not a further problem for epistemology, if that means that we should be looking for a way of resolving the problem that goes beyond the way it is done in the ordinary course of inquiry. Take the example of influenza again. Controversy continues about whether it is airborne or transmitted only by contact, with each side marshalling evidence in its favor. If we ignore the temporal complexity of inquiry, then we can see this as a serious problem for epistemology: given this conflicting evidence, what should we believe, what should we do? If we attend to the process of inquiry, however, we see that this is simply an intermediate phase of the investigation. Philosophers cannot settle it by fiat; scientists must settle it. And they must do so by proposing and refining hypotheses that explain the discord, finding reasons to reject apparently relevant evidence, gathering further evidence and constructing new experiments that bring

the controversy to a close.

One might respond that while scientists might have all the time in the world to settle the theoretical question of the nature of influenza, decisive action must be taken *now* to control and prevent this sometimes life-threatening disease. So it must, but as we saw with Snow's case, policy itself is not separate from the process of inquiry. Policy is itself an application of a hypothesis to a problem. If the inquiry has not been satisfactorily resolved, *policy itself* must be of the nature of an experiment. How the experiment is run depends on particulars of the situation and the values in play. Amongst other things, this means that precaution must be played off against cost, judgments must be made about what at present is the most likely answer, and where possible, alternatives must be tried. Consequences of the policy must be monitored carefully, and the policy must be periodically reconsidered on the basis of the evidence it generates (as is rarely done once a policy is put in place).

Dewey indicted much of traditional philosophy for attempting a short-cut around inquiry when only inquiry would do, a quest for certainty which is misconceived at best and positively damaging at worst. Philosophers have been at their best when they observe and distill the lessons of inquiry so that they may be made available in other inquiries, though even this often happens *despite* their intentions to seek certainty.<sup>17</sup> In the current discussions of evidence, the temptation is always to try and find a short-cut around the difficult task of inquiry, or to declare the problem insoluble. These temptations must be resisted.

### 3.3.3 The Value of Robustness

In attempting to respond to the experimenter's regress and related problems, the defenders of the value of robustness have created an insoluble dilemma. Robust-

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<sup>17</sup> See the final chapter of the *Logic*, "The Logic of Inquiry and Philosophies of Knowledge," where Dewey discusses the ways in which traditional philosophers of different schools have, by partial attention to features of inquiry, gotten certain aspects right and others wrong. (LW 12: 506–527)

ness must on such accounts achieve independence from foreground expectations and background theory, not because individual parts of the robust set are so independent, but because the members of the set are so independent from each other that the potentially infecting theories, concepts, assumptions, and expectations “cancel out.” But in order to achieve independence, the members of the set of evidence must end up being mutually incommensurable, because commensurability requires conceptual connections that endangers the required independence. However, the incommensurability of evidence brings with it the problems of discordance and incongruity, which threaten the very possibility of determining how the evidence bears on a hypothesis, especially when the evidence seems to pull in different directions, but even in the cases when the evidence apparently agrees.

We’ve seen that robustness isn’t necessary to solve the apparent problem of the experimenter’s regress, which is really just a problematic artifact of an impoverished model of inquiry. We’ve seen that the problem of discordant evidence is solved once we recognize that it is a necessary but mediate phase in any inquiry. But we should also notice that “robustness” as I’ve argued that its defenders are forced to define it, as the property of a set of mutually incommensurable evidence, is actually an impossible requirement, if we intend the inquiry to move towards resolution. For inquiry to come to resolution, we must be able to form the evidence into a unified whole, bringing that whole into coordination with a series of hypothetical reasoning. While it is doubtful that there are any formal methods that once and for all commensurate discordant evidence, it is a necessary part of the creative, explorative process of inquiry to forge connections between the data, to suggest additional possibilities for test that will resolve inconsistency.

From the above, it may seem that robustness has no place as a scientific norm. This is an unacceptable conclusion, given the obvious value of robustness as an epistemic norm and as an explicit commitment amongst scientists (Culp 1995, 441ff.). But it isn’t the value of robustness *per se* that has been challenged in this chapter.

Rather, it is the particular way of understanding robustness that Culp and others are forced into. Robustness, as it figures in the methodological platitudes which the defenders of robustness cite, is merely the recommendation to seek evidence of several types from different sources. The further requirement of complete independence is forced by the purposes that Culp puts robustness to. If we relax these impossible restrictions on robust evidence, then what value is there to robust evidence?

The stated aim of robustness in most of its defenders is to seek *independent* sources of evidence. This is usually understood as *conceptual* independence: the expectations, concepts, theoretical background, etc. that inform each piece of evidence is varied throughout the robust set so that the “same” conclusion is reached by pieces of evidence with independent backgrounds. As we’ve seen, in the course of a particular inquiry, this requirement that different pieces of evidence be conceptually independent from each other and the hypothesis in question is not only impossible but counter-productive. It would hinder rather than advance inquiry. But another type of independence that is not only possible but productive is *physical independence*.<sup>18</sup>

Experiments or observations are physically independent when they involve distinct physical sources of evidence or processes of evidence-gathering. This can mean that the instruments are constructed to work in very different ways, that the background *physical conditions* are different, or that the actual process under study is different. Thompson’s use of cathode ray tubes and Millikan’s oil-drop apparatus provide very different set-ups both giving results of the corpuscular nature of electricity and the charge (or mass-charge ratio) of the electron. The measurement of a single phenomena under different conditions (different locations, altitudes, on

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<sup>18</sup> Nothing metaphysically fancy is meant here by “physical.” I only mean to make a distinction with what is conceptual. That is, physical independence is a matter of the subject-matter, the background conditions, and the apparatus used to gather evidence, not the concepts we have of any of these things. It is about the set of events and interactions that happen in nature when an observation is carried out.

earth and in space, under different temperatures, and so on) is sometimes pursued. And different phenomena relevant to the hypothesis can be pursued: Snow's research on cholera looked at evidence both about the way that the disease develops in the cholera patient once they have contracted the disease, and at evidence about how the disease spreads throughout the population, and both coordinate well with his hypothesis about how it is spread (ingestion).

Why is such evidence valuable? In a single case, evidence is an interpretation of some bare happening. That bare happening is the result of background physical conditions, the process putatively being observed, and the instruments (which may just be light passing through air into our optical system). What may seem to be the behavior of the process in question may actually be a result of the idiosyncratic combination of system, background, and apparatus. A robust set of evidence aiming at physical independence can lessen the amount of such idiosyncrasy.

A certain limited sort of *conceptual* independence is valuable as well, in different ways at different stages of the inquiry. It is probably best if the initial observations that attempt to determine the conditions of the situation and the terms of the problem are as independent *as possible* from controversial or untested ideas. Starting with radical or controversial theoretical backgrounds can sometimes pay off, but it is a risky way to begin. (If less risky methods fail to lead towards solution, of course, it is reasonable to backtrack and take a more controversial path.) Beginning with observational techniques that have yielded warranted conclusions in many and various past inquiries is a safer bet,<sup>19</sup> but it is not always an available option. When "standard" techniques are not available, it is better to begin with observations from a variety of relatively independent perspectives. Of course, complete incommensurability of evidential techniques cannot survive the course of inquiry, but it *may* be a useful place to begin under certain conditions.

While a certain degree of conceptual unity is necessary for resolving any par-

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<sup>19</sup>Hopefully, this is a simple platitude.

ticular problem, the scope of conceptual resources almost always outstrips a particular case. There will be questions that a particular hypothesis or theoretical framework speaks to that isn't relevant to the case at hand. When those hypotheses and frameworks are closely tied to the evidence used in the inquiry, it might be best to seek evidence that is conceptually independent *with respect to the controversial but unnecessary claims of the theory*. For example, for much of Snow's research, what matters to his hypothesis is that diseases are communicable, that they can "reproduce" in the body, and that they can be spread by ingestion. Snow also believed that diseases were single-celled organisms, but nothing in the version of the historical story told above requires it. If any of Snow's evidence depends on the cellular nature of disease, it would be helpful to make the evidence-base more *robust* by finding techniques that coordinated with the hypothesis in the necessary ways but was independent of the cellular theory.

Much of this discussion will seem commonplace, and hardly worthy of attention. It is important to show, however, that there are reasons why scientists aim at different sources of evidence, both physically and conceptually, reasons that are defensible independently of the problematic ways that philosophers have lately understood the demand for robustness. Robust evidence is indeed of value, though robustness cannot be a panacea to philosophical concerns about evidence, nor is it a trumping value over other concerns. But it is, *ceteris paribus*, an important condition on a warrantedly assertible resolution to inquiry.

I now turn my attention to a set of concerns that at first blush are concerns I am quite sympathetic with. I will discuss the ways in which I am in favor of Nancy Cartwright's calls for philosophical analysis of "evidence for use," but I will also suggest certain precautions for this enterprise on the basis of the foregoing discussion.



### 3.3.4 Evidence for Use

Nancy Cartwright, in her “Well-Ordered Science: Evidence for Use” puts forward the principle:

What justifies a claim depends on what we are going to do with that claim, and evidence for one use may provide no support for others. (983)

Though I have not relied heavily on this feature so far, the pragmatist model of inquiry suggests a certain contextualism: the conditions of warrant are set not so much in an abstract or universal way, but relative to the particular problematic situation at hand. In other words, it is the perplexity that we are trying to resolve which decides whether we have a solution, not some abstract criteria. Given the sheer diversity of perplexities that beset scientists and guide scientific projects, Cartwright’s principle of contextual warrant seems to follow.

In discussing evidence for use, Cartwright asks two related, but very different questions. One of them is right on target: What account of evidence can we offer to make sense of how warrant travels from experiment to application. In this case, I would offer the model of inquiry above, which closely ties experiment to application, as an excellent starting point, though not the only reasonable one. I would, on the basis of the confusions discussed in the previous section, and on more general grounds, have significant worries about any model that failed to account for the temporal dynamics of inquiry, the functional variety of evidence, or the action-oriented nature of science. Ignoring or denying any of these important general characteristics will make a satisfactory account of the use of science difficult or impossible, it seems to me.

In this vein, Cartwright calls for the development of expertise in combining evidence, suggesting that we don’t have a good idea of how to pursue inquiry which escapes the bounds of particular scientific disciplines. There are many inquiries where we need to combine work being done in different modes, languages, with different

levels of precision, and so on. This is especially palpable when large-scale social problems are in question. Here, I would start to suggest caution. If what Cartwright really means (and it sometimes seems that she does) that we will solve this problem by a *philosophical* account of evidence (by which she seems to mean analytical, rigorous, and formal), then it seems to me that she is on the edge of that fallacy of attempting to replace the need for first-order research, that is, inquiry, with a formal short-cut. It is not scientific *epistemology* that we need to solve problems at these interfaces. It is scientific *inquiry*, done not in the mode of this or that discipline, but taking seriously the subject-matter of the boundary itself. We need interdisciplinary and cross-disciplinary research programmes of the sort that are becoming increasingly popular (science studies, cognitive science, neuroeconomics, bioengineering, to name a few). On the other hand, if she means instead that we need to philosophically demonstrate the *need* for such inquiry, then she is arguing precisely along the lines that I have been suggesting in this chapter.

The second question, which seems even more clear to me to be a matter of confusion, is the question, how does warrant travel from experiment to theory to use. One answer to this question is the “Positivist/Popperian picture of *exact science*” (983) where evidence all goes into warranting the theory, which then provides “off-the-shelf” results for immediate use. The absurdity of this sort of view has been demonstrated in the prior discussion, and Cartwright’s critique does an even better job of it. Nonetheless, Cartwright’s proposed project of providing a theory of “evidence for use” promises to fail if what she’s looking for is a different answer to the same question. Evidence serves many purposes, and the “support” relation from evidence to theory is just one small part of the story. Experimental evidence is directly connected to use, in the sense in which a theoretical hypothesis is *used* to solve a problem. Admittedly, Cartwright immediately suggests that warrant might not travel in this way at all, so that we might need a very different kind of account. From the perspective of the inquiry-model, what ultimately warrants a conclusion

or judgment is inquiry, and the same basic pattern applies to both theoretical and practical inquiry. Again, it is not clear that I have any quarrel with Cartwright; I merely mean to recommend one future direction on the basis of her call and warn of the probably futility of some others.

Another way of construing these worries in a way less problematic is to construe them in terms of *bets*. There is a body of standing evidence; I want to know what decisions to make, given what I know. The question amounts, then, to a question about how to make decisions under uncertainty. Some tools for answering the question understood in this way have been and can be developed by decision theory; here, I have insufficient expertise or interest to comment. But if the inquiry-model has the wide applicability that I believe it to have, and that Dewey attempted to demonstrate, then the pattern of inquiry has a role to play in any such decision that doesn't take the form of a snap-judgment, under which no in-depth consideration of the evidence is possible.

### 3.4 Real Perplexities of Evidence

Nothing in the discussions above says that there are no real worries about evidence that should plague scientists or philosophers. There are rather two major lessons to be learned: first, any problem of evidence is only relative to the *model* of evidence or inquiry in question. Philosophical questions are not independent of the concepts, vocabularies, and models we use to pose the questions. Some apparent problems may disappear without remainder when we switch the model. Second, there are no easy simple or certain answers to difficult problems. We cannot shortcut empirically or philosophically difficult issues that require first-order research. Acquiring, using, balancing, and understanding evidence is hard work, but it is *hard* because they are parts of research, and research is a *hard* process, requiring creativity, ingenuity, gumption, and time. To think that we can provide formal frameworks or

philosophical tricks that will dissolve the difficulties is of a piece of the futile “quest for certainty” that has been with philosophy from the beginning.

While changing our model may show some problems to be merely artifacts of the model, this isn’t always the case. Problems are our attempts to carefully formulate real perplexities, which are independent of the model we use in the formulation. While we sometimes find that the model itself generates problems that can be avoided by giving up the model, we also find that there are perplexities that don’t go away when we give up a certain way of stating the problem.<sup>20</sup> We struggle both in the doing and the understanding of the scientific process with concerns about the way that evidence is arrived at and its interaction with hypothesis. We strive both to codify the lessons learned in the process and make them available to increasingly wide areas of problem-solving, and to improve the process itself. There are many areas of concern to address in future work.

I will conclude this chapter by discussing one such area that has lately received much philosophical attention from a fairly traditional perspective. Philosophers have raised many worries about so-called “evidence-based policy,” which, despite their grounding in problematic ideas about evidence, show the even more problematic assumptions underlying the evidence-based policy movement. It is a problem of serious social relevance, and one that the model of inquiry that we’ve been discussing is particularly well-suited to address.

### 3.4.1 “Evidence”-Based Policy

Evidence-based policy (EBP) is a fast-growing movement in public policy, especially in the areas of medicine and education. Already, plenty of government funding, hospital policies, and educational mandates hang on the existence of certain kinds of evidential standards. In practice, this means that policies are funded or

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<sup>20</sup> In my view, *the* crucial problem with Richard Rorty’s version of pragmatism is his seeming inability to recognize this particular fact.

approved on the basis of whether there exists evidence for the policy that ranks highly on one of the prominent evidence-ranking schemes, such as SIGNS (the Scottish Intercollegiate Guidelines Network) and the “What Works Clearinghouse” of the US Department of Education. These schemes inevitably put randomized controlled trials (RCTs) at the top of the list, and things like case studies, ethnographic studies, and expert opinion either aren’t mentioned or are ranked very low (Cartwright and Efstathiou, March 2008).

Philosophers have raised a variety of objections. For example, John Worrall has argued that EBP has overestimated the value of RCTs and underestimated the value of expertise, because it pursues an unrealistic strategy of attempting to eliminate alternative explanations without making any judgments of what counts as a *plausible alternative* (Worrall, 2002). Nancy Cartwright has argued that EBP lacks justification because we lack any “reasonable and practicable” theory of evidence that could do the work EBP requires. The standards in place, on her view, are too restrictive, make plain wrong claims about strength of evidence, and provide no useful information about combining evidence. (Cartwright, 2007). The standard rankings also evaluate soundness about evidence without providing information about whether the evidence is sufficiently *relevant* to the policy in question (Cartwright, forthcoming 2009). These are just two examples of prominent criticism, and once one gets into the details for particular areas of medicine or education, criticisms of such standards are legion.

What can we say about EBP from the point of view of the inquiry-model? As everyone in the discussion is quick to point out, *of course* basing policy on evidence is a good thing (though one often wishes this view were more prominent or consistently held amongst politicians). Nevertheless, the particular way that evidential standards have been drawn up suffers from the same worry that many of the discussions of evidence do, namely, it attempts to short-cut the need for research with an easy answer. Understanding this point of criticism in the policy case requires that we

shift how we think of the nature of policy-making. This shift came naturally for Dewey but may seem quite unusual today.

The most common model of the relation of science to policy conceives of them as very different pursuits that become related in the course of policy-making by a two-step process. Science in this model is understood as the generator of *information* about the world. Policy is conceived of as the process of deciding on *goals* and choosing ways to execute those goals. (For example, in the policy process, we decide that we want to focus on better education, and we pass initiatives aimed at improving education.) In the first step of the interaction between science and policy, policy-makers approaches scientists with a *query* for information that is needed in order to assess alternative policies for meeting a goal. In the second step of the process, science *responds* with relevant information, either by consulting accumulated knowledge or by performing new studies. It is a single-channel process, in which questions flow one way, and information flows the other.

This *query-response* model is highly idealized in several ways, but it nonetheless underlies much thinking about evidence in policy. EBP is meant to do two things, then: it is meant to make the query-response process *mandatory*, and it provides a standard of evidence *for policy* to assess the quality of evidence provided. In the real world, the sources of evidence are more diverse and complex than some monolithic Science, running the gamut from publically-funded research to corporate R&D, and the policy process itself is complex and adversarial. Since representatives of science *tout court* rarely convene to provide an univocal answer to policy queries, some standards for assessing evidence in favor of competing proposals seems necessary.

Let's consider an example. Suppose that standardized testing finds that math scores are down on average from a decade ago. Once this result is made known to the public, an outcry for action leads policy-makers to make improved mathematical aptitude a key goal for immediate action. Several proposals for action are brought up

by interested parties. One suggests revising the math curriculum to provide a heavy emphasis on set theory before teaching arithmetic, another for a curriculum that enriches traditional math education with reading stories and making art projects, and a third which advocates cutting class sizes in half. Now we must query the evidence. Suppose that each of these proposals has theoretical justification, expert testimony, and anecdotal evidence in its favor. The second proposal, however, can show that a randomized control trial covering math classes in two middle schools in North Carolina showed significant improvement on test scores for the new math-art-stories textbook versus the older textbook, which focused solely on practicing math skills by working problem sets. An RCT is graded as very “high-quality” evidence, and so the second proposal, according to EBP, is the policy that will be enacted, even if the other, “lower-quality” forms of evidence favor the alternatives.

The attraction of such a standard is obvious, as it makes the difficult process of weighing evidence susceptible to a fairly simple algorithm. But its sheer simplicity is cause for alarm. Consider an analogous case in scientific inquiry: when a controversy between two competing theories or explanation is in process, with each side marshaling evidence in its favor, the question cannot be answered, and inquiry brought to a close by the application of an algorithm. As we’ve seen, this is a question that requires *further inquiry*, and short-cut solutions won’t do the job.

The shift that ought to be made in our understanding of policy in order to avoid the false certainty of a short-cut solution is to regard *policy itself* as an inquiry,<sup>21</sup> different in many ways from scientific inquiry, but inquiry nonetheless. Already the inquiry-model requires that we regard science as something other than a mere accumulator of impartial information; rather, science is a problem-solving process that attempts to resolve a variety of perplexities, from the mundane and practical to the abstruse and distant from immediate application. The policy-process itself can be profitably understood as one of identifying and attempting to resolve

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<sup>21</sup>See Kaufman-Osborn (1985); Caspary (2000).

social problems of a certain sort. When low test scores in mathematics appear, a problem is set, and the policy response is an attempt to resolve that problem.

If public policy is just a kind of *social* inquiry, and inquiry is understood according to the inquiry-model, this has radical implications both for the relationship between science and policy and how we understand and undertake policy-making itself. It requires us to see policy problems as open to restatement and reinterpretation. They are no longer set by the agendas of politicians, by political ideologies, or by philosophical notions of the major functions of the state; rather, they are set by the concrete tensions and disturbances in matters of public concern that must be resolved in order to stabilize public life. Much vitriolic political debate can be seen as a disagreement over the *nature* of the problem in question; for example, in debates over science education, in teaching areas of research like evolutionary biology that are publicly controversial, one side frames the problem as one of airing both sides of a controversial issue and allowing students to form their own opinions, while the other side frames the problem as having to do with communicating an *accurate* picture of a scientific field at a time, in which there is no significant controversy. It sometimes seems to be an irresolvable ideological struggle, but the inquiry-model would have us recognize two things. First, each way of framing the problem captures something important and valuable: hearing multiple perspectives on an issue and coming to one's own decision is genuinely valuable, but so is having an accurate picture of a scientific field. Second, how one frames the problem or categorizes the issue should itself be responsive to evidence and ultimately, even one's own favored interpretation is open to rejection and revision if the problem proves insoluble in those terms. In the case at hand, the evidence shows that the locus of controversy is not amongst biologists with the relevant expertise. Rather, it is about the foundations or scope of biology, the applicability of the methods of biology to certain questions, or about the relationship between mechanical and teleological problems at a philosophical level. One can accurately communicate the state of the field while simultaneously airing



the relevant controversy by having a discussion at the level of history and philosophy of science.<sup>22</sup>

On the inquiry-model, a general commitment to “evidence-based policy” is a no-brainer. Obviously policy, like any inquiry, *must* be based on evidence. But evidence doesn’t come pre-packaged by other areas of inquiry. While the warranted conclusions of other inquiries provide *prima facie* materials for further inquiries, the adaption of evidence into different contexts is never automatic, nor can pre-existing evidence be expected to be sufficient for resolving an inquiry. New evidence must be gathered on the problematic situation *at hand*, on the basis of the current perplexity. The relevance of old evidence must be determined by attempts to coordinate it with an understanding of the problem and proposed solution and on the ability to generate further evidence on that basis, congenial to solution. The validity of the evidence in a new context is always in question, susceptible to revision or rejection as the inquiry moves forward.<sup>23</sup> Ultimately, *policy itself* must be understood *not* as a final answer, but as itself an experiment which must be approached tentatively and taken as provide evidence about the adequacy of a proposed solution.

The inquiry-model poses a tough challenge for both public policy and public relations with science. If I am right to treat public policy as a type of social inquiry, it makes the jobs of policy-makers and scientists even more difficult. Scientists can no longer hold insular committee meetings under the auspices of the National Academy of Sciences in order to hand down pronouncements of the scientific evidence (See Beatty (2006)). Policy-makers can no longer cherry-pick scientific reports to bolster their proposals. Rather, policy-makers must become more like scientists, in that they will have to involve themselves in the direction of evidence-gathering and the assessment of evidence, as well as creative problem-solving, and scientists and policy-makers will have to work together in coordinating inquiries that can solve social

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<sup>22</sup>I don’t pretend that this adequately solves the problem; rather, it is meant to challenge the supposed either-or situation of the policy debate.

<sup>23</sup> Cf. Bryan Norton’s discussion of risk assessment in (Norton, 2005).

problems. While this is a difficult request indeed, it is also true that if science itself is any guide here, there are no easy ways out. The cookie-cutter, formal procedures for evaluating evidence that currently inform public policy and the abstract schemas for handling evidence still popular amongst philosophers try to provide quick solutions to problems requiring a long process of research. If we want policy-making to be evidence-based and intelligently guided, then we have to take the hard road and treat it as a type of inquiry that must be governed by lessons learned about the process of inquiry generally.

As Dewey wrote in the concluding paragraph of *Logic: The Theory of Inquiry*:<sup>24</sup>

Failure to institute a logic based inclusively and exclusively upon the operations of inquiry has enormous cultural consequences... Since scientific methods simply exhibit free intelligence operating in the best manner available at a given time, the cultural waste, confusion and distortion that results from the failure to use these methods, in all fields in connection with all problems, is incalculable. (LW 12: 527)

In Dewey's era, the understanding of logic and reasoning that most informed education and public discussion was Aristotelian with a smattering of formal logic. While today we have the benefits of statistics and some of the methods of science, we have yet to take up the deeper lessons of science. We can see this in the dogmatism which informs much of politics, and in the urgently felt need for an "evidence-based policy." Overcoming these problems requires more thoroughly assimilating the lessons of experimental science into the wider scope of human life.

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<sup>24</sup> Expanding the epigraph of this chapter

## Chapter 4

# Genuine Problems and the Significance of Science

### 4.1 Introduction

Toward the end of the last chapter, we discussed both the problem of evidence-for-use and the problems surrounding so-called evidence-based policy. These can both in some sense be regarded as discussions of *scientific* constraints on policy, the way that scientific evidence should (or should be formulated in order to) inform our policy decisions. This chapter will turn now to the social and political constraints on the operation of science. Modern science is a large-scale social and institutional endeavor, and in order to understand it, we need to understand its role within society and amongst our political institutions. What will be the research agenda for science? How should we distribute funding amongst potential and ongoing scientific projects? How should science be arranged in order to be just? What are the social and political responsibilities of scientists *qua* scientists?

To many scientists and philosophers of science, these questions will seem inappropriate. It has been a widespread belief that science is an essentially value-free

activity, especially in philosophy of science after World War II.<sup>1</sup> When it functions well, it provides for us a store of objective truths. When moral, political, and social values enter in, they are essentially corrupting—Lysenkoism is a stock example. Technology, on this common view, is just the application of science and instrumental rationality towards some goals—while values enter in, it is only as goals set from the outside. This view is mostly shared both by the boosters and debunkers of science, differing largely over whether actual science manages to live up to this idea or whether science has become “corrupt.”

It is becoming harder and harder to deny that values play an essential role in science, and that science—at least science for humans, as it actually exists—is essentially a social activity. At the same time, many people now argue that this need not threaten the value of science. A growing number of philosophers are attempting to craft a new image of science, in which the role of values of science are not corrupting, in which they might even play a positive role. In such efforts it is common to explore the social nature of science, determine the proper relation of science to democracy, and problematize the simple dichotomy between science and technology. John Dewey also rejected the traditional view that was already entrenched in his time of science as value-free, and unlike many present-day philosophers who do so, he was not trained under the subsequent tradition that regarded science as essentially value-free. His work provides a useful starting point for this kind of work, in part because Dewey does not face the threat of falling into old, bad assumptions about science. I hope to start from a position free from the mistaken assumptions and false starts of the tradition in philosophy of science.

In this chapter, I will analyze the recent work by Philip Kitcher in which he works towards such an image. Kitcher has in recent years begun to draw on a variety of pragmatist ideas and espouse some distinctively pragmatist views. While I don’t believe Kitcher has any desire to be an orthodox pragmatist or Deweyan as such, it is

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<sup>1</sup>See Ch. 1; Reisch (2005); Richardson (2002, 2003); Howard (2003, 2007)

still fitting that we consider him in the course of understanding Dewey's philosophy of science, as well as using Dewey to evaluate Kitcher's own pragmatist-leaning ideas.

Kitcher's *Science, Truth, and Democracy* (2001) sets forward a two-part theory of the relation of science to democracy and the social, political, and moral constraints of science: First, he provides an argument for viewing science as context-dependent but nonetheless objective, in which the concept of *scientific significance* plays a major role. Scientific significance is supposed to capture the knowledge that a certain scientific community or discipline has about what areas of research are significant. Second, this context-dependent representation of scientific significance is used as an input to an ideal democratic deliberation procedure—in which ideal representatives of the preferences of citizens deliberate and attempt to reach consensus—in order to determine the ideal research agenda for science (in our liberal democracy). He calls this ideal “well-ordered science.” The philosophical-epistemic story about what is significance about science is thus a first step in a social-political ideal of science. This idea is useful, e.g., in funding decisions and decisions of individual scientists in what research to pursue, because we can compare the actual situation and future options to the ideal.<sup>2</sup>

I'm going to focus in on the first part of the story, the account of scientific significance. This paper will challenge, and attempt to improve on, that account, and then trace briefly the consequences for the relation of science to democracy. The main challenge is that Kitcher's account of significance leaves out too many of the concrete features of the contexts that give science its significance. Kitcher captures some of the conditional or relational components of what makes certain scientific pursuits or claims significant. I argue that these are not enough, however,

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<sup>2</sup>Or, since Kitcher's procedure doesn't produce an *actual* research agenda, but merely points to the kind of procedure that would produce one, what Kitcher actually offers is a ground for arguments about what would or wouldn't be on the agenda. The judgments one would be able to make would necessarily be fairly coarse (e.g., pursue research on third-world disease rather than more advanced liposuction techniques).

and that he leaves out components of significance that are immediate or inherent in the practice itself.

John Dewey's pragmatist theory of how problems arise and spur inquiry provides part of the missing story. By analyzing how problems arise from concrete situations, by understanding when and how such problems are genuine, we can also get a better picture of how significant they are. Dewey said that it was the neglect of the "context which controls the course of thought" (*Context and Thought*, LW 6:6) which was the most serious and pervasive fallacy of philosophy. Kitcher does much to avoid the problem, but not enough. I will try to take the project one step farther.

## 4.2 Why Significance?

Kitcher is not the first or the only philosopher of science to have searched for an account of the significance of science. The positivists searched for criteria of "cognitive significance" that would rule out all non-scientific statements as meaningless. More recently, Joseph Rouse (1996) has argued that an account of the significance of scientific practices ought to be a category at the forefront in science studies, helping analyze every level of practice (pp. 25ff). Kitcher's project, like Rouse's,<sup>3</sup> aims to answer a range of questions, some of which were traditionally filled by less modest traditional notions such as "objectivity" and "objective explanation." Kitcher's project also bears similarities to Larry Laudan's analysis of the evolving aims of science in *Science and Values* (1984).

One way Kitcher motivates the need for an account of "significance" over and

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<sup>3</sup>Kitcher fails to cite Rouse, but their way of putting the problem is uncannily similar. A likely common source is Popper (though only Kitcher cites him as a source of his ideas on significance, in the bibliographic notes to chapter 6). Rouse seems much more keenly aware of the specific issues of scientific practice, paying keener attention to practice than Kitcher does. Kitcher, as we'll see, tends to fall into many traps that Rouse does a better job avoiding.

above mere truth is by an analogy to maps. One might imagine that the ultimate goal of cartography is the production of an ideal atlas, a set of maps which can be used to serve any purpose. Kitcher thinks the possibility of such an atlas that is sufficiently comprehensive and practically useful is absurd: “There is no good reason to believe in the ideal atlas” (60). The wide variety of actual and possible aims served by map-making, the competing constraints, the need for selectivity in crafting useful maps, and the finitude of resources, casts doubt on the realizability and even the coherence of an ideal atlas. The only map with sufficiently rich information for all purposes of the territory itself; but the territory itself is not a map at all.

To see why this is so, consider three different maps: a topographic map, an electoral map, and a subway map. The topographic map contains many geographical and geological features, and is especially informative about changes in elevation. On the other hand, one would have a difficult time navigating a city based on a topographical map, since so little of the available information is relevant. An electoral map—of the sort so many of us were obsessed with throughout the latter half of 2008—contains precious little in the way of the information on the topographic map. There are no roads, no landmarks, no cities; no changes in elevation, rivers, or lakes. About all the map shows are political divisions—states, counties, districts—and the predicted or actual pattern of voting within those divisions. A subway map bears some resemblance to an electoral map: its geographic features are distorted, it contains little information about streets or natural landmarks. These simplifications are necessary to the effectiveness of the map, and even the basic relations of north-south, east-west are optional (and sometimes left out in the ones on the subway car itself). These examples give a clear picture of the way in which constraints of map-making compete, and how intimately tied up they are with our purposes. These capture just three of a potentially infinite variety of maps serving our potentially infinite variety of purposes (to say nothing of things like star maps and maps of abstract spaces).

So we must understand maps as representing territory in a way that picks out the significant features for *our* particular purposes. As it goes for maps, so too for science. Science does not merely seek truth, but *significant* truth. Mere truth is no good: most truth is uninteresting (the infinity of truths about the contents and arrangement of my office over time, for example), and some of it is unwelcome or dangerous. What we want is *significant* truth, the significance of which, Kitcher argues, is highly contextual and interest-relative: “[W]hat counts as significant science must be understood in the context of a particular group with particular practical interests and a particular history” (61).

It is important to point out what significance is and is not supposed to capture for Kitcher. It does not provide an answer to the old “demarcation problem.” Whether some truth or some line of inquiry counts as significant is not meant to tell you that it is or is not “scientific.” I will follow Kitcher and presume from the outset a rough-and-ready understanding of what science is, and that we are talking about the sciences already. What this talk about “significance” is meant to capture is the relative importance of different parts of science. For Kitcher, this importance must be understood in terms of *our* goals, purposes, and interests. Further, though everyone should recognize that *practical ends* play an important role in attributing significance to certain scientific projects, what is needed is a portrayal of a goal that is distinctively *epistemic*.

The objectivist or strong realist might try to avoid Kitcher’s move by seeking an objective goal for science. They would thus account for which truths are scientifically significant in a context-free way, as being those truths that contribute to the objective goal. Kitcher considers several traditional views on the epistemic or theoretical aim of science, including identifying laws of nature, providing a unified account of nature, or discovering the fundamental causal processes (66). Each of these fails, because of the difficulty of answering for each, “What would be so valuable about gaining that?” (66). Once we rule out practical and theological justifications,



it is hard to find any justification for these goals. According to Kitcher, the most promising traditional view is: “The (epistemic) aim of science is to achieve objective understanding through the provision of explanations”(66). Objective understanding in this sense is *not* based in the activity of explanation that responds to actual questions, but is the recognition of whatever special facts or relationships exist that *grounds* particular explanations (if they are genuine or objective).

The reasons that this view fails are neither subtle nor complex. We are seeking an understanding of scientific significance that will help us pick out important parts of science from the myriad of banal facts. Thus, the aim of science, if it is to be “an all-purpose explanatory device” that is context-independent, it must be systematic. It will fail “if it is simply a long list of potential explanations, one for each context”(68) because then it will fail to sort the epistemically significant from the significant, including everything somewhere on the list. The easiest way to guarantee this sort systematicity is to defend some sort of Unity-of-Science view,<sup>4</sup> in which intertheoretic reduction of some sort could be attained between the various special sciences, including definitions that could link the diverse vocabularies of the various disciplines (69). The failures of these views is familiar: the successful cases of reduction from which the movement drew inspiration were of a fairly limited class involving individual or small clusters of laws, whereas it is difficult to imagine that much of biology or psychology could take this form; there is much science that has little or nothing to do with general laws at all (69); linking definitions between theoretical vocabularies seems a near-impossible goal for disciplines like psychology (69); the crucial features of many sciences involves “the form of [the] processes, not the material out of which the things are made”(70-1), and these forms are quite diverse and explanatory, in many situations in which a reductive explanation would have zero explanatory power; consider Kitcher’s example of trends in the number of

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<sup>4</sup>Kitcher’s discussion of the “Unity-of-Science movement” may depart drastically from the actual historical movement headed by Otto Neurath. See Reisch (2005) and Cartwright et al. (1996).

births of males versus females: an “explanation” in terms of the psychiochemical basis of this trend would not advance our understanding at all, whereas a non-reductive explanation in terms of selection pressure would be much more helpful. Just as the idea of an ideal atlas to serve all possible cartographic purposes is untenable, so too the Unity-of-Science view fails.<sup>5</sup>

One might argue that the failures of this view, instead of signaling the impossibility of objectively sorting significant from insignificant truths, merely shows that it is an open project for philosophy of science to discover what notion of objective understanding will serve the purpose (73).<sup>6</sup> But again, this approach will fail if all kinds of “mundane truths” are counted as significant (73). It won’t be the case that everything in the store of information in which objectively complete answers lie, the store that picks out the truths that are significant, will be relevant to *any* question, because of the failure of the Unity-of-Science view. So we will need a way to filter just the truths that are “pervasive” (but not completely so) from the banal (74). One possibility is to say that whatever truths play a role in a *complete causal narrative* of an event are the objective explanatory resources for the event, but this fails because often the causal history doesn’t give us the explanation we need (as in cases in which

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<sup>5</sup>One may find fault with Kitcher’s characterization of the Unity-of-Science view, or find his criticisms lacking. There are certainly many careful and sustained critiques of the idea. Kitcher refers the reader to Fodor (1974); van Fraassen (1980, 1989); Kitcher (1984); Dupré (1993); Cartwright (1999). Dewey’s own critique and re-interpretation of the Unity-of-Science (as an anti-reductionist opposition to supernaturalism) can be found in his contribution to Volume I of the *International Encyclopedia of Unified Science*, “Unity of Science as a Social Problem” (LW 13:271-280). Paul Feyerabend was widely critical of such a view, arguing for the necessity of metaphysically and conceptually incommensurable theories and an antagonistic theory of scientific progress along the lines of Mill’s *On Liberty*. See also Galison and Stump (1996).

Despite the difficulties of the issue, I suspect a simple argument will do here. We ought to believe that science *actually* provisions explanations, and that we are currently able to make reasonable judgments about significance. However, science at present does not form the unified edifice dreamed of by the Unity-of-Science movement, nor does it even approximate it. Therefore, any explanation of the significance of science applicable to science *as it actually exists at present* cannot depend on this far-off ideal of the Unity-of-Science.

<sup>6</sup>This is one way to understand much recent work on “models” and “mechanisms” which treat those as general schemes of scientific explanation.

structural features or equilibrium conditions do the explaining) (74), and indeed any truth will figure in *some* causal narratives (74-5). One could try to solve the filtering problem by *counting* the number of explanations that each truth might play a role in, but this will fail because any true statement will figure in an infinity of possible explanations (continuum many, if time is continuous) (75).

The general problem here is that our actual, everyday explanations are quite heterogeneous both in the questions they answer (not just “Why?” but also “How?,” “What?,” “How is it possible?,” etc. (73)), in the kind of information (causal or otherwise) that they rely on, and in what determines what is relevant to that explanation. Explanation is a task that is too context-dependent to be given a context-independent foundation. It is not that there is no such thing as objective explanation (in line with the ideal of objectivity that Kitcher pursues in Chapter 3):

Objective explanation goes on in the sciences, then, but only against the background of our questions and our interests. The most we can expect from a theory of explanation is some understanding of how these questions and interests shift as our inquiries, and the complex environments in which they occur, evolve. (75-6)

Hence the need for a theory of the significance of science: we want to know the aim and importance of inquiry; “discover truth” will not do, as most truth is banal and insignificant; none of the accounts in terms of laws, causes, unification, or objective explanation that is free from considerations of context and interest will do; thus, we need to understand how our questions and interests, both practical and theoretical, work to pick out certain things as significant.

## 4.3 Kitcher's Theory of Significance

### 4.3.1 Significance Graphs

Kitcher's explanation of how elements of science count as significant proceeds from his insights into the complex interconnectedness of science. We are naturally interested in a number of broad questions, such as "What were our hominid ancestors like?" and "How do single-celled organisms regulate their metabolism?" (76). In addition, much of science is concerned not with general laws or broad questions, but with rather narrow issues and very particular results (76). Large projects and more mundane accomplishments are interconnected (76-7), but the flow of significance should not simply be seen as going from the theoretical top to the particularistic bottom (77). Epistemic and practical interests are interwoven (76). So a treatment of significance ought to provide a picture in which "the connections that confer significance seem to radiate in many different directions" rather than being a simple hierarchy (77).

Kitcher uses an apparatus he calls "significance graphs" to capture the way that different parts of science get their significance. They are directed graphs that show connections between the research projects, questions, problems, claims, techniques, parts of the natural world, and practical goals dealt with by a scientific field. (See Figure 4.1 for a toy example which traces the significance of some areas of thermodynamics.) Significance graphs display the ways in which particular scientific efforts come to inherit significance from other projects. They are indexed to a particular time and will change dynamically as the field in question develops. The significance graph is meant "to make explicit what workers in the field know at the time" (78); they are part of what we might call the "disciplinary matrix" of the field.<sup>7</sup>

Notice that the information these significance graphs capture is relational or conditional. These graphs trace the ultimate source of significance to either practical

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<sup>7</sup>Following Kuhn (1970, p. 271).

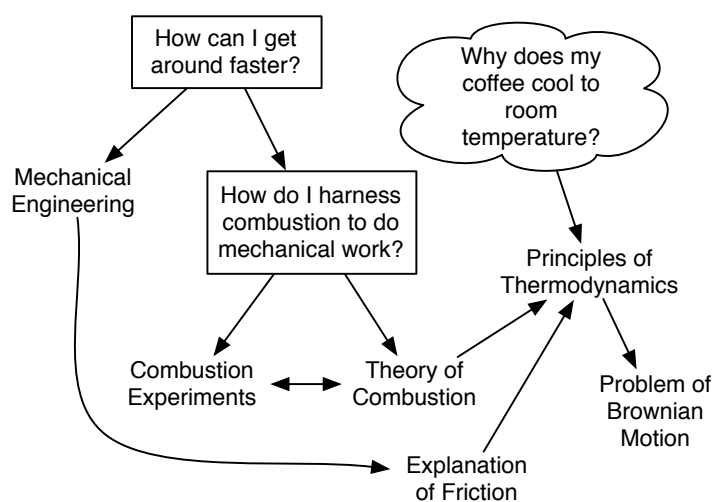


Figure 4.1: **Toy Significance Graph.** Practical goals are outlined in square boxes, and questions about which we might be naturally curious are outlined by clouds (my addition). (See Figures 1 & 2 in Kitcher (2001, pp. 79-80) for more detailed examples.)

questions (boxes) or questions that stem from “natural curiosity” (clouds). Everything else has significance *only derivatively*, via an inheritance arrow drawn to it from a practical concern, a natural question, or another scientific concern. There are many ways the inheritance can work: a more technical question must be answered in order to help answer a larger question; data can serve as evidence for a claim; new experiments are suggested by a comprehensive theory. Not only does the explanation of scientific significance by way of significance graphs account for all the insights and concerns above, but the significance-graph framework also takes into account the variety and the complex interconnections of scientific activities, and the fact that significance is dynamic and historically situated.

### 4.3.2 Problems for Kitcher's Theory

This is a weak peg on which to hang the significance of science.<sup>8</sup> Remember, while Kitcher is committed to practical and epistemic sources of significance being interwoven, he wouldn't want to reduce all significance to practicality. The only other source, on Kitcher's account, is the contribution of "natural curiosity." Remember now the test that Kitcher applied to other candidate accounts of the aim of science. When we ask of one of these "natural" questions, "What would be so valuable about knowing that?" Kitcher has little to say. He insists that

Human beings vary...with respect to the ways in which they express surprise and curiosity...But...we do count some of our fellows as pathological, either because they obsess about trifles or because they are completely dull. In claiming that *sciences ultimately obtain their epistemic significance from the broad questions that express natural human curiosity*, I am drawing on this practice of limited tolerance, on our conception of "healthy curiosity" ... (81, my emphasis)

This story about "the ultimate source of epistemic significance" he says, is "commonplace and disappointing to those who expect a grand theory that will invest the sciences with overriding importance" (80).

But not only is it commonplace and disappointing to those who exalt science unduly, it is difficult to see how the significance of scientific projects, even on a modestly pro-science account, can have its source *entirely* in practical questions and curiosity. Are the questions and projects at the bleeding edge of science all ultimately of interest only through the practical projects they might relate to and the very general questions about which we are "naturally curious"? If epistemic significance comes down to a purely subjective feeling of curiosity, natural though it may be, the whole project of distinguishing scientific from mere utilitarian significance hangs on just a feeling. By pushing back the explanation to items of *natural curiosity*,

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<sup>8</sup>P.D. Magnus has raised a variety of related problems for Kitcher's use of "natural curiosity" in an unpublished manuscript.

his account of significance hangs on a claim that I find doubtful: that people will, *without further reason*, agree on a broad swath of what they find interesting. Put another way, Kitcher's ideas will radically underestimate the significance of many projects in science. The mere fact that some or even many people feel a bit curious about some topic counts for very little in the face of our pressing needs. Practical significance will undoubtedly wash out the effects of curiosity.

Remember also that Kitcher's criticisms of traditional accounts of the context-independent aim of science turn on their inability to answer the question "What would be so valuable about gaining that?" But could they not answer in the same way that Kitcher does? Why can they not simply reply that those who cannot see the inherent value in such pursuits are dull and incurious? If the answer is not satisfactory in their case, it will not work in Kitcher's, either. A related and more familiar situation might be trying to explain the significance of technical work in philosophy by referring to general questions that people should *obviously* be naturally curious about, like "What is knowledge?" or "How are scientific concepts related to the world?" I have found that in the face of such claims, many people remain pretty unimpressed. Perhaps most of the non-philosophers I know are just dull, but the suggestion is at least impolite and at worst overwhelmingly elitist—a bad start for an attempt to communicate with laypersons about the significance of science.

What's more, Kitcher also underestimates the potential for idiosyncrasy of curiosity. A significance graph crystallizes the implicit knowledge *of a discipline* as to what is significant in that field. The broad, "natural" questions in their significance graph then need not necessarily be natural for everyone. The questions that drive my basic curiosity might only be "natural" for people like me in certain respects, and that respect might be what draws people to say, physics, but not to microbiology. The questions that most physicists are "naturally" curious about might be quite idiosyncratic. For example the microbiologist and nobel laureate Salvador Luria (1984) "confess[es] a lack of enthusiasm. . . in the 'big problems' of the Universe or of

the early Earth”(119).<sup>9</sup> The questions of supposed “natural curiosity” which drive astronomy, physics, or even much of biology would be of little interest to Luria, as compared with the concrete problems facing microbiologists, about which it is possible to make obvious progress. In such a case, Kitcher will either devalue the field (who cares what those physicists are curious about?), or become an elitist (such that only physicists determine or have access to whether their projects are significant), which goes against his attempt to subject scientific aims to what ideal democratic layperson-deliberators would choose. Of course, there is no reason that curiosity can’t or shouldn’t play a role in attributing significance; but it is inadequate to carry the whole project.

In the face of all of these problems,<sup>10</sup> I suggest an alternative approach, based on the pragmatist views of Peirce and Dewey. Their theory of inquiry can help us further understand the problems with Kitcher’s theory, as well as pointing the direction towards fixing it.

## 4.4 The Pragmatist Model of Inquiry

It is worth reviewing some key features of the pragmatist theory of inquiry developed in chapter 2 at this point, with an emphasis on those features of the theory relevant to the question of the significance of science and the value of various lines of inquiry. I will begin with Charles Saunders Peirce’s somewhat simpler way of putting the point, by way of introduction, before explaining Dewey’s view and showing how it should be developed. This requires a divergence from addressing the main topic—significance—to which I will return in the following section.

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<sup>9</sup>Quoted in Feyerabend and Terpstra (2001, p. 148) and Feyerabend (1988, p. 35)

<sup>10</sup>Further problems arise when one attempts to use Kitcher’s analysis of significance for Kitcher’s own project of reconciling science and democracy. See Simon (2006).



### 4.4.1 Peirce's Insight

One of the founding insights of C.S. Peirce's pragmatism was his analysis of the structure of belief-formation,<sup>11</sup> which provides an important distinction between *genuine* doubt that leads to inquiry and new belief, and "*paper*" doubt that is often used for pernicious philosophical purposes. The basic idea is the familiar difference between the experience of an actual, pressing problem or a real, nagging uncertainty, versus the posing of an idle question, the seemingly silly questioning of what's obvious without any *reason* behind it.<sup>12</sup> Peirce claims that all inquiry begins with genuine doubt. One way Peirce offers for understanding genuine doubt is by contrast to the methodological doubt of Descartes. Such doubt is complete and schematic, and, Peirce thinks, feigned. According to Peirce, this method is fruitless, because *genuine doubt* requires more than just putting a question on paper:

Some philosophers have imagined that to start an inquiry it was only necessary to utter a question whether orally or by setting it down upon paper. . . But the mere putting of a proposition into the interrogative form does not stimulate the mind to any struggle after belief. There must be a real and living doubt, and without this all discussion is idle. (EP 1: 114-5)<sup>13</sup>

What distinguishes genuine doubt is that, first, it must be felt or experienced. The feeling that Peirce talks about is variously characterized as one of unease, surprise, or novelty. It is an experience that breaks up old beliefs and habits, and leads one to struggle after new beliefs. Without these new beliefs, one is unable to move forward. A simplistic example is coming to an unexpected fork in one's path through the woods. You are surprised, and perhaps uneasy about which way to go. You must

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<sup>11</sup>The *locus classicus* being his 1877 series of essays in *Popular Science Monthly*, especially "The Fixation of Belief" (Peirce, 1877).

<sup>12</sup>The lack of a good reason for doubt is crucial to the idea of a paper doubt. Part of what Peirce and Dewey are after is an explanation of what counts as such a reason.

<sup>13</sup>"The Fixation of Belief" (Peirce, 1877). Citations of Peirce will refer to Peirce et al. (1992) according to (EP *volume:page*), and citations in this chapter refer to Peirce (1877) unless otherwise noted.

settle at least on a tentative belief about which way to go before you can move forward.

Peirce argues that the formation of *all* beliefs has a complex logical and temporal structure, and no belief can arise immediately.<sup>14</sup> Peirce's model, which we might call the doubt-belief model of inquiry, proceeds from *genuine doubt* into *inquiry* and finally to *settled belief*. This is the core of Peirce's theory of inquiry. Genuine doubt must precede (or be an early stage in) any genuine inquiry. The temporal development of inquiry, when it is successful, moves away from this doubt and towards some resolution.

#### 4.4.2 Dewey's Elaboration of the Model

John Dewey takes up Peirce's line of thought in his own writings on logic and inquiry:

The function of reflective thought [i.e., inquiry] is . . . to transform a *situation* in which there is experienced obscurity, doubt, conflict, disturbance of some sort, into a situation that is clear, coherent, settled, harmonious. (*How We Think*, Rev. Ed., LW 8:195, my emphasis)

The affinity with Peirce is clear, in that inquiry takes us from a situation that is (among other things) doubtful to one that is settled, but Dewey elaborates and transforms Peirce's insight. The most crucial transformation is from Peirce's terminology of mental-states like doubt and belief to Dewey's discussion of "situations." A situation is not merely personal and subjective; it includes the whole person *or group of persons* and the constituents of their environment relevant to the inquiry or practice at hand.<sup>15</sup> Problems do not arise as purely intellectual matters, but rather due to "incidents occasioning an interruption of the smooth, straightforward course of behavior and that deflect it into the kind of behavior constituting inquiry" ("Reply

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<sup>14</sup>Belief here should be understood in its dispositional, not occurrent sense.

<sup>15</sup>For a discussion of the non-subjective, non-mentalistic nature of a situation, see chapter 2.6.

to Albert G.A. Balz,” LW 16:282). An indeterminate or problematic situation for Dewey is a “breakdown” of practice, as it is for Heidegger, and in both cases it is what makes reflection and knowledge possible.<sup>16</sup> The unit of analysis is not the *mind* but *behavior* or *practice*.

Consider a common situation in medical practice. A patient comes in showing familiar symptoms, and the physician prescribes the usual antibiotic. If everything works out fine, the smooth course of behavior continues; there is no “inquiry,” properly so-called. On the other hand, if the antibiotic doesn’t seem to work,<sup>17</sup> there is a disruption of the habitual course of activity. As the physician, looking at what you have in front of you, it isn’t clear which way to go, what the features of this situation signify. You must dig for more evidence, consider alternative explanations, and try to sort out what to do before proceeding with a course of treatment.

A second difference is the phenomenological richness of the terms of Dewey’s account, the elaboration of the qualities that characterize the initial and final moments of inquiry. Thought begins with a situation that is obscure, doubtful, conflicted, disturbed, etc., and it terminates when the situation attains clarity, coherence, settledness, harmony. What Dewey provides here we might call an *aesthetics of logic*, an analysis of the nature and role of qualities in the production and guiding of inquiry.<sup>18</sup>

Unlike Peirce’s terminology, which, despite Peirce’s own understanding of terms like “belief” and “doubt” in terms of habits and practices, connote subjective, individual mental states, Dewey rigorously avoids presupposing fixed dichotomies of mind/body and individual/world in laying out his phenomenological description of problems and inquiry. Dewey considers all human activity to be a species of embodied life-activity, in which an organism is always engaged in transactions with an environment. In this situational picture, qualities of the situation like “doubtful”

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<sup>16</sup>See Koschmann et al. (1998).

<sup>17</sup>And such failures aren’t a familiar occurrence for which there is another immediate response.

<sup>18</sup>This is especially developed in Dewey’s essay, “Qualitative Thought” (LW 5).

or “indeterminate” describe the transactions between organism and environment, the particular character of the goal-directed, situated activity of an embodied creature. “We are doubtful because the situation is *inherently* doubtful” (*Logic*, LW 12:109, my emphasis). The indeterminacy of the situation is not *merely* a subjective feature, but rather an objective imbalance or disequilibrium in the organism/environment system. Subjective states of doubt that are not evoked by a “doubtfulness” or instability in the situation are pathological (*ibid.*).<sup>19</sup>

Clearly practical problems—in the narrow sense of problems of immediate use and enjoyment—are one kind of genuine problem, at the extreme of immediate needs and direct applicability. Natural curiosity might be at another extreme: curiosity need not be feigned or idle; we may be genuinely interested in some general and basic questions at quite a distant remove from any applicability. But these questions seem to lie at the intellectual extreme of doubts and problems, and they threaten to become mere “paper” doubts if they become entirely cut off from practice in the broader sense that encompasses all of our activities. The bulk of the problems of science lie between these extremes. They are not concerned with immediately practical issues, although they are systematically and deeply related to a variety of practical issues. They also have a significant intellectual component, but are not concerned with the general and basic questions of common sense; rather, they arise in the practice of working out and developing theoretical frameworks and abstract models, observational and experimental techniques, and the interaction of the two.

For example, Einstein’s theory of General Relativity seems to make the surprising prediction of gravitational waves (similar, in some way, to electromagnetic waves).<sup>20</sup> This spawns both theoretical inquiries—trying to determine whether the prediction really follows from the theory, and what expectations to have about them—and experimental inquiries, about how to detect these hitherto unknown

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<sup>19</sup> Both Peirce and Dewey think it is a characteristic of the scientific attitude to seek out problems, not merely passively wait for them to occur. See Bernstein (1966, p. 105) and Browning (1994).

<sup>20</sup>See 3.3.1 for discussion of this case.

fluctuations, which spawn further technoscientific problems about putative detection-events, and so on. At each step, novel and surprising situations encompassing scientists, theories and techniques, and the natural world lead to reconstructing theoretical, experimental, and technological practices.

For what follows, the key features of Dewey’s theory of inquiry are

1. Inquiry is a deeply social affair.
2. Genuine inquiry addresses a genuine problem.
3. Genuine problems result from a felt disruption of practice (a problematic situation).

I will now indicate how this model bears on the question of scientific significance.

## 4.5 Genuine Problems and Scientific Significance

There are two ways we can connect the previous remarks on inquiry and genuine problems to Kitcher and the question of how to assess scientific significance. The first is to make *genuineness* a necessary condition on a problem having *any* significance. The second is to look more deeply at the factors which *make* a problem a genuine problem, and see if that can give us a lead on how to assess *degree* or *amount* of significance. First, I’ll use these two general ideas to diagnose what is dissatisfactory about Kitcher’s account, then I will fill out the alternative view.

The first problem with Kitcher’s account is that just because one can trace out some logical connections between what is going on in the field and some new question, that doesn’t really make the question a significant one. To put it in the pragmatist idiom, you could sit down and draw out a significance graph for many a “paper problem,” but that doesn’t make it a real problem. Surely, Descartes’ evil daemon has certain connections to any area of inquiry whatsoever; if the evil daemon

exists, then we can't trust the results of any observation or reasoning. And yet, this isn't a serious worry for any scientist or scientifically-minded philosopher, and not because skepticism has been directly refuted. In other words, having significance-graph connections a'plenty is not a sufficient condition for significance.

The second problem is that Kitcher seems to deny the possibility that truly novel areas of inquiry can arise and still be significant. It seems possible that a whole new area of inquiry might open up in an area of practice hitherto unproblematic, or even in an area not known before to exist.<sup>21</sup> Such an area might have thin connections on a significance graph to prior scientific pursuits, or even to narrowly practical application and natural curiosity, and yet capture our attention in a way that makes it quite significant. It seems then that being thickly connected via significance-graphs isn't even necessary for being very significant.

The crucial problem with Kitcher's account is that the significance-graphs only capture *conditional*, *relational significance*, and this misses the intrinsic, immediate significance of most inquiries, the significance they have from the practice or the problem itself, rather than more remote areas of science to which it is connected. While Kitcher has shown the way in demonstrating the need for a context-dependent theory of the significance of science, much work is left to be done in providing an adequate answer. He needs yet a stronger grounding in the concrete features of the situation in order to limn the significance of scientific pursuits. In other words, we need to understand not only the intellectual-historical context of items of science, but the concrete situational context that constitute the problems that science aims

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<sup>21</sup>While it is doubtful that any inquiry is possible that is *completely* disconnected from prior practical and scientific investigations, and it seems unlikely that any significant area of research could arise without many such connections, there do seem to be several candidates for areas of inquiry whose significance far surpasses the relatively thinner connections to prior questions, problems, results, etc. of earlier science, as well as practical application and natural curiosity: Darwinian evolutionary theory, cellular automata theory, chaos theory, computer science, mathematical logic, and climatology, especially in the earlier days of those sciences, are quite novel in terms of their problems, subject-matter, and methods. Unlike Kitcher's favorite examples, it seems difficult to explain the intense level of interest in these areas in terms of significance graphs.

to resolve. Here's how I think the account ought to go.

Inquiries have significance in virtue of addressing some genuine problem. The conditions of genuine problematicity tell us whether some pursuit is significant. In other words, it is a necessary condition on attributing significance to some inquiry that it address a genuine problem, and any work on mere "paper problems" is disqualified from being counted as significant. Abstract skeptical worries don't count as significant problems. Problem sets in a college physics course aren't significant scientific research.

Secondly, the amount of significance depends on the features of the context or situation that make a problem genuine. Remember, a genuine problem is based on a real problematic situation. A situation is defined by a certain practice, and the situation becomes problematic when that practice is disrupted. The key questions for determining how *significant* the problem is, I want to suggest, depend on just what is the practice, the situational transaction, that is disturbed. How important is that practice, and so what is the urgency that we resolve the disturbance? And how much is it disturbed? (See Figure 4.2) We can imagine a small disturbance in a quite important practice may be very important. For example, suppose that we become aware of even a relatively small flaw in the practice of vaccination, such as a very low level uncertainty about its side effects. Because of the importance of vaccinations to modern medicine, this presents itself as a crucial matter. Second, consider a rather large disturbance in a much less important practice. Suppose you put very little stock in research in high energy physics (along the lines of S.E. Luria above). Nevertheless, a problem which shakes that area at a fundamental level might be quite significant indeed. Kitcher's significance graphs will only capture these comparisons *if* they lead to greater numbers of connections to other parts of science.

Kitcher's significance graphs will not work as a way of representing the full significance of inquiry, but they are able to get at conditional, relational factors that contribute to significance. They succeed in providing due recognition to the complex

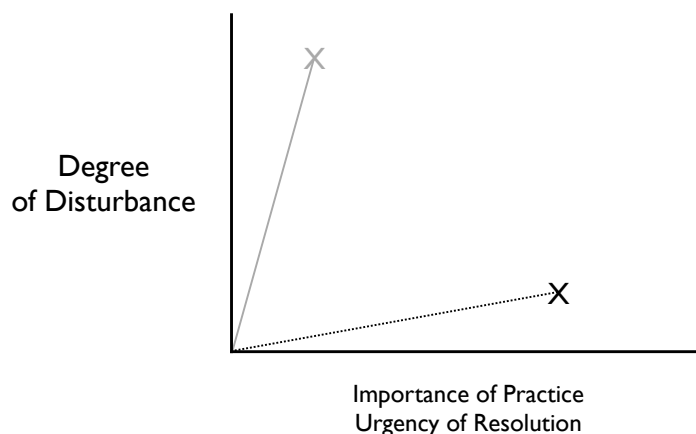


Figure 4.2: **Measure of Intrinsic Significance.** In addition to the kind of relational information about significance captured by Kitcher’s significance graphs, we must take into account the degrees of intrinsic significance of an inquiry. Problematic situations can arise from greater and lesser degrees of disturbance of some standing practice, and the practice itself has some importance that indicates the urgency that disturbances of it be resolved.

connectedness of science, and they may provide for us a spur to new inquiries, helping us discover new indeterminate situations that have not yet come to our attention. But they are not the right starting-point in understanding the significance of inquiry. Kitcher over-intellectualizes the problem of significance; in his account, it is the problem-formulations themselves that matter, not the indeterminate situations they represent. What’s crucial is that we begin with practices that matter,<sup>22</sup> and the more or less serious problems that arise in the course of those practices. This is what makes the problem-solving endeavors of science significant.

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<sup>22</sup>My thanks to Nancy Cartwright for providing this apt phrase for describing my view.



## 4.6 Consequences for Science & Democracy

Attempting to trace out the consequences of these criticisms and alternatives for Kitcher's project of providing a framework for understanding the social and political constraints on science, or providing an ideal of "well-ordered science" in a liberal democracy, would be a whole other chapter, perhaps a whole other book. I will point at some fairly obvious consequences, which seem to me to have the virtue of being obviously right.

First, I hope it is clear that, fully comprehending the significance of some part of science is going to require much more intimate knowledge about the situation than is available in Kitcher's significance graphs. The "importance of practice" here captures much of what Kitcher is after here with his idea of well-ordered science. Unlike Kitcher's account (but like the view that Simon (2006) thinks Kitcher is necessarily but unfortunately committed to), social values are already coming in at the point of assessing significance. Also, assessing the significance of particular research requires an understanding of the larger practice. This makes carrying out Kitcher's project much harder than it would be on his original account. Kitcher's significance graphs are suppose to make key information for assessing significance accessible to lay deliberators; democratic assessment of science would thus be possible. Unfortunately for Kitcher, this can't be done in the simple and schematic way that he hopes. Kitcher wants a short-cut solution to a hard problem, and as we've seen before, such attempts in philosophy, while common, tend to create more troubles than they the resolve. I've told a story about how to get "significance" right, necessary for engaging with the policy, and engage we must! No simple diagrams or single democratic meeting will set the research agenda for science. A more complex process will be necessary.

Of course, I've provided equally or more simplistic representations of my own, but the graph is not meant to be a full story. I'm not necessarily suggesting that

the added component of significance can be captured by a two-dimensional vector space and assigned Cartesian coordinates. Rather, I think that in order to be able to understand concretely a claim such as Figure 4.2 represents, you need to know about the standing practice in question and the way in which the problem disrupts that practice, as well as having some sort of sympathetic connection to the practitioners in question and the way in which they experience that disruption to be confusing, troubling, etc. This means that, insofar as information about significance is supposed to “tutor” the preferences of our ideal deliberators, that process will have a significantly more human face. In fact, I think the whole discussion needs to start to sound less like an ideal Rawlsian fairytale and more like an actual human discussion.

Second, I think that the demarcation between significance, which comes from the scientists’ side, and the image of well-ordered science, produced on the basis of ideal representatives of the interests of layperson groups, becomes untenable. Assessing the significance of a particular part of science will depend on the significance of the practice of which it is part. To oversimplify, the problems that arise in physics depend on the significance of the ongoing tradition of work going on over there in the physics department. However that gets cashed out is going to depend in part on complex relations of science as a practice to the rest of human life and affairs, and a necessary part of that story is going to be social, ethical, and political values. The way in which significance “informs” debates about science is going to be a more iterative, more reciprocal process. Thus we return to one of the main lessons at the end of last chapter, that science and social policy cannot be set apart and interact with each other in a thin way. We need to understand more closely the relationship between scientific practice and *social* problems, a project that Dewey called for long ago.

## Chapter 5

# Pluralism, Perspectivism, and Pragmatism

Ronald Giere's recent and remarkable book, *Scientific Perspectivism*, joins a long line of attempts to go, *Beyond Objectivism and Relativism*,<sup>1</sup> *Beyond Realism and Anti-Realism*,<sup>2</sup> *Beyond Positivism and Relativism*,<sup>3</sup> and so on.<sup>4</sup> Giere wants to find a way between an absolutist, objectivist realism and the constructivist or skeptical alternatives. The search for such a via media is quite admirable, though perhaps the attempt is not as novel as Giere implies.<sup>5</sup> His solution is treating sci-

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<sup>1</sup>See Bernstein (1983).

<sup>2</sup>See Hildebrand (2003), Goodman (1996), Frede (1987), and Rorty, (1986).

<sup>3</sup>See Laudan (1996).

<sup>4</sup>Besides those with obvious titles, we might also count Kitcher (2001), a variety of works by Rorty and Putnam, much of Feyerabend's work after 1987 (and arguably before), Kuhn's post-*Structure* writings, Peirce, James, and Dewey, back at least as far as Hegel, and a whole host of others in contemporary philosophy of science.

<sup>5</sup>Giere cites scientists and philosophers who fall on the objectivist side, and many sociologists and historians who fall on the constructivist side, but unfortunately discusses none of the work by those trying to find a way between the two. From sociology, Bruno Latour has been very critical of constructivism, and his positive view involving "hybrids" and "quasi-objects" shares much in common with perspectivism, as far as I can tell (See, e.g., Latour 1993). The philosophers mentioned in the notes above have similarly attempted to overcome the dilemma. I have heard that Richard Rorty once said in a seminar that, Every decade or so someone writes a book called something like *Beyond Realism and Idealism*. Then the critics go at it, and it always turns out that what lies beyond

entific observations and theories as “perspectives,” a visual metaphor that implies a subjectively-oriented component that avoids the negative aspects of objectivism, but enough of a world-oriented component that it also avoids the negative features of relativism and constructivism.<sup>6</sup> Giere also takes pains to emphasize perspectivism’s pluralistic nature. He even hopes that his view qualifies as a new species of realism.

I will attempt to show that Giere’s perspectivist project bears much in common with the work of two earlier philosophers: from the prior generation of philosophers of science, Paul Feyerabend, particularly his late work just before his death, and from the first half of the century, the experimental theory of inquiry of John Dewey. Further, I will show that their work can help improve and extend perspectivism in helpful ways, especially on the issues of representation, projection, and purpose. In the course of these comparisons, I hope also to throw light on part of Feyerabend that has thus far not been much discussed or well-understood and to demonstrate the relevance of pragmatist theories of inquiry to contemporary philosophy of science. These goals face the inevitable problem of attempting to reconcile the vocabularies of three philosophers working in different moments, which I will have to overcome by doing my best to stick to a common terminology.<sup>7</sup> Finally, I will investigate some remaining ambiguities or instabilities in the views being discussed, and I will suggest that the culprit is a continuing, but only partial adherence to the visual metaphor of a “perspective.”

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realism and idealism is... idealism! (Commentary at <http://crookedtimber.org/2007/06/09/richard-rorty/> retrieved June 25, 2007.)

<sup>6</sup>Cf. the use of “subject-sided moments” and “object-sided moments” throughout Hoyningen-Huene (1993).

<sup>7</sup>I will try to accomplish this by sticking primarily to Giere’s terms, and only introducing new terms for concepts that Giere lacks.

## 5.1 Giere's *Scientific Perspectivism*

The major claims of Giere's perspectivism,<sup>8</sup> as I see it, are:

1. Human and scientific observation and scientific theories are all perspectival.
2. Perspectives are an asymmetric<sup>9</sup> interaction between human (biological, cognitive, social) factors and the world.
3. Perspectives are partial and of limited accuracy.
4. Perspectives are neither objectively correct nor uniquely veridical.
5. Scientific truth-claims are relative to a perspective and are about the fittingness of perspectives.
6. Representation is a quadratic, not dyadic relation: "*S* uses *X* to represent *W* for purposes *P*" (60).<sup>10</sup>

(1) and (2) guarantee that the view avoids both objectivism and constructivism. Together with (3) they lead naturally to (4), which keeps objectivism from sneaking in as the uniquely-best perspective. (5) indicates that there is a limited role for truth and realism, and (6) provides an overall model for how the pieces fit together.

Giere begins his discussion with the case of color vision (Chapter 2). We know that the visual system works something like this: in the eye, there are cone cells that are differentially sensitive to wavelengths of light (unlike rod cells, which more or less don't differentiate). Most humans have three types of these cells. When

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<sup>8</sup>In addition to the book, Giere has laid out pieces of this view in a variety of other works. Principle among these are several discussions in Giere (1999), Giere (2004), and (2006b).

<sup>9</sup>The asymmetry is that humans have perspectives on the world, but the world has no perspective on us.

<sup>10</sup>In the remainder of the chapter, I will use parenthetical citations to refer to Giere (2006a) unless otherwise noted.

these cells detect light, they relay that information to what are called color-opponent cells. These cells combine the inputs from the cone cells in order to be able to detect the varying color-profiles of light. This leads our color experience to have a certain structure. So, for example, you will never see a red that looks greenish, because of the way these colors are opposed.<sup>11</sup> But while normal color vision is trichromatic, there are humans who are red-green colorblind, and thus only have two cone cells, and there are humans and animals with only rod cells who are monochromatic, and there are even reported cases of human women who are tetrachromats, which is the ordinary condition for some species of fish and birds. They would all have differently structured color experience.

Consider the comparison between trichromat humans, with three basic color cells, and monochromats whose vision is only black and white. Giere draws the following lessons from the comparison: (i) Neither perspective is objectively correct or uniquely veridical. Both perspectives are produced by the interaction of a visual system with light from the objects. Within the perspective, robust judgments can be made, but this is true both for the trichromatic and the monochromatic perspectives. Different biology, or different evolutionary paths, would have given us different perspectives, but there seems to be no way to say that one is more veridical than the others. Put differently, colors are not inherent properties of colored objects, but are produced by our interaction with them. (ii) Nonetheless, some perspectives are richer in some ways than others. The trichromat is sensitive to a variety of information from the environment that the monochromat is unaware of, and thus the trichromat can distinguish things the monochromat cannot. (iii) The different perspectives are not incompatible. Knowing the science of color vision, it doesn't seem to make sense to say that the two perspectives conflict with each other. The monochromat might

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<sup>11</sup>Giere's book contains a number of color plates that illustrate these features quite nicely, as well as the different perspectives in scientific instruments discussed below. The discussion and color illustrations in Churchland (2005) provide further resources for illustrating the neural workings and phenomenological structure of color vision. (Giere cites Churchland (2005) on p. 123n19.)

naively think that the trichromat's judgment that "this is red and that is green" contradicts his own judgments, but recognition of the different perceptual mechanisms involved makes it clear that the disagreement isn't genuine.

These give us the bones of Giere's perspectivism, in the case where he thinks that it is the best explanation of the science (and the explanation that most scientists would themselves use if they had sufficient conceptual clarity).<sup>12</sup> Though I've left out some of the interesting features of Giere's argument along the way (e.g. his argument for naturalism, and his defense of the causal-structural unity of the world, both on methodological grounds), this example captures the crucial features of the doctrine.

Next, Giere extends the account to scientific observation (Chapter 3). This is fairly straightforward, and can be illustrated with another quick example of Giere's: Say we want an image of the Milky Way. We have a couple of options. We can use an optical telescope to produce a standard, black and white photograph, registering the light that reaches us and is within visible wavelengths. Or we can use an infrared telescope, such as the one on the Infrared Space Observatory. The data from this telescope is processed by various computer manipulations, which result in a false-color image, in which visible colors are assigned to elements of the infrared spectrum. These two images, while of the same object, offer very different perspectives on that object, the optical and the infrared (See Figure 5.1). Each provides us with different information, may be used for different purposes, and may vary along certain axes of richness of information.

Finally, and most radically, Giere also argues that theories are perspectival, in the following ways: (i) They are partial in that they only describe some aspects

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<sup>12</sup>I should point out that it seems to me that Giere's position in this area is not uncontroversial, since there seem to be pretty significant disagreements amongst philosophers of color in how to interpret the findings of the science of color vision. Giere admits as much, but argues that "perspectivism" presents the best interpretation of the scientific data. Since I have no real stake in the proper interpretation of the metaphysics of color, I will simply leave the example as it stands.

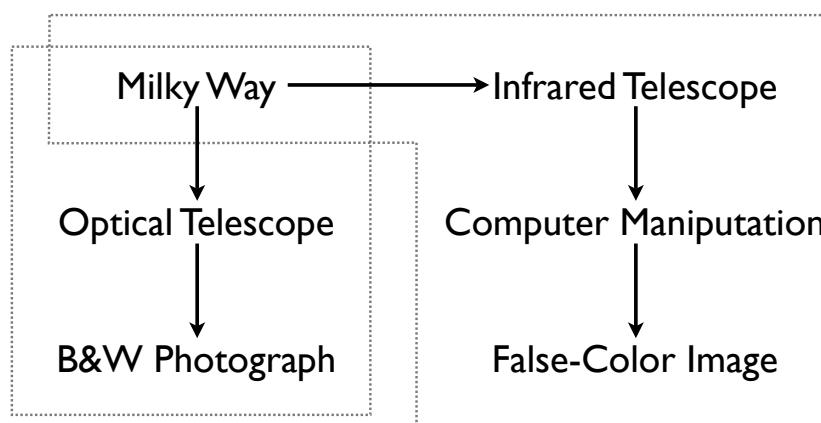


Figure 5.1: **Observational Perspectives on the Milky Way**

of nature. So Newton's laws provide a mechanical perspective, while Maxwell's equations provide an electromagnetic perspective. These only represent parts of any actual situation. (ii) Their accuracy or fit with the world is limited. No theoretical perspective is ever perfect, even when we narrow our focus to the aspects of the world it is meant to deal with. (iii) Scientific representations are 4-place relations of the form: Subject  $S$  uses representation  $X$  to represent the world  $W$  for purposes  $P'$ . (iv) Scientific representation is to be understood in terms of models rather than systems of statements.

Giere's preferred way to understand theoretical perspectives and how they represent the world is models-based (See Figure 5.2). If we hope to avoid the extremes of objectivism and constructivism, we want an alternative explanation of how theoretical principles are related to the world. Giere first notes that, by themselves, theoretical principles are never directly related to the world; they are definitional. If you add to them specific conditions, you can generate (constructively, not deductively) representative models that *do* aim to represent some aspect of the world. On the other hand, the World itself doesn't figure in to the comparison, either. The



World, as approached by instruments and basic data analysis taken together generate what Giere (following Suppes) calls “models of data,” which are processed, cleaned up, often idealized versions of the raw data produced by our instruments. Then, via application of the representative models, that are tested for their fit with models of data, hypotheses and generalizations are generated.

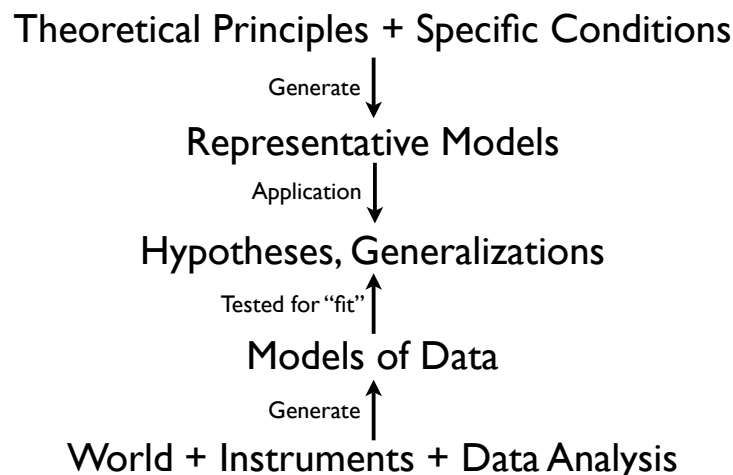


Figure 5.2: **Giere’s Models-Based Account of Theories** (adapted from Giere 2006a)

Note that all of these arrows represent constructive processes, not logical deductions or mere inductive generalizations. While the processes may become quite entrenched and second-nature, they are not “automatic” in a deeper sense. We might call these processes “projections.” When they become second-nature, they often become transparent, unwarrantedly lending credence to more objectivist accounts.

One of the reasons that Giere prefers the models-based account of theories is that it is supposed to avoid certain confusions in the linguistic account:

The assumption that scientific representation is to be understood as a two-place relationship between statements and the world goes along with

the view that scientific theories are sets of statements. A focus on the activity of representing fits more comfortably within a model-based understanding of scientific theories. (60)

It is hard to see the force of this argument. First, there seems little reason to believe, given what Giere has said, that there are two fundamentally different types of representation, models and statements.<sup>13</sup> Whatever type of relationship representing is, four-place or two-place, it should be so for language as for models. Second, it is not universally agreed that linguistic representation is a two-place relation. C.S. Peirce's semiotics,<sup>14</sup> for one, treats representation as a three-place relation (and his "interpretant" does the work of Giere's agents and purposes, and more besides). The preference for models over statements must not hang on general features of representation, which they should share, but on more specific claims about the role of models and statements in scientific practice, where Giere may be on firmer ground. It would be interesting to know whether linguistic representation plays a role that is not subservient to the construction of models in the way Giere says it is, though. I suspect so, since Giere's story here seems a little *too* neat.

Unlike different observational perspectives, different theories should be, but are not automatically compatible. Just as maps derived from two different systems of projecting the globe can be incompatible when they give different areas for the same continent (78-80), scientific theories that describe different geometries of space-time are incompatible. There is clearly some breakdown of the analogy to human perspectives, here, but it isn't entirely clear why Giere goes this way. Consider two maps of the world,  $X_1$  and  $X_2$ . If one holds the purposes  $P$  fixed,<sup>15</sup> then it is clear that there would be some incompatibility between e.g., a Mercator map and a

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<sup>13</sup>Craig Callender suggested a similar line of criticism to me, without any reference to Peirce.

<sup>14</sup>See Peirce (1894) for one of many discussions.

<sup>15</sup>The incompatibility also depends on holding the subjects  $S$  and the world  $W$  fixed. Though a radical Kuhnian might insist that scientists working in different worlds could use different models without generating an incompatibility, this possibility is controversial in any case argued against by Giere. It also seems like one makes a going assumption that subjects of representation are interchangeable. In any case, the point remains that once we regard representation as a four-place

Robinson map, if one's purpose is to understand the relative sizes of Greenland and Africa. But if  $X_1$  and  $X_2$  have their own purposes associated with them,  $P_1$  and  $P_2$ , which they presumably do to some degree, since Mercator's map was created to make navigation easier, while Robinson's was created to give a better overall picture of the sizes and shapes of continents, then their incompatibility might be tied to their inherent purposes, and they only seem incompatible when the context is ignored, like the judgments of trichromats and monochromats, or the optical and infrared pictures of the Milky Way. Likewise, two scientific theories could be compatible if we considered them to be associated with different purposes, and thus different measures of fit or similarity.<sup>16</sup>

Giere promises a quadratic picture of representation, including purposes and agents. Mostly, however, his comments on these features are schematic. The partiality and limited accuracy of perspectives does much of the specific work in Giere's account, rather than agents or purposes per se. Giere doesn't say much about how the features of the scientist play a role. He does say that since the perspectival data produced by scientific instruments must be *public*, the subjectivity of the scientist shouldn't play much of a role, and also that we might productively analyze scientific practice using a framework of "distributed cognition" that would bring in ethnography and cognitive science into science studies (Chapter 5). But none of these things plays a significant role in the detailed discussion of theories and models.

The role of intentions and purposes is not explored systematically or in-depth.<sup>17</sup> Here are the different ways in which purposes may play a role, according to Giere: picking out the features of the model which will be compared to the system modeled (63-4), determining the measure and strictness of similarity to determine

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relation, it is difficult to regard any two representations as incompatible unless the other 3 elements remain fixed.

<sup>16</sup>Though it seems to me an open question at this point whether scientific theories might be sufficiently multipurpose or serve similar purposes as to allow incompatibility to remain.

<sup>17</sup>Interestingly, though he makes them seem crucial to account of representing, "purpose" doesn't even appear in the index of the book.

whether the model fits (64, 69), choosing which features to attempt to represent (73), and choosing conventions of interpretation of the models (74), etc. The only role for purposes that receives much specific discussion, however, is “whether the model fits the world as well as desired” (89). Nothing about the scientists’ specific purposes plays a role. For example, if I want to evaluate the accuracy of this model because I hope to make predictions about the weather or the movement of planets, or because I want to intervene to treat disease or to fix an injured ecosystem, I will have to supplement Giere’s account. Of course, Giere’s account makes room for such an extension, which is much to his credit.

With these concerns in mind, I want to move now to a discussion of Feyerabend’s work on the invention of perspective in Renaissance art, and its relation to scientific representation.<sup>18</sup> Feyerabend takes the place of the agent seriously in a way that Giere avoids, but on many points, they are in substantial agreement.

## 5.2 Feyerabend on representation in art and science

Looking at two pictures of the Madonna with child, one from the thirteenth century and another by Raphael in the sixteenth,<sup>19</sup> and without much knowledge of recent art history and criticism, we may be inclined to think of the earlier one as clumsy, unrefined, unrealistic, and a poor representation of its subjects, while the second might strike us as deft, sophisticated, and highly realistic. In Chapter 4 of Feyerabend (1999), he attempts to show us that we ought to regard this reaction

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<sup>18</sup>Giere himself suggests such a comparison (p. 14), though he doesn’t follow up on it, and Feyerabend is not among the authors he cites as having made connections between perspective in Renaissance art and scientific representation.

<sup>19</sup>Feyerabend includes two such examples on pp. 90-1. You can find them at [http://www.artandarchitecture.org.uk/fourpaintings/daddi/inner\\_centre/humanity.html](http://www.artandarchitecture.org.uk/fourpaintings/daddi/inner_centre/humanity.html) (Figure 1) and <http://www.wga.hu/frames-e.html?/html/r/raphael/2firenze/1/22grandu.html> (both retrieved May 28, 2007).

as naive, that we can understand both of these paintings as equally realistic, or, alternatively, as equally artificial and conventionalized. In doing so, he points to a sophisticated, perspectivist theory of representation.

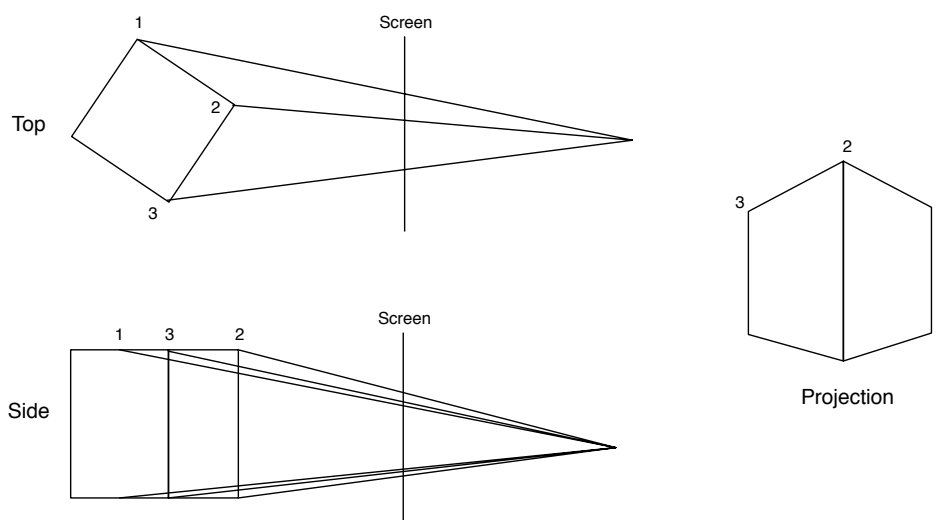


Figure 5.3: **Rules of Projection for Single-Point Perspective** (based on Feyerabend 1999, p. 96, figure 6)

Feyerabend takes us to the Renaissance, and the invention (or rediscovery) of perspective in modern painting. The innovators in the use of perspective like Brunelleschi brought techniques from architecture, geometry, and optics to create definite rules for the construction of a painting. Seen in Figure 5.3 is one representation of such a construction principle.

In Figure 5.4 we have in schematic form an “experiment” by Brunelleschi discussed at length by Feyerabend. Brunelleschi created a picture of a church in Florence, “the Baptisterium,” as seen from a spot a certain distance away from it. To view the painting one must come up to this spot, hold the painting a certain distance from the ground, and peer through a small, conically shaped hole in the center of it. A mirror, held across from the painting, reflects the image back to you,

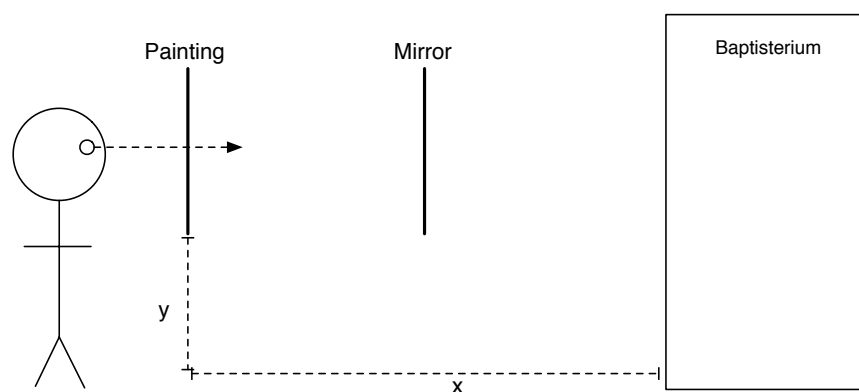


Figure 5.4: **Brunelleschi's Perspective Experiment** (based on Feyerabend 1999, p. 95 figure 5)

though the hole, ensuring that you are at exactly the right place for viewing the picture. Remove the mirror and something remarkable happens; there is very little change in what one is seeing! If the positioning is absolutely right, one should be able to move the mirror in and out and see just how well the two match.

Feyerabend describes the situation thus:

Brunelleschi examined his painting by checking it against something else. This “something else” was not a building... it was an aspect of a building... the effect (of an object) on an individual, or a group, or a device... that approaches, uses, views, analyzes, or “projects” it according to more or less clearly describable, though not always clearly recognized, procedures... His experiment involved two artifacts, not an artifact (the painting) and an art-independent “reality” (Feyerabend, 1999, p. 100).

So, we don't have direct comparison of the painting and the building. What we have is a projection of the painting and an aspect of the building, both arrived at thanks to rigorously specified viewing conditions. We might call the projection of the painting a “representational model,” because, without the system of projection, it would not be seen as similar to the building,<sup>20</sup> though, significantly, the painting

<sup>20</sup>Indeed, this was a problem for later Renaissance painters, who wanted to produce paintings

is not an abstract object, and the methods of projection are physical rather than abstract. Likewise, Brunelleschi produces an aspect of the building just as scientific instruments produce one or another aspect of the Milky Way, and these, not the objects themselves, are compared to the model.

Only when things are arranged just so between the painting, building, and viewer, can we make the comparison:

The best way to describe the situation is by saying that Brunelleschi built an enormous stage,<sup>21</sup> containing a preexisting structure (the Baptistery), a man-made object (the painting), and special arrangements for viewing or projecting both. The reality he tried to represent was produced by the stage set, the procedure of representation itself was part of the stage action, it did not reach beyond it. (Feyerabend, 1999, pp. 100-1)

So, in Figure 5.5 we have Brunelleschi's stage, with the stage action called "representation" happening only within this circle.

Feyerabend applies the same schema to scientific experiments as well (thus implicitly accepting the idea that the problems of scientific representation are just a specific case of representation generally, not an entirely other beast). In the case of the CERN experiments that led to the discovery of the W and Z particles, we have Nature being projected via a large, complicated, and delicate set of instruments to produce proton-antiproton collisions (an artifact), and we have the electroweak theory being adapted by clever mathematical tricks and computer models. The data

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that could be viewed in a normal way. For example, it was known by Leonardo, Raphael, and others that the "correct" projection, according to the geometric rules, of a sphere is usually an ellipse. Nevertheless, they are always represented by circles (though Raphael did experiment with ellipses in engraved reproductions, eventually coming to regard them as unacceptable). See (Feyerabend, 1999, p. 98n.8).

<sup>21</sup>I think "stage" here is an unfortunate mixing of metaphors, since Feyerabend has so far been working with perspective and painting rather than a dramatic example, and that this has contributed to the difficulty of understanding this chapter. Perhaps the dramatic metaphor came readily to his mind because of his experience as an actor earlier in his life. Perhaps, however, the metaphor of the stage does work that sticking to the perspective and painting metaphors would not do so easily. And a play no less than an artwork provides the audience with a certain perspective.

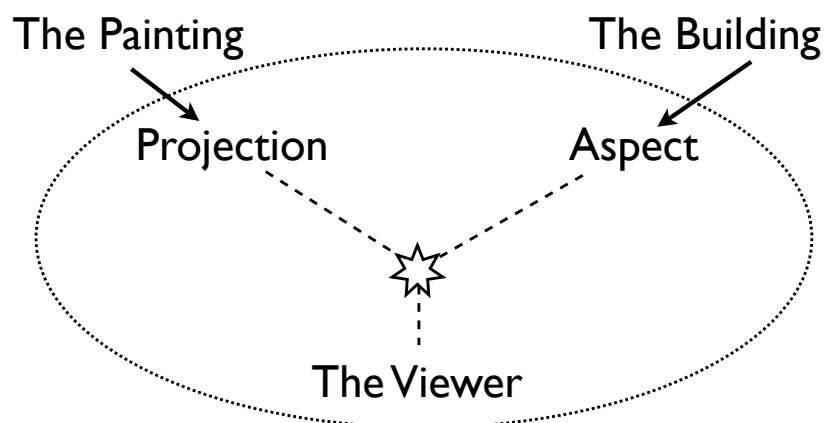


Figure 5.5: Brunelleschi's "Stage"

arrived at is then further processed and idealized, and only then does comparison take place (See Figure 5.6).

Figure 5.7 shows a generalization of Feyerabend's model to scientific representation, given in Giere's own terms. Man-made objects (paintings, theories) are compared with the World only through projections, just as in Giere's own view. Theoretical principles must be transformed into representational models, and the scientist must generate models of data in order to make a comparison. That comparison must also equally account for and *create* an audience, that is, the mostly unspecified "subject" of Giere's account. In addition to the background features of the agent, their beliefs, habits, practices, biological and cognitive capacities, they take on a role as the audience of the representation. Just as with the generation of representational models and models of data, the adoption of this role is projective and additive.

What get compared, what really are part of the act of representation, are two functional artifacts, two things created by their role on the stage: representational models and models of data. Theories are not compared with the world. Additionally,



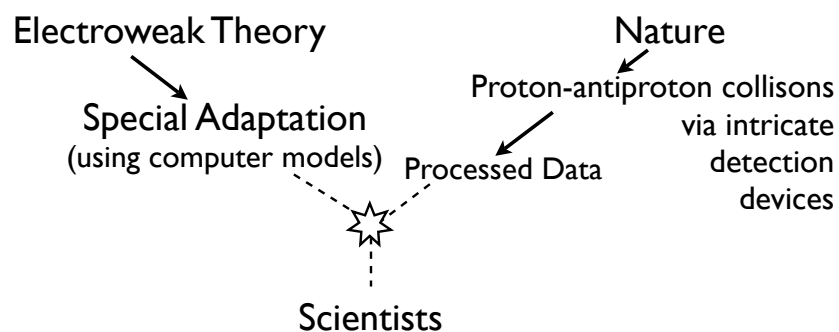


Figure 5.6: The CERN Stage for the  $UA_1$  experiment for the discovery of W and Z particles

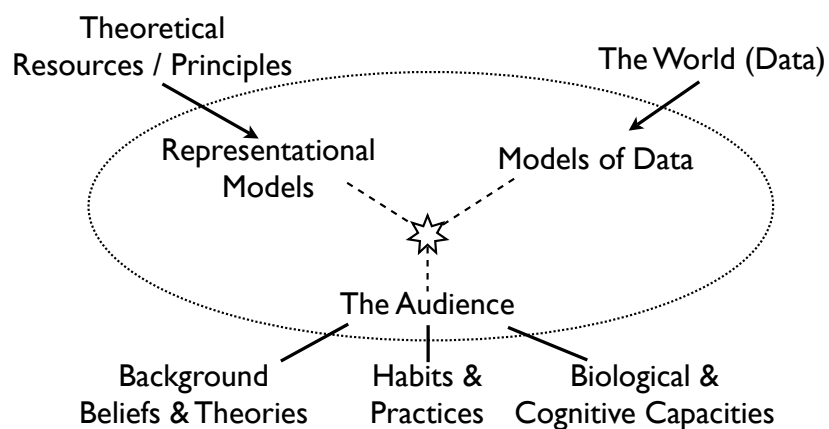


Figure 5.7: Feyerabend's Dramatic Model of Scientific Representation

the comparison, the similarity or fit between these two objects, is not an abstract relation, but it is an act carried out by agents fulfilling another functional role in the process of representation, the audience. The “stage” highly constrains theory, facts, and audience; its construction makes comparison possible, and this construction is a projective process. As we’ve seen, the process of projection can take many forms; sometimes causal-physical action or constraint, sometimes highly abstract processes.

By looking at things in this way, in addition to the further specification of the role of agents in the act of representation, we might make a further distinction between types of purposes that play a role in representation. On the one hand, there are purposes that form part of the background of the audience; call these “interests.” Much of Giere’s own discussion of purpose seems to fall in this category. On the other hand, there is the purpose that guides the comparison in the first place, that prompts the construction of the stage; call this the “guiding purpose.” It is this purpose that will provide the most fundamental reference to use, insofar as a representational activity has connection to human practices, and it is this sense of purpose that seems highlighted by the scheme “ $S$  uses  $X$  to represent  $W$  for purposes  $P$ ,” but which is largely neglected by Giere’s own discussion.

For help with these problems, we will now turn to a discussion of the pragmatists, philosophers of practice and purpose *par excellence*.

### 5.3 Pragmatism on purpose and inquiry

The main reason to turn to a discussion of Peirce and Dewey<sup>22</sup> is that, both for Giere and Feyerabend, the question of “purpose” or “interest” has arisen, but the role that purposes play in the processes of representation that have been discussed has been fairly under-specified. Clearly, it has to be part of the human contribution in both cases, what I have called “interest.” But this seems to be insufficient, and I

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<sup>22</sup>Gieryn discusses his own relation to pragmatism in his (1999).

also want to understand the role of purpose in guiding the overall activity, in bringing the “stage” together in the first place.

Giere says that fit is interest-relative, but the overall purpose is just to represent a certain aspect of the world to a desired degree. So, we may look at how a subway map represents the subway. Our interest in using the map to navigate the city will inform how accurately the otherwise highly idealized map fits the landscape; if all we care about are the relative positions of stations and lines, it may fit with complete accuracy. But here, purpose is only being discussed at a late stage in the game, at the level of hypotheses and generalizations. Yet, obviously, the map was created for a reason, and while Giere clearly acknowledges that there is an overall purpose guiding the activity, he says little about it.

Feyerabend is clear that there are many other purposes besides imitation for works of science or art, though he also focuses on imitation. But imitation or representation by itself doesn’t suffice for a purpose. Without an idea of the purpose or interest one has in constructing a representation, it is a vain or silly enterprise, a kind of game. Children may engage in games of imitation, following around a sibling and repeating their every action, mimicking everything they say; scientific representation is more than this. We need to know what distinguishes pointless from significant representations, arbitrary from useful similarities. One could create a model that quite accurately fits a large or perhaps infinite number of facts about the contents of my desk or this table, but this representation has very little significance to anyone, and really no significance to science. As Giere says, any object is similar to any other in countless respects (63). Giere and Feyerabend haven’t given us the resources to distinguish significant from insignificant representations, and this is because of a relative neglect of the guiding role of purpose.

Let’s restrict our discussion from here on out to cases where the activity that representation figures in is inquiry, and ignore other activities, such as immediate use and application, or art, or storytelling, or education, though a more complete

account would include them.

According to John Dewey, the purpose of inquiry is to resolve a problematic situation by constructing a judgment that resolves the problem.<sup>23</sup> That is to say, we begin in a certain situation that involves us, our environment, and the projects and practices we are engaged in. Something in that situation becomes disturbed or problematic, and inquiry is the process of trying to return that situation to a settled state. The projects and practices in the situation can vary from the mundane and practical to the *recherche* and academic, and so inquiry is not restricted to narrowly practical problems.

It will be helpful to mention Peirce, because he first developed the pragmatist theory of inquiry that was brought to higher articulation by Dewey, and he described it in somewhat less technical terminology.

C.S. Peirce's theory of inquiry argues that inquiry begins with genuine doubt, which arises from disruptions of concrete practice, not idle speculation. Peirce's favorite foil for his scientific epistemology is Descartes, who wants to begin all inquiry by doubting everything that can be doubted, and building up only from what is absolutely certain. Peirce thinks this method is fruitless and impossible, as such "paper doubt" cannot actually get us to challenge our beliefs. Of course, everyone nowadays thinks that Descartes method is fruitless and impossible, but what's important is that Peirce's explanation of this failure is that it fails to create the irritation of doubt that can lead to real inquiry and the creation of new beliefs.<sup>24</sup> Competent inquiry proceeds until belief is so settled as to allow practice to continue without further disruption.

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<sup>23</sup>Perhaps it is not entirely right to think of this as a general purpose of inquiry. The purpose of inquiry is going to be set by the particular problems in the situation at hand, and it might be infelicitous to refer to the guiding purpose of all inquiry as problem-solving. If this is right, then it is better to think of problem-solving as a purpose-schema.

<sup>24</sup>Compare Kierkegaard: "The method which begins by doubting in order to philosophize is just as suited to its purpose as making a soldier lie down in a heap in order to teach him to stand up straight" (Kierkegaard 1952, p. 5).

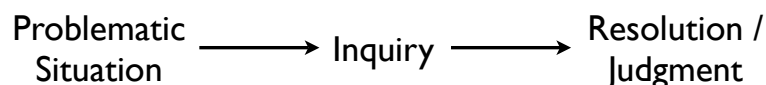


Figure 5.8: **Dewey on the Temporal Development of Inquiry**

As Peirce says:

The irritation of doubt is the only immediate motive for the struggle to attain belief. . . Some philosophers have imagined that to start an inquiry it was only necessary to utter a question whether orally or by setting it down upon paper. . . But the mere putting of a proposition into the interrogative form does not stimulate the mind to any struggle after belief. There must be a real and living doubt, and without this all discussion is idle (Peirce, 1877).

Dewey takes up this line of thought in his own writings on logic and inquiry:<sup>25</sup>

The function of reflective thought [i.e., inquiry] is. . . to transform a situation in which there is experienced obscurity, doubt, conflict, disturbance of some sort, into a situation that is clear, coherent, settled, harmonious. (LW 8:195)

So, for Dewey, inquiry begins in a problematic, doubtful, conflicted situation, it proceeds to identify and attempt solve the problem at hand, until a judgment is issued that resolves the difficulty and is thus called a “warranted assertion.” If this progression (see Figure 5.8) is successful, and the result is stable, we would say that inquiry has succeeded in its purpose.

Dewey also has a picture of inquiry that bears similarities to the perspectivist accounts given by Giere and Feyerabend. Though Figure 5.9 leaves out many features of Dewey’s theory of inquiry, it highlights those features that are most directly relevant to the present discussion. We begin, on the one hand, with general

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<sup>25</sup>The most important sources for this are *Essays in Experimental Logic* (1916), *How We Think*, Rev. Ed. (LW 8), and *Logic: The Theory of Inquiry* (LW 12).

theoretical, or to use Dewey's favored term, ideational resources, and on the other, with nature or experience. From the ideational resources, we construct or project a series of ideational propositions that lead us from general theoretical principles to applicable claims. Through interacting observationally and experimentally with the world, we construct a set of factual claims meant to help identify the problem and test solutions. The whole process concludes when the ideational and factual resources can be combined or coordinated to issue a judgment.<sup>26</sup>

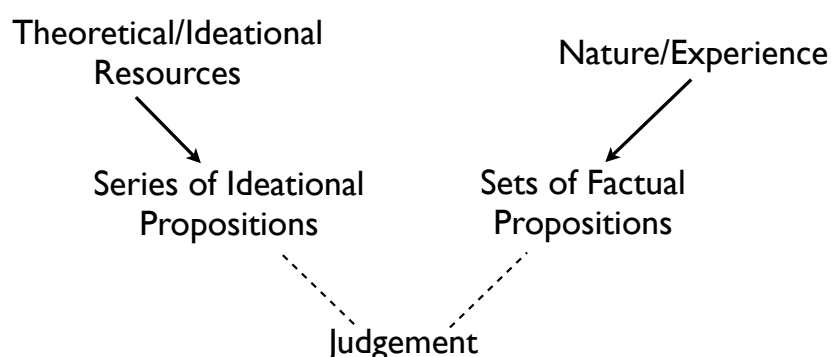


Figure 5.9: **Dewey on the Production of Judgment**

Like Giere, Dewey believes that theoretical principles or ideational propositions, at the most general level, are abstract structures that do not directly refer to or describe any concrete features of the world. While for Giere, the content of such principles depends on their ability to create lower-level models that *do* have representational content, for Dewey it comes from both the interrelationships between theoretical concepts, and their eventual operational power of applicability. From our theoretical resources, we arrange a series of propositions that leads closer to appli-

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<sup>26</sup>One of the most important differences with Giere that the reader will notice is role of linguistic terms: ideas, facts, propositions, judgments, claims. Actually, though Dewey uses these terms, his views about them differ radically from the tradition, in that all of these stand both for meaningful symbols (not necessarily linguistic) and for *operations* (they have operational meanings).

cability, and thus can be put into operation.<sup>27</sup> Dewey says of facts that they are not given by, but taken from experience, emphasizing the constructive element in this process. So, not the world in itself, but its projection via experimentation and fact-determination plays a role in inquiry. While Dewey did not benefit from the later development of a “models-based” understanding of theories, his views clearly resonate with it and with perspectivism in many ways.

The goal of inquiry is called “judgment,” and it is understanding this goal that can help us understand the role of purpose in guiding inquiry (See Figure 5.10). For Dewey, what guides the selection of facts and the inferences from theoretical

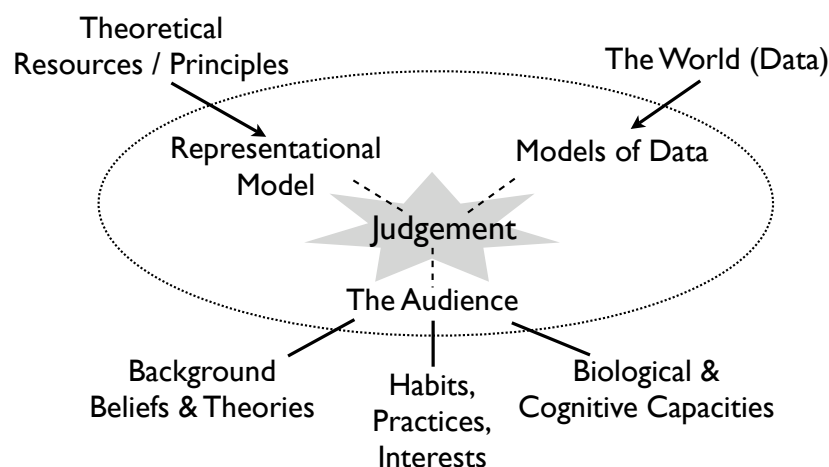


Figure 5.10: **Complete Dramatic-Pespectivist Model of Scientific Inquiry**

resources to the ones that are directly applied is their ability to lead to a judgment that can effectively solve the basic problem; to oversimplify: it answers whatever question needs to be answered for practice to resume. All features of inquiry—problem-statements, hypotheses, theories, facts, methods, and ultimately forms of

<sup>27</sup>Dewey has interesting things to say here about logical forms that guide this development. See (LW 12:310-346).

warranting—are revisable in service of this basic goal. Also, Dewey makes clear that, in inquiry, this process (which Feyerabend would call “stage-construction”) is a highly experimental enterprise (which should be no surprise to anyone familiar with the production of plays). It is understanding the various requirements on judgment that a problem-situation can create that will provide specific lessons about the role of guiding purpose in different contexts.

## 5.4 Conclusion

First, I want to emphasize the ways in which these three philosophers converge: All of them provide a picture in which inquiry and representation inherently requires projection, both of facts and theory, and they make clear that the process of projection is a highly constructive, not deductive, process. Some projection is physical rather than abstract, as with Brunelleschi’s viewing set-up and scientific experiments. The process of projection may become transparent as it becomes routine, thus (falsely) encouraging naive, objectivist realism.

The final picture of perspectivism, as shared among these authors, is the following:

1. That observation and theory are both limited and partial perspectives on the world.
2. That inquiry doesn’t disclose a single, coherent description of the world, but a plurality of overlapping perspectives, which are compatible in one sense, which are all perspectives on the same world, but don’t add up to an absolute view of the world.
3. These perspectives are inherently bound to our purposes, interests, practices, and abilities.



4. Representation is a 4-way affair between theory, world, audience, and guiding purposes.

To return to Giere's recent work, I think we have learned that the human contribution cannot be downplayed, but must be understood as making as much of a contribution to the activity of comparison as the things being compared. We need a thorough account of purpose as guiding not just fit, but the selection of theory, fact, and methods of projection. The whole activity of representation is guided by a purpose. And we must understand how different purposes can allow for guiding scientific activities in different ways. One promising direction is to treat the purpose of scientific inquiry, in the most general terms, as problem-solving.

There are many difficulties in understanding perspectivism, some of which I've tried to mitigate here. Perhaps one source of the difficulty is that "perspective" is a visual metaphor that suggests knowing is ultimately a passive activity, i.e., the "spectator theory of knowledge" that Dewey warned us about again and again.<sup>28</sup> While the perspectivist would be quite right to respond that visual perception is not at all a passive process, the naive association remains there to cause trouble. Perhaps, since Giere speaks of perspectives as a particular type of *interaction* with the world, and since we've seen that his account could benefit from bringing the discussion to the forefront, it would be best to change metaphors. One option is to more fully adopt Feyerabend's artistic-dramatic metaphor, which highlights the active elements, and end up with *Scientific Dramatism*. Or perhaps it would be better to follow Dewey, and replace the model of visual perception with a model of practical coping in the world, thus giving us *Scientific Pragmatism*. Fretting about terminology aside, Giere has clearly made a significant contribution to making the view clear and compelling.

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<sup>28</sup>Especially in *The Quest for Certainty* (LW 4).

An earlier version this chapter has been accepted for publication as “Models and perspectives on stage: remarks on Giere’s *Scientific Perspectivism*” in *Studies in History and Philosophy of Science* (doi:10.1016/j.shpsa.2009.03.001), and will appear later this year.

## Chapter 6

# On the Very Idea of Pragmatist Epistemology Answering Richard Rorty

### 6.1 Introduction

In this chapter, I will give sympathetic reconstructions of Rorty's and Feysabend's attacks on epistemology and scientific method. I will highlight which of their lines of criticisms are the most compelling, and show how a pragmatist theory of inquiry of the sort that I have defended deals with their objections.

### 6.2 Rorty's Attack on Epistemology

The aim of Richard Rorty's *Philosophy and the Mirror of Nature*, according to the introduction, is not to outline or to argue for a philosophical theory, but

to undermine the reader's confidence in "the mind" as something about which one should have a "philosophical" view, in "knowledge" as some-

thing about which there ought to be a “theory” and which has “foundations,” and in “philosophy” as it has been conceived since Kant. (Rorty, 1979, 7)

In this section, I will be looking at the second part of this attempt, the attempt to undermine the view that knowledge is something about which one should have a theory. As stated, the claim is obviously too strong; there are many different types of theories one might have about knowledge, many of which Rorty would have no reason to consider illegitimate, among them the theory that one ought not have (some specific type of) theory of knowledge.<sup>1</sup> It will be necessary, therefore, to pin down Rorty’s target. I will argue that he takes aim at the sort of theory common to mainstream, traditional epistemology, what we might call *Global Epistemology*, a theory of knowledge that is particularly *philosophical*, as well as universal, ahistorical, and acultural.

It is tempting to interpret Rorty’s attack on epistemology in the following way:

1. Epistemology is a contingent development.
2. Therefore, doing epistemology is optional.
3. Therefore, we should opt out of epistemology.

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<sup>1</sup>Though one must be careful calling this a theory. Rorty himself wants to avoid being seen as theorizing: “Whereas less pretentious revolutionaries can afford to have views on lots of things which their predecessors had views on, edifying philosophers have to decry the very notion of having a view, while avoiding having a view about having views. This is an awkward, but not impossible position” (Rorty, 1979, 371). Others have noted that it is awkward precisely because it is impossible. I think the question is much more complicated than that. Rorty continues, “Perhaps saying things is not always saying how things are” [*ibid.*, Rorty’s italics]. Fair enough, but even if you aren’t saying how things are, a view that it is best not to think of knowledge in the traditional ways (or in any way) still seems like a view or a theory, very broadly speaking. So we at least need to figure out what saying “how things are” about knowledge amounts to for Rorty.

This interpretation is clearly inadequate.<sup>2</sup> It is true that Rorty puts a lot of effort into showing that epistemological problems and the view of mind that they presuppose is contingent and optional; these arguments are powerful and largely successful, but they are not sufficient for the conclusion Rorty draws.<sup>3</sup> What would make the inference from (2) to (3) successful, however, would be arguments that epistemology *as a pursuit* is undesirable, problematic, hopeless, or mistaken. As I will show, Rorty provides a variety of such arguments, which provide strong support for his conclusion.

If Rorty undermines Global Epistemology, a vacuum is left in its place. A question remains whether there is room for a theory of knowledge that is sufficiently general, in some way philosophical (rather than merely psychological or historical), yet which avoids the problems of Global Epistemology as Rorty has characterized it. Rorty himself fills the void with a sort of anti-theory, *hermeneutics*, which he characterizes as an expression of hope for continued conversation, for open attempts at reconciliation between local discourses and epistemologies, which presupposes no method of commensuration nor any authority of one mode of discourse over another. Rortyan hermeneutics involves, *inter alia*, a refusal to consider general features of knowledge or inquiry.

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<sup>2</sup>The inference from (1) to (2) is questionable, but I would accept it, if it is understood in the following way: if epistemology is a project that was picked up at a certain point in history but might not have been, it seems clearly optional in the sense that we could have done and could be doing something else. It is a legitimate issue whether, having gotten ourselves into the epistemological bog, we can get ourselves out. I am inclined to agree with Rorty that we could. (Though I don't believe that the post-epistemological world would be simply a return to our pre-epistemological innocence. We cannot go back to Eden.) Leaving this aside, the inference from (2) to (3) is clearly fallacious.

<sup>3</sup>Compare the work of the Churchlands, where much effort is expended in showing that the commonsense framework is a theory that is vulnerable to criticism and replacement. It would be insufficient to stop there; they must provide arguments that the commonsense framework is significantly problematic and that one can improve their situation by adopting a less problematic alternative on offer from neuroscience.

### 6.2.1 Three Versions of ‘Epistemology’

Before examining Rorty’s attack on epistemology, it is important to pin down Rorty’s target. Sometimes it seems as though Rorty is attacking theorizing about knowledge in the most general sense. Other times, it seems that Rorty is attacking merely a single family of epistemological positions. There is a triune ambiguity in Rorty’s use of the term ‘epistemology,’ yet only one of these pursuits is Rorty’s main target.<sup>4</sup>

The first and least common usage to which Rorty puts the term ‘epistemology’ is the most broad: theories of knowledge in the most general sense.<sup>5</sup> This would include not only the epistemology of Rorty’s critique, but also the stance of epistemological behaviorism, which Rorty either adopts or at least utilizes in his critique,<sup>6</sup> as well as theories like empirical cognitive psychology and history and sociology of knowledge, which Rorty considers legitimate in their own spheres.<sup>7</sup> This target is far too broad to be the subject of Rorty’s critique, which is at least aiming at something

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<sup>4</sup>Susan Haack also sees a different triple-ambiguity in Rorty’s use of the term ‘foundationalism,’ which she disambiguates along the lines of (i) empiricist foundationalism, (ii) the view that epistemology provides *a priori* foundations for the sciences, and (iii) the thesis that criteria of justification require objective grounding in a relation to truth. These categories cross-classify Rorty’s critique and won’t help us identify Rorty’s target. Haack makes the distinction to show that Rorty’s attack is really on three fronts, and that the battle on (iii) does not go so well (Saatkamp, 1995, 130-3). But it is difficult to see precisely what Haack is after with the “need of objective grounding” and “relation to truth.” On one reading, she might be after something that is either vacuous or which is directly attacked by pragmatist arguments (this is how Rorty understands the situation, and his response to this reading is effective (Saatkamp, 1995, 148-53)). But perhaps she means these things in a more subtle, pragmatically informed way that might open more room for the position defended in this paper.

<sup>5</sup>See (Rorty, 1979, 7), but, as Rorty is here identifying his target, he must mean “theory” in a specific sense.

<sup>6</sup>It is never quite clear how committed Rorty is to epistemological behaviorism; he probably shrugs off any apparent ontological commitment to this view. Compare above, footnote 1. Rorty claims that Quine-Sellars’ epistemological behaviorism “is not the attempt to substitute on account of human knowledge for another, but an attempt to get away from the notion of ‘an account of human knowledge’ ” (Rorty, 1979, 180).

<sup>7</sup>See, e.g., his rejection of Quine’s philosophical anti-intensionalism in the realm of empirical psychology (Rorty, 1979, 194ff.).

uniquely philosophical.

Another way in which Rorty uses ‘epistemology’ is what I will call *Local Epistemology*. Local Epistemology is the study of the patterns of justification in particular sciences, research programmes, paradigms, cultures, traditions, etc., what Rorty refers to as ‘normal discourses,’ an expansion of Kuhn’s ‘normal science’ (Rorty, 1979, 385). Rorty does not go into much detail describing this sort of enterprise, but it seems he is considering the sort of interpretation and criticism of methodology and technique that is internal to some discourse, which might be discussed by either members of the discourse or specialized philosophers. Much of contemporary philosophy of science is of this form; for example, the methods of evolutionary biology might be discussed and evaluated both by biologists and by philosophers of science who specialize in philosophy of (especially evolutionary) biology. These Local Epistemologies will generally vary across different discourses and across time within a discourse. But Local Epistemology cannot be the target, either. As Rorty himself says, the possibility of hermeneutics is parasitic upon this type of epistemology (Rorty, 1979, 366).

What Local Epistemology lacks are pretensions to universality. The activity that Rorty really targets I will therefore call *Global Epistemology*. The basic way to understand Global Epistemology is as the attempt to generalize from some Local Epistemology to cover all of inquiry over all time (Rorty, 1979, 385). Global Epistemology is characterized by Rorty in a number of ways. It is taken as an all-encompassing discipline which is taken to legitimize or ground all other disciplines (Rorty, 1979, 6). It is a permanent neutral matrix for inquiry and for all of culture (Rorty, 1979, 8). It aims at apodicticity and universal commensuration (Rorty, 1979, 136-8, 349). It treats knowledge as a collection of representations of the world (Rorty, 1979, 136). Often it brings along ideas that demand a moral commitment, such as “Reality, Truth, Objectivity, [or] Reason” (Rorty, 1979, 385).

One may fairly ask whether these three categories exhaust all the activities

that epistemological philosophers have engaged in, and especially whether they have engaged in Global Epistemology, which is really the only pursuit that Rorty has strong arguments against. While this is debatable beyond the scope of this chapter, I am inclined to agree with Rorty that at least *most* epistemological philosophers since Descartes have engaged in an activity that has at least some of the pernicious features of the Global Epistemology that Rorty attacks. Even if there are other projects out there that escape Rorty's attack, *this* project should certainly strive to avoid or answer Rorty's critiques. So, accepting for now that the target is worthy, I now turn to a sympathetically critical reconstruction of the objections Rorty brings to bear upon it.

### 6.2.2 Rorty's Critique of Global Epistemology

In addition to the work Rorty does to show that epistemology<sup>8</sup> is an optional pursuit, he musters a variety of different objections to pursuing it. The importance of these objections in Rorty's anti-epistemological project is not always clear; Rorty often represents his aims as merely showing that a certain way of thinking about knowledge, doing philosophy, etc. is optional (e.g., Rorty (1979, 136)). Clearly, however, this would be insufficient for Rorty's project; he means not only to argue that it is *possible* to give up epistemology, but that it is *desirable* to do so,<sup>9</sup> and to do this it is not sufficient to show that epistemology is optional; he must indicate the ways in which doing epistemology is undesirable, in order to encourage the decision to move away from it. I have identified several such lines of objection, which I will here attempt to reconstruct.<sup>10</sup>

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<sup>8</sup>Hereafter, I will refer to 'Global Epistemology' as merely 'epistemology' with the qualification understood unless otherwise stated.

<sup>9</sup>Though, always careful not to eat his own tail, he never insists that we *must* give it up, that rationality *requires* it, etc.

<sup>10</sup>All of these objections merit more critical attention than I have space for here. Unfortunately, I offer little more than a laundry list of objections. I should just mention that I agree with commentators like Haack who argue that Rorty presents his attacks against Epistemology as a whole,



## The Objection to Privileged Representations

Perhaps Rorty's most forceful attack on epistemology is that it depends on concepts like the given, the necessary, the analytic, and other suspect notions that have to do with privileged representation. This objection receives top billing in *Mirror*, and the main protagonists in Rorty's explication of the objection are Quine and Sellars. The standpoint common to these authors is *epistemological behaviorism*, which Rorty defines as "Explaining rationality and epistemic authority by reference to what society lets us say, rather than the latter by the former" (Rorty, 1979, 174). From the point of view of epistemological behaviorism, it is fruitless to attempt the kind of explanations of justification that traditional epistemology has tried to give.

Something of an ambiguity runs through Rorty's discussion of epistemological behaviorism. Sometimes, it seems that Rorty is only arguing that we *can* be epistemological behaviorists. This is major progress, but, as we've seen, this would not be enough. Rorty must be arguing at least that we *should* be epistemological behaviorists.<sup>11</sup> Rorty must argue not only that privileged representations are part of one possible view, but that that view is problematic.

Rorty definitely takes this step. After all, Quine argues not that the notions of meaning, necessity, and analyticity are optional, but that they are problematic, explanatorily inefficacious, and probably untenable. Sellars argues similarly that the given is a mistake, that the attempt to give an account of "epistemic facts" is akin to the naturalistic fallacy in ethics (Rorty, 1979, 180n). While Rorty wants to soften the blow of the arguments against using representational concepts when it comes to giving *causal* explanations (Rorty, 1979, 177),<sup>12</sup> he retains the arguments against

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while the damage they do is often more limited. Nevertheless, it seems to me that together, Rorty's objections provide a serious challenge to most traditional approaches to epistemology. Furthermore, while one could try to soften the impact of each of these criticisms, it seems to me that a suitably pragmatist approach could avoid all of them, anyhow.

<sup>11</sup>Rorty should acknowledge this point when he asks whether behaviorism begs the question (Rorty, 1979, 175).

<sup>12</sup>One reason that Rorty may be committed to this view is the simple reason that the nature of

them when it comes to giving justifications for beliefs.

### The Normativity Objection

Rorty argues that epistemology confuses causal explanation and justification. The rise of epistemology in the modern period is the attempt to answer the demands for justification of practices, particularly of science, with pseudo-mechanical (pseudo-)causal explanations of the production of grounded knowledge, but neither the pseudo-mechanical causal explanations of modern epistemology nor the explanations from empirical psychology that some philosophers hope to replace them with can provide such justification. In Sellars' terminology, it confuses the 'space of causes' and the 'space of reasons' and commits an error analogous to the naturalistic fallacy.

The core point that Quine and Sellars share, according to Rorty, is the irreducibility of norms and justifications to facts and explanations (Rorty, 1979, 180). To understand the basic thrust of the objection, consider Plato's stories about wax blocks and aviaries, or the host of pseudo-mechanical theories of knowledge from Locke to Kant, or the mystical light of reason in rationalist epistemologies. How can any of these help you in an attempt to justify your knowledge or cement your status as a knower. Knowledge is a matter of public justification, not private mental processes. Even if these were adequate theories of belief-formation, they would have nothing to do with the social activities of justification that have to do with knowledge. One can either theorize about the way beliefs are produced (as Quine did), or about social practices of justification (as Sellars did, in Rorty's view), but to run the two together will produce nothing but confusion.

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humans or minds (or knowledge?) cannot be determined *a priori*.

## The Objection to Conversation-Stopping Certainty

According to Rorty, epistemology seeks total certainty, an end to the conversation, the point where there is no longer any need to engage in inquiry. On one level, this attempt is simply absurd in that it is the doubly useless attempt to answer the skeptic: it is useless on one level because it is impossible; it is easy to see that debating the skeptic is useless, as he will either lead you to an infinite regress, or to justifying something on the basis of something unjustifiable, both of which are absurd.<sup>13</sup> It is also useless because it is pointless; it is simply a mistake to take the skeptic seriously, as Peirce showed us.<sup>14</sup> While it is of course useful to question certain beliefs and practices, it is fruitless to try and doubt all of one's beliefs at once. Without the need to answer the skeptic, Rorty claims, the need for epistemology dissolves, and all that remains is acceptance or measured criticism of one's local pattern of justification.

There is a deeper objection here. Apodictic certainty is objectionable not merely because it is absurd but because, in a certain sense, it is morally objectionable. Rorty argues that "proposals for universal commensuration through the hypostatization of some privileged set of descriptions," that is, foundational languages and sets of facts, have the tendency to be conversation-stoppers. The danger Rorty sees here is nothing less than the "freezing-over of culture. . . the dehumanization of human beings," that human beings will be seen as objects rather than subjects. Rorty cites with approval Lessing's choice of "infinite striving for truth over 'all of Truth' " and of Kierkegaard's choice of "subjectivity" over "system." Rorty sees the continuation of conversation as a much better goal for philosophy (Rorty, 1979, 377-8), and the goal of certainty and complete commensuration as dangerously deceptive.

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<sup>13</sup>On the absurdity of the later, e.g., see Rorty (1979, 361).

<sup>14</sup>E.g., "Some Consequences of Four Incapacities," "The Fixation of Belief" [CITE]

## The Objection to Cognocentrism

Rorty points out that knowing is just one human activity among many:

Normal scientific discourse can always be seen in two different ways—as the successful search for the objective truth, or as one discourse among others, one among many projects we engage in. (Rorty, 1979, 382)

The former point of the view is the myopic view that normal science takes towards itself in its normal practice. But we are not always engaged in the normal activity of a normal discourse, and to recognize this is to begin to ask questions external to those normal practices, evaluating those projects and situating them relative to other projects. Positioning the practices of science as completely overriding is a form of domination. Rorty sees the domination of one set of practices by another as undesirable as well as unjustifiable, as a repressive tyranny, and seeks to replace it with a vision of culture where the many different human pursuits are treated as equally valuable.

This, of course, doesn't require that we allow *all* human pursuits (murder, genocide, credit-default swapping) to count as equally valuable, nor that we allow certain things to *pretend* to be science when they clearly aren't a part of that kind of discourse. Rather, I think Rorty's point is that there are other projects besides the attempt to know or seek the truth which (a) cannot be reduced to the cognitive project, and (b) are just as important: politics, poetry, and art, for example. Epistemology contributes to the idea that the main, most important, or only way that humans have of encountering the world is through *knowing* it. This is the idea that Rorty seeks to dethrone.

## The Transcendental Overseer Objection

Epistemology does not stop with merely the opinion that scientific projects override other projects. Epistemology sees itself as aiming at special knowledge that

will allow it to oversee inquiry and all of culture. It aims at the special knowledge that allows it not only to privilege science over other projects, but to act as the judge of good and bad science. Epistemology sets up philosophy as “the discipline which adjudicates the claims of science and religion, mathematics and poetry, reason and sentiment, allocating an appropriate places to each” (Rorty, 1979, 212). It puts the philosopher in the role of

the cultural overseer who knows everyone’s common ground—the Platonic philosopher-king who knows what everybody else is really doing whether *they* know it or not, because he knows about the ultimate context...within which they are doing it. (Rorty, 1979, 317-8)

But inquiry and culture don’t need such overseers. In fact, the attempt to provide such an overseer is likely to do more harm than good:

[E]pistemology—as the attempt to render all discourses commensurable by translating them into a preferred set of terms—is unlikely to be a useful strategy...the Whiggish assumption that we have got such a language blocks the road of inquiry. (Rorty, 1979, 349)

The assumption of a neutral language in which all legitimate discourse can be translated is in conflict with the possibility that we may want to change the language of our explanations, which is just a “special case of the permanent possibility of someone’s having a better idea” (Rorty, 1979, 349). Epistemology shuts itself off from this possibility, as it shuts itself off from continued conversation.

### **The Stagnation Objection**

Despite its high aims, epistemology makes no progress. It is enmeshed in ‘eternal’ problems, yet reaches no agreement on answers to those problems. Instead, we have the unsettled argument between different schools. For Rorty, it is not hard to see why. For the questions that epistemology asks, like “Why is science so powerful?” or “How does our knowledge approximate the truth?,” no one knows what a good answer would be like (Rorty, 1979, 341). As Rorty points out, it is

a matter of brute fact [that] there is no such thing as a ‘language of unified science.’ We have not got a language which will serve as a permanent neutral matrix for formulating all good explanatory hypotheses, *and we have not the foggiest notion of how to get one.* (Rorty, 1979, 348-9, emphasis mine)

Given the apparent hopelessness of the pursuit, which is of a piece with the hopelessness of answering the skeptic, and given that the pursuit is itself optional, it seems clear that one’s time would be better spent elsewhere.

### **The Existentialist Objection**

According to Rorty, epistemology attempts to avoid one’s responsibility for choosing one’s projects. He takes the point from Sartre: “Sartre... sees the attempt to gain an objective knowledge of the world, and thus of oneself, as an attempt to avoid responsibility for choosing one’s project” (Rorty, 1979, 361). To gain objective knowledge of one’s self is to see oneself as an object. Epistemology attempts to ground one’s choice of vocabulary, and ultimately one’s choice of attitude, in facts about the objective self and its relation to the world, a grounding that will force assent. If we can see objectively the relation between the self and the world, and see that one sort of relation allows selves to get the world right, then it is incumbent upon us to bring our practices into line with this relation. But this obscures the fact that “to use one set of true sentences to describe ourselves is already to choose an attitude toward ourselves, whereas to use another set of true sentences is to adopt a contrary attitude” (Rorty, 1979, 363-4). It helps us to pretend that seeing ourselves as “knowers of true sentences” can be separated from choosing our lives, our actions, our projects. But to adopt a discourse is to choose a project for ourselves, to commit ourselves to some view of ourselves and our projects, and to commit ourselves to the sorts of normative allegiances that go along with consciously and conscientiously adopting a practice. To adopt universal standards in an attempt to see the situation otherwise is bad faith.

I think it is important to keep in mind that while some of Rorty's critique takes aim at *foundationalist* epistemology, Rorty does not conflate epistemology and foundationalist epistemology. What Rorty is concerned with includes all universalist, ahistorical epistemologies that might be concerned with commensuration. So, while foundationalist epistemologies have the biggest difficulties, certain non-foundationalist epistemologies that privilege certain categories of representations (e.g., logical truths), that privilege noetic or scientific activity, that seek certainty or a method of commensuration, that make justification private, etc. will still fall under Rorty's attack.

We will return to these objections at the end of the chapter, when we try to distill some general considerations for a theory of knowledge that will avoid the criticisms of Rorty.

### 6.2.3 Hermeneutics as an Alternative to Epistemology

If we accept Rorty's objections to Global Epistemology, and agree with him that it is a project we should no longer engage in, then we will be left with a vacuum: what can be said about knowledge in general? Rorty seeks to fill the vacuum with *hermeneutics*.<sup>15</sup> He argues that instead of seeking certainty, universal commensuration, and grappling with perennial problems, we should aim at continued conversation, mutual enlightenment and understanding (in a non-epistemic sense), seeing incommensurable vocabularies as genuine alternatives and seeing many different projects as valuable. Hermeneutics is an expression of hope for openness replacing the goal of ending conversation with certainty.

Having said this, I should be clear that Rorty does *not* see hermeneutics as a successor discipline to epistemology. The reason is twofold. On the one hand, hermeneutics is not meant to fill the "cultural vacancy" left by epistemology; it provides no commensuration of diverse discourses, no certainty, no substantive doctrines

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<sup>15</sup>Which is a term of art for Rorty that may diverge greatly from the common use.

about knowledge. On the other hand, hermeneutics is not even a discipline; it has no method; it is not a research program (Rorty, 1979, 315). But it does fill the vacuum in a certain sense; it does *confront* the questions about knowledge in general. Unlike epistemology, hermeneutics does not attempt to give a positive answer to these questions. Its response is *Mu*, entirely negative. Hermeneutics suggests we un-ask the question and is in a sense an anti-theory of knowledge. All it leaves us with is encouragement to openness and innovation, to break down conventions that might get in the way of the path of inquiry.

Is hermeneutics enough? Rorty seems to return us to a pre-Platonic phase where we can say nothing about knowledge in general. When the question “What is knowledge?” is posed, Rorty would seemingly have us answer, along with Theaetetus, with a heterogeneous list of prototypes of knowledge.<sup>16</sup> While it is possible that this is all we can say, it isn’t obvious or *a priori* that it is so, and Rorty has only ruled out one sort of answer to the question. David Hildebrand captures the unsatisfactoriness of Rorty’s displacement of epistemology by hermeneutics well:

Rorty’s goals for the philosopher are unobjectionable but, I think, somewhat emasculated. They repair to a conception of inquiry that is . . . simplistic and unambitious. . . this portrait of inquiry underestimates the spectrum of problematic situations we all face. (Hildebrand, 2003, 102)

Perhaps there are some general features of inquiry worth considering. Perhaps we can say interesting and useful things about knowledge-in-general, yet with sufficient modesty to evade Rorty’s attack. In the following chapters, I will attempt to show the outlines of a theory that will do just that, relying in part of the work of John Dewey. In order to sharpen Rorty’s critique, then, I will examine Rorty’s critique of Dewey’s own epistemological work.

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<sup>16</sup>See Plato’s *Theaetetus*, 146c.



## 6.2.4 Rorty's Critique of Dewey

### Dewey on Inquiry and Epistemology

Dewey had a complex relationship with 'epistemology.' As Larry Hickman points out, "Dewey did not develop a theory of knowledge in the usual sense of 'epistemology,' but he did have a well-developed theory of inquiry." Dewey shares many of Rorty's misgivings about traditional epistemology, and he sees the root of the problem in the traditional epistemologist's mixing of useful logical tools with psychological and metaphysical baggage. An adequate theory of inquiry would be rid of such baggage, and 'logic' and 'epistemology' would thus become synonymous as the theory of inquiry (Hickman, 1998, 166).

As we've seen, Dewey's logic gives us a theory of the pattern of successful inquiry and the logical forms and methods that are a part of it. Dewey's logic is a naturalistic theory. This means that it proceeds, not *a priori*, but in the same way as a scientific inquiry, namely, by empirical investigation into activities of inquiry (LW 12: 26).<sup>17</sup> As Hilary and Ruth Anna Putnam put it, "Logic as the theory of inquiry is itself the result of an inquiry" (Putnam and Putnam, 1992, 41). It is an inquiry into inquiry, with the goal of discovering the conditions and patterns of success in inquiry. While Dewey stresses the "continuity between operations of inquiry and biological operations and physical operations" (LW 12: 26), he also stresses that inquiry is an irreducibly *social* activity, and thus he terms his naturalistic conception of logic "*cultural naturalism*" (LW 12: 26-8). It is similar in certain respects to a social science like economics.

In earlier chapters, I have elaborated and defended some of the basic fea-

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<sup>17</sup>According to standard practice, references to John Dewey are parenthetical citations to the critical edition, *The Collected Works of John Dewey, 1882-1953*, edited by Jo Ann Boydston (Carbondale: Southern Illinois UP, 1969-1991), cited according to sub-collection (*The Early Works: 1891-1898 (EW)*, *The Middle Works, 1899-1924 (MW)*, and *The Later Works, 1925-1953 (LW)*). Citations are made with these designations followed by volume and page number, along with essay or manuscript title where this is not clear from context.

tures of Dewey's theory of inquiry. In understanding and responding to Rorty's critique, three major theses of Dewey's logical theory are important: (i) inquiry is the intelligently controlled transformation of an indeterminate situation into a settled situation; (ii) logical forms arise from and control inquiry for certain subject-matters; and (iii) ideas and facts are functional distinctions within inquiry which carry out different functions in that inquiry, rather than absolute epistemological or ontological categories.

**The Role of Reflective Inquiry.** Recall the basic definition of inquiry:

*Inquiry* is the controlled or directed transformation of an indeterminate situation into one that is so determinate in its constituent distinctions and relations as to convert the elements of the original situation into a unified whole. (LW 12: 108, emphasis mine)

The goal of inquiry is a controlled transformation of this indeterminate situation into a unified situation. The transformation is crucially *of the situation*, not merely a subjective change in the inquirer:

Since these operations [i.e., operations of inquiry] are existential they modify the prior existential situation. . . The transformation is existential and hence temporal. The pre-cognitive unsettled situation can be settled only by modification of its constituents. (LW 12: 121)

Experimental operations are necessary to change the existing conditions, as reasoning only supplies the means for changing conditions, but cannot do it on its own (LW 12: 121). The unified situation is one in which the doubtfulness and uncertainty is settled, where the inquirer has a clear course of action and the constituents of the situation are related to each other in a more meaningful way. It is one in which equilibrium has been restored.

**Logical Forms in Inquiry.** Dewey describes the fundamental thesis of *Logic: The Theory of Inquiry* as follows: “Logical forms accrue to subject-matter when the latter are subjected to controlled inquiry” (LW 12: 105). What this means is that logical forms arise within or originate from the operations of inquiry, and are used as tools to control and direct further inquiry so that it produces the warranted assertions that resolve the indeterminate situation. It is important to note that, as Ernest Nagel points out in his Introduction to the *Logic*, that “logical forms” for Dewey are not merely the syntactical structure of statements, but more a function that directs inquiry (LW 12: xx). We do not read the logical structure off the world, nor do we impose it on the world *a priori*. Subject-matters come to have the logical structure they do in virtue of being subjected to continued inquiry, just as certain advanced tools and techniques accrue to certain industries as a result of continuing development and production. In fact, Hickman (1992), following Hook (1996), argues that seeing inquiry as a kind of technology is the most crucial metaphor for understanding Dewey’s work.

So, in the course of inquiries, subject-matters acquire logical forms that are then used to control further inquiries. Logic is the discipline of inquiry into inquiries which attempts to identify the best methods, principles, or logical forms of inquiry available at a given time. The best methods of inquiry are identified on the basis of their success in guiding continued inquiry (LW 12: 21). Logic concerns itself with investigating the logical forms of successful inquiries, with an eye to the conditions that affect success in those particular inquiries and to whatever patterns may be identified in successful inquiry.<sup>18</sup> In future inquiry, new methods will be invented, new logical forms will accrue, and thus logical theory will change (LW 12: 22).

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<sup>18</sup>Hildebrand makes this point clear (Hildebrand, 2003, 93), and it will become important in understanding whether Dewey’s ambitions amount to what Rorty identifies.

**Ideas and Facts.** Two major functional distinctions in Dewey's logical theory are facts, percepts, or existences on the one hand and ideas or concepts on the other. These two logical forms are important because they play a role in all processes of inquiry. Dewey thinks that Kant's fundamental logical insight was that concepts and percepts cooperate, that they are both necessary, but he thinks that Kant misunderstood them in seeing them as products of different sources requiring synthesis. He describes their logical relationship in the Kantian terminology:

In logical fact, perceptual and conceptual materials are instituted in functional correlativity with each other, in such a manner that the former locates and describes the problem while the latter represents a possible method of solution. . . Both are finally checked by their capacity to work together to introduce a resolved unified situation. As distinctions they represent logical divisions of labor. (LW 12: 115)

Concepts and percepts are not ontologically given distinctions, they are distinctions that are taken as functional within a specific inquiry.

Dewey usually describes the division in terms of 'observed facts' and 'ideas' (or 'ideational contents'). Dewey switches to this vocabulary to describe the relationship of these two forms in greater detail:

It was stated that the observed facts of the case and the ideational contents expressed in the ideas are related to each other, as, respectively, a clarification of the problem involved and the proposal of some possible solution; that they are, accordingly, functional divisions in the work of inquiry. Observed facts in their office of locating and describing the problem are existential; ideational subject-matter is non-existential. How, then, do they cooperate with each other. . . ? . . . both observed facts and entertained ideas are operational. Ideas are operational in that they instigate and direct further operations of observation; they are proposals and plans for acting upon existing conditions to bring new facts to light and to organize all the selected facts into a coherent whole. (LW 12: 116)

So both ideas and facts are functional aspects of particular inquiries. Facts are aspects of the situation taken as conditions with which the inquiry at hand must

deal. Facts clarify the problem for inquiry and provide the known resources and elements that can be brought together to solve the problem. Taking Dewey's example of a fire alarm in a crowded assembly hall (LW 12: 112-3), the facts of the case, or the observed factual conditions, include some information about the fire (e.g. its location), the fixed aisles and exits, and the evolving reaction of the crowd. These facts are settled by observation, and are taken (not given) as conditions for the purposes of the inquiry, namely, for the purposes of resolving the indeterminate situation raised by the fire alarm.

Once the facts are determined, certain possible relevant solutions or *ideas* present themselves. "Ideas are anticipated consequences (forecasts) of what will happen when certain operations are executed under and with respect to observed conditions" (LW 12: 113). Ideas identify possibilities and make predictions, suggesting observational operations and ways to interpret their results. As an inquiry progresses towards resolution, and more facts are brought to light, ideas become less vague and some are ruled out as going possibilities. Returning to the fire example, at the start, several possibilities present themselves: one may stay and get trampled, one may go for the exits. Determination of further facts may make some ideas less vague (one can construct the route to the exit), may close off some possibilities (the exits are blocked by the panicked crowd), and new ideas may present themselves (grabbing and using a fire extinguisher to force a new way out).

It is important to remember that neither facts nor ideas are absolutely given. Some facts may be settled prior to the inquiry at hand; one may already have a working knowledge of the layout of the building. The facts are taken, not given, as conditions for the inquiry, and the factual conditions are revisable within the inquiry in light of new information. Even when the facts stand as unquestioned conditions or as evidence, this should be understood to have logical, not metaphysical import; it is only a matter of their functional role in inquiry. Ideas are suggested by the determined factual conditions and by the creative efforts of the inquirers; they begin

life as suggestions that simply spring up or occur to us in a flash (LW 12: 113-4). The two work together to move inquiry towards resolution.

**The Big Picture.** We should now step back and ask just what sort of theory Dewey's logic is. It should be clear that his logic is a general theory of knowledge. Dewey provides a naturalistic yet normative theory of logic and inquiry, which provides a characterization of the tools and methods of inference, experimentation, and reflective reasoning that tend to result in successful solutions to problems of inquiry. Furthermore, he provides a larger framework for a naturalistic understanding of the problem-solving processes that are the basis of cognitive inquiry. He provides a unified framework for three previously separate subjects: traditionally *logical* subjects like inference, propositions, judgments, and logical forms; *epistemological* subjects like warrant and justification, ideas and facts; and issues of *scientific methodology* including the role of reason, observation, and experimentation, as well as causation and causal reasoning. The *Logic* provides impressive, detailed discussions of all of these questions in one powerful framework.

### Dewey's Answers to Rorty's Critique

I will now address each of Rorty's objections from the Deweyan perspective, showing how Dewey's theories avoids the pitfalls of traditional epistemology.

**Privileged Representations.** Dewey himself attacks the given and the *a priori*. He argues at length that all knowledge is mediate (LW 12: 142-60), and the nonexistence of privileged representations comes out as a consequence of his theory. Neither logical theory nor knowledge depend on anything that is immediately given or completely *a priori*, though inquiries can take advantage of facts that are *taken* as

settled in previous inquiries, and principles that are *operationally a priori*<sup>19</sup>—habits that have proven useful in previous inquiries, taken (tentatively) as guiding principles for the present inquiry. Any epistemic privilege is functional within a particular inquiry and for certain purposes, but there is no absolute basis of epistemic privilege.

**Normativity.** Dewey might be said to confuse descriptions and norms, or, alternatively, to fail to capture any normativity (a criticism that has fallen on Quine’s Naturalized Epistemology, perhaps wrongly). This criticism is mistaken. Logical theory is a descriptive study of successful inquiry, the study of the patterns and principles that are successful tools in inquiry, and the conditions of their success. It carries with it the same normativity with respect to inquiry as the study of successful farming to farming practices, study of medical techniques to medical practice, or studies of economics to behavior in the marketplace.<sup>20</sup> Dewey is unsympathetic to exaggerated worries about justification: “Men think in ways they should not when they follow methods of inquiry that experience of past inquiries shows are not competent to reach the intended end of the inquiries in question” (LW 12: 107). This is all one needs with respect to justification of methods of inquiry.

**Certainty.** Dewey holds that science is a fallible enterprise, and he presents his theory as fallible in the way any scientific theory is. Dewey seeks neither to ground any one discourse in absolute certainty, nor to provide universal commensuration of all discourses. Dewey’s aim is more modest: he hopes to provide an account of logical tools which have proved useful in previous inquiries.

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<sup>19</sup>Not unlike what Foucault called the “historical *a priori*” and what Kant called the “*a priori secundum quid*.”

<sup>20</sup>Though, to the degree that such applications are actually *problematic*, Dewey’s framework may provide helpful resources with dealing with those problems. This is an interesting issue for further research.

**Cognocentrism.** Dewey recognizes that knowing is just one sort of experience, and claims that all knowledge starts from and ends in non-cognitive experience. The critique of cognocentrism is a fundamental principle in Dewey's philosophy, if anything is. Dewey *does* want to recommend the methods of successful inquiry in the case of a problematic situation, but he would deny that the resolution of problematic situations by reflective inquiry exhausts human activity.

**Stagnation.** Dewey's approach to logic makes it a progressive discipline in two ways: (i) The study of inquiry improves like any empirical study, i.e., the techniques and results of inquiry into inquiry improve with time as do the techniques and results of inquiry into physics. (ii) The logical forms of inquiry improve with the continuation of particular inquiries. "When in the future methods of inquiry are further changed, logical theory will also change" (LW 12: 22). As techniques in particular primary inquiries improve with continuation of those inquiries, those new tools can be studied by inquiry into inquiry. Just as we now use cars instead of horse-drawn carriages, we now have more useful tools for inquiry than, for instance, Aristotelian syllogism.

**Transcendental overseers.** The laws, methods, forms of inquiry are not transcendental, they arise out of inquiry. They control inquiry insofar as they are the forms of successful inquiry, but they are also mutable and fallible, so if they do not work for a certain subject-matter, we revise them rather than denigrating the subject-matter. The methods of inquiry are tools to aid inquiry, not gate-keepers that attempt to sort good inquiry from bad. Furthermore, knowledge-making is recognized as one among many human activities, and though it can be useful in directing some of them, it is not the judge of all activities.

**Authenticity.** Is it bad faith for the doctor to choose a procedure because it has shown to be successful in treating patients? If not, then it is no more inauthentic to adopt the methods of successful inquiry.



## Another Attack from Rorty

So far, I have argued that Dewey's theory of inquiry is not vulnerable to the same objections Rorty raises against traditional epistemology, and I have suggested that it is the sort of positive theory that could fill the vacuum left by Rorty's critique, where Rorty only sees room for hermeneutics, which is entirely negative, an anti-theory. But Rorty has also engaged in a sustained critique of Dewey's logic, which I will now address.<sup>21</sup>

Sometimes Rorty represents Dewey's theory of inquiry as not just wrong, but as fundamentally at odds with Dewey's pragmatism. This gives real force to the question, Can Dewey provide a philosophical theory of knowledge? Is such a theory at odds with Dewey's most worthwhile and fundamental commitments? Rorty argues that it is. But this is a counterintuitive claim given the usual characterizations of pragmatism. For example, Louis Menand writes that "Pragmatism is an account of the way people think" (Menand, 1997, xi). If this were true, it would seem a mistake to say that pragmatists were forbidden from having a theory of inquiry or "reflective thinking," as Dewey sometimes called it.<sup>22</sup>

One of Rorty's most concise and powerful criticisms of Dewey's theory of inquiry appears in his introduction to Volume 8 of *The Later Works* (Rorty, 1986).<sup>23</sup> Here Rorty presents his major lines of argument against Dewey: (i) There is no room between general platitudes and the practices of specific disciplines for Dewey's

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<sup>21</sup>Rorty engages in two sustained critiques of Dewey: one against his theory of logic and scientific method, the other against his metaphysics, especially as seen in Dewey's *Experience and Nature*. I'll only consider the first, here.

<sup>22</sup>Rockwell (Spring 2003), along similar lines, has argued at length that Rorty is wrong to reject Dewey's epistemology, that he argues by confusing the failure various answers given by traditional philosophy for the failure of the questions, and that it is undesirable and impossible to avoid having an epistemology at all. While I don't agree on all of the finer points of Rockwell's interpretation and critique of Rorty, I do agree with the general sweep of his argument, and in some ways his response is complementary to my own.

<sup>23</sup>As far as I am aware, the only Dewey scholar to engage with this particular work is Hartmann (2003). For other Dewey scholars defending Dewey's theory of inquiry against Rorty, see Hickman (1998); Hildebrand (2003); Rockwell (Spring 2003); and Saatkamp (1995, Ch. 3,5).

theory. At most he is merely recommending an experimental and critical *attitude*. (ii) Rorty argues with Feyerabend that no method is the best method. (iii) Dewey is hiding his ideological agenda by pretending that his logical theory is a neutral characterization of successful thinking, hiding its liberal-political motivations. (iv) Rorty accuses Dewey of scientism, claiming that Dewey overdid his esteem of the scientist as exemplar of inquiry. I will argue that a Deweyan approach has the resources to resist all of these objections, but this discussion will help us to keep in mind a number of important considerations for formulating an epistemological project.

**Against Method.** Rorty's first two criticisms are related; the tension Rorty sees in Dewey is a conflict between the anti-method Dewey who thinks that recipes for inquiry will inevitably become restrictive, and the pro-method Dewey who wants to show people how to think or inquire better:

[Dewey] wants, on the one hand, to claim that most attempts to specify a "method for correct thinking" have merely hypostatized the vocabulary and practises of a certain period or of a certain preferred area of culture. But, on the other hand, he does not wish to conclude (as such recent writers as Paul Feyerabend have concluded) that the way to encourage experimental thinking is to give up the very idea of "method" as an outdated shibboleth. He is torn between the temptation to say that the only rule of logic we require is Peirce's "Do not block the road of inquiry!" and the need to lay out some procedures which, if adopted, will improve people's thinking. (Rorty, 1986, xiii-xiv)

Rorty thinks that Dewey is in an impossible situation. Rorty's view is that there are no methodological claims that are neither so specific as to be simply part of a Kuhnian "disciplinary matrix" nor so general that they are trivial (Saatkamp, 1995, 218n), a view he inherits from historicist philosophers of science like Kuhn and Feyerabend.

This view is suspect on two fronts: on the one hand, it pays insufficient attention to Dewey's historicism, which he retains when doing logical theory, and,

on the other, it overstates the lessons that Kuhn and Feyerabend teach.

Consider Feyerabend, at his most radical in *Against Method*:

there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: *anything goes*. (Feyerabend, 1993, 18-9)

Rorty seems to take Feyerabend's message here to be that we should proceed without method. But this is the same sort of misinterpretation of Feyerabend's position made by most of the critics of *Against Method*:

[In *Against Method*] I argue that all rules have their limits and that there is no comprehensive 'rationality', I do not argue that we should proceed without rules and standards... I suggest a new relation between rules and practices. It is this relation and not any particular rule-content that characterizes the position I wish to defend. (Feyerabend, 1979, 32-3)

Feyerabend's argument is that there is no method that works "under *all* circumstances and in *all* stages of human development," not that there is nothing interesting we can say about method in general.

What is Feyerabend's suggestion for a relation between method and practice? He suggests that method is "a guide who is part of the activity guided and is changed by it" (Feyerabend, 1979, 33). But this is *remarkably* like Dewey's own position. Remember, Dewey holds that logical forms (methods, principles, etc.) originate in and are changed by inquiry, as well as being used to control inquiry. Far from providing grounds for a criticism of Dewey, Feyerabend is engaged in a very sympathetic project. Feyerabend attacks the philosopher who wants to impose universal, ahistorical rules on science from the outside, not anyone who wants to say anything about methodology.

Dewey is engaged in such a suitably modest project, as Hildebrand points out in *Beyond Realism and Anti-Realism*:

Dewey does not expect to be able to determine, generally and in advance, a specific algorithm for inquiry *per se*. It is a historical fact that all sorts of methods have been tried in various contexts and with various results. Dewey's claim, it seems to me, is that one *may* investigate these methods to understand (1) how conditions affected their success or failure and (2) whether patterns are present in such occurrences without *necessarily* intending the aim of such an investigation to be the determination of a Final and True Method. (Hildebrand, 2003, 94)

Dewey's goal is to investigate the conditions under which particular methods have proved successful, and to look for patterns shared by such cases. He hopes that this investigation will yield general tools that may be helpful in the prosecution of further inquiries.<sup>24</sup> What he does *not* aim at are final rules that will restrict inquiries. He aims at recommendations, not restrictions. Dewey's method is a tool to be used where effective, not a weapon to enforce a certain way of doing things. For Dewey, the acceptance of or the value of an idea is ultimately dependent upon its practical value,<sup>25</sup> and methods are acceptable only insofar as they are efficacious to that end. Taking method as legitimating ideas would be to get things exactly backwards. And as Hickman points out, Dewey's theory of knowing pays close attention to the specific context of that activity: "He argued that knowing is characterizable only relative to the situations in which specific instances of inquiry take place, and that it is an artifact produced in order to effect or maintain control of a region of experience that would otherwise be dominated by chance" (Hickman, 1992, xii). Furthermore, to say that Dewey's results are trivial is unfair, given that Dewey's work in the field is preliminary on his own admission. It would seem hasty indeed to reject the entire pursuit based on the mere fact that the results he got were modest.

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<sup>24</sup>See Hickman (1992, xii-xiv).

<sup>25</sup>The reader should be careful not to construe "practical value" too narrowly. In the case of inquiry, the practical value has to do with whether it leads the inquiry towards resolution, solves the problems, returns the situation to equilibrium, and produces warranted assertions. It has nothing to do with whether the inquiry is into building bridges or high-energy physics.

Larry Hickman, who has aptly characterized Dewey's theory of inquiry as treating inquiry "as a productive skill whose artifact is knowing" (Hickman, 1992, xii), highlights the specificity of knowing in Dewey's view:

He argued that knowing is characterizable only relative to the situations in which specific instances of inquiry take place, and that it is an artifact produced in order to effect or maintain control of a region of experience that would otherwise be dominated by chance. Knowing is thus provisional: when conditions change, further inquiry may be called for if control is still required. . . the goal of inquiry is not epistemic certainty. . . but instead a matter of ongoing interaction with novel situations by means of constantly refashioned artifactual tools. Unlike most philosophers of technology, Dewey held the view that technological instruments include immaterial objects such as ideas, theories, numbers, and the objects of logic. . . He argued that one of the great impediments to successful inquiry is the taking of the tools he termed inference, implication, and reference as entities existing in their own right prior to inquiry. (Hickman, 1992, xii-xiv)

This is far from the view of Dewey's epistemology that Rorty has.

The confusion in Rorty's critique is a common confusion of two different stances towards methodology. Laudan (1989) makes the same confusion from the opposite direction in his critique of Feyerabend. Feyerabend's main targets in his attacks on method, logical positivism, critical rationalism, and related philosophies of science, take strictures of scientific method to be gate-keepers for what counts as meaningful or what counts as science. Along with Feyerabend's usage, we might call such approaches to method *rationalist*. On the other hand, the approach to method shared by Feyerabend's positive remarks, Dewey, and Laudan takes method as generally useful heuristics or tools in inquiry, which have a dialectical relationship to the practices they guide. We might call such an approach *pragmatist*. Laudan confuses the two in his attack on Feyerabend, confusing his own position with the position under attack, when in fact it is very close to Feyerabend's own positive

stance towards method.<sup>26</sup>

**The Ideology of Dewey's Method.** In his introduction, Rorty suggests that there is a tension in Dewey between two public images, between two rhetorical styles, and that this tension undercuts his project in works like the *Logic*. In one mood, Dewey is the philosopher as activist, where he presents his philosophical views as instrumental to advancing sociopolitical projects. In another mood, Dewey is the philosopher as sage, and he presents his philosophical research as grounding or certifying his project for social reform. Dewey happily moves between these two styles, which is perfectly natural given that he believed in no priority among disciplines nor in any politically or morally neutral subject-matters.

Rorty sees this ambiguity in Dewey as, in a certain sense, disingenuous, on account of the fact that most of Dewey's audiences took for granted things like "the political and moral neutrality of such subject-matters as 'logic' and 'psychology'" (Rorty, 1986, xi). Sometimes, Dewey did not try to appear neutral, but this left him "open to the charge, often made by his enemies, that he is making socialist propaganda and disguising it as a 'philosophical,' and thus presumably neutral, discussion of the nature of thought" (Rorty, 1986, xii). Other times, Dewey seems to adopt a neutral standpoint, discriminating better from worse ways of thinking from a lofty philosophical perspective.

On the one hand, Rorty sees Dewey as saying that seventeenth century science discovered not just better theories, but a new method of inquiry, new tools for thinking that could help us in all areas of thought, no matter what our aims or political views. To Rorty, Dewey seems to say:

"All of us, no matter whether we would prefer a more religious or a more secular culture, or whether we are politically radical or politically conservative, naturally want to use the best possible tools in our work. The

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<sup>26</sup>Though Feyerabend does criticize Laudan's position later in his career. See §6.3 below. We will return to this distinction between views of method in the following chapter.

method discovered in the seventeenth century is a better, unfortunately neglected, tool. A study of the nature of thought, of how we think, will make the virtues of this tool clear to us" (Rorty, 1986, xii).

On the other hand, Rorty claims that "it seems obvious this tool is much more suited to the secularizing and left-leaning intellectual" (Rorty, 1986, xii-xiii), that the recommendation amounts to not much more than being innovative, experimental, and forward-looking in the way that is much more acceptable to progressive aims. On Rorty's estimation, the only people who do not practice Dewey's recommended "reflective thinking" are "those who are dogmatic, opinionated, unwilling to listen, difficult to converse with" (Rorty, 1986, xv), and these are exactly the sort of people for whom the offer of a better method is inappropriate. It would only be appropriate if the method offered were effective for doing something they already wanted to do, but these people precisely *don't* want to be critical and experimental.

A major problem, according to Rorty, is that Dewey's discussion of method is plagued by an "ambiguity between the descriptive and the normative" (Rorty, 1986, xii). But surely, this claim is bizarre coming from someone who has written on "the trouble with the fact-value distinction" (Rorty, 1979, 363-5).<sup>27</sup> If anything comes close to a central pragmatist doctrine, the principled rejection of the dualism between the descriptive and the normative does, as Dewey explicitly holds (and as Rorty has already mentioned in this introduction!). What is going on, here?

Dewey, as Rorty points out (Rorty, 1986, xi n.), is in the same bind that Protagoras is in when he expounds the wisdom of relativism or Feyerabend when he attacks rationalism in a rational debate. Dewey may occasionally adopt a *rhetorical strategy* that smacks of neutrality, but, after all, "saying something is not always saying how things are" (Rorty, 1979, 371). Dewey fully acknowledges that no description is free from the influence of values, and he no doubt holds this true of his own theory.

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<sup>27</sup>I believe, though I will not argue it here, that Rorty himself is guilty of flip-flopping on the notion of normativity. On the one hand, as in his attack on Naturalized Epistemology, he seems to hold a sharp fact/value distinction. Elsewhere, he seems to reject it in good pragmatist fashion.

Nevertheless, if it is useful to adopt a rhetorical strategy that sounds neutral in order to speak to certain audiences, this is not dishonest. If a pedant criticized Dewey on this point, he could freely admit what he was doing, as did Feyerabend (Feyerabend, 1979, 143) and Protagoras [*Theaetetus*, 166e-167c]. Fear of this objection may lead to the constant qualification Rorty makes about not having views on things.

Dewey might also balk at Rorty's cynicism. Is it really true that people who don't consider themselves liberal *don't want* to be critical and experimental? That they prefer to be dogmatic? Could it really be true that, faced with evidence of real, practical success, conservative or religious types would spurn useful tools? Everyone is faced with problematic situations, and if certain methods can be more helpful than others, will that have no effect? Surely, there will always be stubbornly ignorant people, but this no more undermines the utility of the theory of inquiry than it does any scientific enterprise.

**What's so Special About Science?** Rorty also finds Dewey's adoration of the scientist objectionable:

Insofar as philosophy has "advanced" since Dewey, the advance may consist in the realization that, like the logical empiricists, Dewey overdid the attempt to make the natural scientists a model for the rest of culture. Both were too concerned to isolate a "method of experimental action called natural science" [LW 8: 68]. Both overestimated the differences between science, art, and politics. (Rorty, 1986, xviii)

In Rorty's view, Dewey, like others before him, saw the success of seventeenth century science as at least partly due to their discovery of a new method. But Rorty thinks that no such method is available. Galileo did just what Aristotle did and what all critical, thoughtful people do. The difference is that Galileo had better *ideas* than Aristotle (Rorty, 1982, 193).

It is true that Dewey has a sort of pre-Kuhnian optimism about science, but it would be unfair to treat him as entirely naive about science. Even Kuhn and



Feyerabend admit that science has been successful in *some* sense; insofar as Dewey is interested in successful examples of inquiry, science provides a rich resource. Even Rorty, in ethnocentric fashion, endorses the scientific spirit (Rorty, 1979, 330).

Though Dewey often writes about science's "use and refinement of intelligent methods" (Hildebrand, 2003, 94), it would also be a mistake to see Dewey as making a major distinction between the scientist and the layperson, between science and everyday activity, art, or politics. Here, Rorty "fails to see the forest for the trees" (Hildebrand, 2003, 93). Hildebrand points out the degree to which Dewey saw science and everyday life as continuous:

Typically, science's subject matters are theoretical; they seek "systematic relations of coherency and consistency" between concepts. "Commonsense" subject matters are typically concerned with "direct existential application." But these are differences in emphasis, not in kind. . .

Dewey's work, then, aimed not at discovering *the* scientific method but at explaining how science was epistemologically continuous with other human endeavors. . . (Hildebrand, 2003, 93)

In fact, in works like *Logic: The Theory of Inquiry* and *How We Think*, Dewey often speaks of "inquiry" or "reflective thinking" in general, without special reference to science, and he usually makes key definitions without referring to science at all (Hildebrand, 2003, 94). Though Dewey often urges philosophy to take its stand with science (MW 10: 39), this is an attempt to bring philosophy down to the modest level of science, not the attempt to force all of culture into the laboratory.

While Rorty's criticisms of Dewey provide helpful reminders of the dangers of a project like the ones undertaken by Dewey and myself, Dewey's project does not fall to the criticisms that Rorty levels against it. This is sometimes a failure of Rorty's criticisms, but is more often a result of the fact that Rorty just *misunderstands* Dewey's views and the general project he is undertaking it. He is too quick to assimilate Dewey's project to the old, bad project of traditional epistemology.

### 6.3 Feyerabend Against Epistemology

I have already referred to the parallels and sympathies between Rorty's critique of epistemology and Paul Feyerabend's critique of theories of scientific method in works like *Against Method* (1993), *Science in a Free Society* (1979), and "The Limited Validity of Methodological Rules" (1999). I will now turn to a further critique from Feyerabend of a project like Dewey's and like my own that goes beyond Rorty's discussion. In "The End of Epistemology?" (1994), Feyerabend criticizes Laudan's pragmatist epistemology. He argues that Laudan's project—a job not unlike the one Dewey is trying to do—can only be done by a scientist enmeshed in the practices such a theory would try to guide. Questions like the ones Laudan is concerned with, questions about "how best to construct theories, when to regard a theory as well supported, [and] when to prefer it to a rival" (Feyerabend, 1994, 190) are important, but Feyerabend questions whether they can be answered from the outside:

They are important questions indeed... However, can they be answered by a person who looks at science from the outside; is unaware of its divergent ingredients; lacks the mathematical skills, the judgment and especially the "tacit knowledge" which define an area of inquiry and which are unavailable to those not actively participating in the enterprise?

Feyerabend goes on to discuss many examples from the history of science that demonstrate the massive complexities of the scientific enterprise and the methodological inventiveness of particular scientists, in particular, the lack of methodological unity that characterizes the sciences. This discussion supports his claim that the abstract concerns of an outside cannot hope to control a complex and disunified activity like science.

Feyerabend starkly outlines his skepticism about the epistemological enterprise:

[W]e are led to suspect that scientific research knows no universal boundary conditions or standards whether of a conventional, aprioristic, or empirical kind but uses and invents rules according to circumstance without regarding the selection as a separate “epistemic” act and often without realizing that an important choice is being made. (Feyerabend, 1994, 195-6)

Feyerabend’s scientist is a methodological opportunist, is not and should not be constrained by universal rules. This last point is one that Dewey would largely accept; what he calls “logical forms” arise in the course of inquiry, in response to the changing needs of different inquiries. Though Dewey may differ on how particularistic such tools might be, the fundamental attitude remains the same.

One thing Feyerabend may be saying is that, since science is an extremely complicated enterprise, attempting to study its methods in order to come to any general conclusions is impossible. But that the subject is a complicated one does not necessarily mean that it is impossible. Feyerabend may be right that many philosophers prefer to control things by passing down pronouncements from a Platonic heaven rather than engage with the nitty-gritty details of scientific practice, but to some degree the situation in philosophy of science seems to have improved. Furthermore, there are a number of resources available to the philosophy of science, such as participant observation, historical and ethnographic research that may be able to ameliorate Feyerabend’s worries.

Furthermore, the view that methodology should be left entirely to the scientist fails to meet one of Dewey’s major and most worthy aims, which is not to attempt to control science, or to control culture with science, but to make available to culture at large the logical tools that science has created. It makes problematic or impossible a central concern of this work, which is to find and characterize the continuity between scientific activity and everyday human life. Feyerabend may remind us that this is a difficult affair, but his arguments do not show us that it is an impossible or unworthy task.

# Chapter 7

## Conclusion: Transforming Experience

In conclusion, I will describe what I take to be the main picture of science and its relation to life that comes out of the discussions of Dewey and the relation of the Deweyan framework to contemporary problems.

### 7.1 Science in a Precarious World

For Dewey, scientific inquiry arises from and is continuous with our everyday activities of problem-solving, our attempts to cope with a world that is precarious and uncertain. Peter Godfrey-Smith aptly characterizes Dewey's view:

Thought is a response to the unsettled, the doubtful, the hazardous, the precarious, the indeterminate, the irregular, the uncertain, and so on. Dewey insists that properties like these are real characteristics of environments, not properties imposed on them by thinkers. (Godfrey-Smith, 1996, 107)

In order to understand thought generally, and science in particular, we need to understand that the world we find ourselves in is one of uncertainty and danger. As

Dewey says,

Man finds himself living in an aleatory world; his existence involves, to put it baldly, a gamble. The world is a scene of risk; it is uncertain, unstable, uncannily unstable. Its dangers are irregular, inconstant, not to be counted upon as to their times and seasons. Although persistent, they are sporadic, episodic. It is darkest just before the dawn; pride goes before a fall; the moment of greatest prosperity is the moment most charged with ill-omen, most opportune for the evil eye. Plague, famine, failure of crops, disease, death, defeat in battle, are always just around the corner, and so are abundance, strength, victory, festival and song. (*Experience and Nature*, LW 1:43)

In our discussions above, the precarious nature of the world features in the formation of *indeterminate situations*. Such situations are indeterminate with respect to their *issue*, that is, with respect to how things will turn out and how we should respond to them. The function of thought, of knowledge, of our technoscientific achievements is to respond to this uncertainty, to forge a way through the wilderness rather than leave our fortunes up to mere chance. While the precarious is a generic trait of our environment, for Dewey, so is stability. There are more-or-less dependable structures in the world which we can use to cope with the uncertain ones. It is the distinctive human achievement to have learned to do this in a way that is self-aware and self-correcting.

One of the reasons that Dewey was so opposed to philosophical perspectives that insist on epistemic certainty, universal laws, essences, and the metaphysical determinateness of reality is that it amounts to burying our heads in the sand rather than facing up to our problems.

We have substituted sophistication for superstition, at least measurably so. But the sophistication is often as irrational and as much at the mercy of words as the superstition it replaces. Our magical safeguard against the uncertain character of the world is to deny the existence of chance, to mumble universal and necessary law, the ubiquity of cause and effect, the uniformity of nature, universal progress, and the inherent rationality

of the universe. These magical formulae borrow their potency from conditions that are not magical. Through science we have secured a degree of power of prediction and of control; through tools, machinery and an accompanying technique we have made the world more conformable to our needs, a more secure abode. We have heaped up riches and means of comfort between ourselves and the risks of the world. We have professionalized amusement as an agency of escape and forgetfulness. But when all is said and done, the fundamentally hazardous character of the world is not seriously modied, much less eliminated. (LW 1:45)

The “quest for certainty” characteristic of so much of philosophy has an honest enough beginning; science and technology represent crowning achievements of prediction and control, an unprecedented ability to reduce the hazards of fortune and secure our fate. Despite these great achievements, the world continues to be a risky place. What philosophy is often tempted to add to this is the honorific “real” or “true” to those achievements of control and stability, and the insult of “mere appearance” to the dangerous and the sporadic elements of our experience. This addition is worse than pointless, since it distracts us from real dangers which we should attend to most clearly, and it removes from existence the very phenomena which make it possible to understand science as an achievement.

Instead, we should attend to the strategies and abilities developed in science and in common sense for recognizing danger and coping with it. The forms, processes, and techniques of controlled inquiry represent the crucial achievements that philosophy of science can help uncover and make available outside of the specialized practices of science.

## 7.2 Transforming Experience

Prior to the formation of habit and concepts, William James famously remarked, experience would be “one great blooming, buzzing confusion” (James, 1890, I:488). Our experience is of a structured, differentiated world “because we have had

a long education, and each thing we now see distinct has already been differentiated from its neighbors by repeated appearances in successive order” (ibid., I:495-6). While we might want to distance ourselves today from the details of Jamesian psychology, especially claims about newborn phenomenology, there is a basic truth in this idea. The accretion of experience, the development of habit, and especially the progress of inquiry make a deep impact on the structure of our experience. In its course, science imparts new meanings to the events of our experience, allows us to identify new and different kinds of objects, and to engage in more productive activities. Thanks in part to the work of science, scientists and non-scientists alike experience and manipulate atoms, electrons, genes, cells, ecosystems, etc. as part of their daily lives.

Hanson, Kuhn, and Feyerabend uncovered the difficulty in holding to a distinction between the observation or visual data, on the one hand, and the interpretation, on the other. As we have seen (§2.8.1), “facts” as they function in scientific inquiry are not some absolute given, but they are *taken* from a situation by operations of observation and selection. Outside the bounds of inquiry, we do not experience appearances, sense-data, or impressions; we experience chairs, tables, persons, and conversations. So too, we experience new things as a result of scientific inquiry. Paul Churchland (1979, 30-34) recounts an experiment that any reader can perform: it may seem inescapable, despite all our knowledge to the contrary, to perceive ourselves as standing on solid ground, while all the heavens move around *us*. Whatever its scientific status, the *geocentric* theory seems to accord perfectly with our naive experience. Churchland describes the conditions under which we might actually undergo a Gestalt shift, coming to see the plane of the ecliptic, our position therein, and the sun at the center of it all, and thus be “at home in [our] solar system for the first time” (Churchland, 1979, 34).

What inquiry and the growth of knowledge do in part is to invest the events we experience with *meaning*. A completely untrained eye might look into the mi-

croscope and see nothing but squiggles and blotches. These brute experiences might be aesthetically interesting or unusual, but nothing follows from them. The trained observer sees not squiggles but *objects*, cells or organisms. They can identify characters of the objects that put them in kinds, and thus can infer the possible origins, behaviors, and reactions they will have in future circumstances. Such an observer can do more than attend to their present behavior; they can think about them later, contemplate their structure or behavior without requiring their immediate presence. As Dewey says,

By this fashion, qualitative immediacies cease to be dumbly rapturous, a possession that is obsessive and an incorporation that involves submergence: conditions found in sensations and passions. They become capable of survey, contemplation, and ideal or logical elaboration; when something can be said of qualities they are purveyors of instruction. Learning and teaching come into being, and there is no event which may not yield information. . . . Even the dumb pang of an ache achieves a significant existence when it can be designated and descanted upon; it ceases to be merely oppressive and becomes important; it gains importance, because it becomes representative; it has the dignity of an office. (*Experience and Nature*, LW 1:132-3)

Events gain meaning, and thus they can be *represented* as well as *representatives* of other events.

## 7.3 Changing the World

It is not enough to say that inquiry imparts new *meanings* to events and thus transforms our experience. As Dewey insists,

The pre-cognitive unsettled situation can be settled *only* by modifications of its constituents. (LW 12:121, my emphasis)

As Peter Godfrey-Smith points out, this is a distinctive feature of *Dewey's* version of pragmatism:



Dewey's epistemology is primarily a theory of problem-solving. It is common within pragmatist philosophies to regard cognition as a response to problems encountered in experience. It is *distinctive* of Dewey in particular to understand these "problems" in terms of specific properties of environments: variability as a property of nature is the source of problems to which cognition is a response. (Godfrey-Smith, 1996, 107, emphasis added)

It is not enough that we come to have new *beliefs* about the world, or that we experience it differently. Problem-solving in general doesn't just result in but *requires* a real change in our situation. There is nothing spooky about this change; it is no idealist backsliding. Consider some advance in the field of medicine. We face a problematic situation, people dying in an epidemic of some new form of disease, and we do not know what to do. A difficult process of inquiry results in the discovery of a cure. Establishing *that* it is a cure *requires* modification of existing conditions; we must experimentally apply the treatment, thereby altering the course of many cases. Once we have concluded our inquiry, the alteration of the world continues, as the treatment is widely applied. Thanks to the development of the basic method of *experimental* science, such transformation of the natural world is involved in *every case of inquiry in which experimental methods are applied*, and given the necessity of experiment *in some form or other* for a warranted conclusion of inquiry, such transformation is pervasive.

Even those transformations which I've referred to as "transforming experience" or "imparting meaning" are in one sense changes to the things themselves. Dewey's understanding of experience and meaning are active, operational and interactional. As Godfrey-Smith describes,

Suppose a new theory of some domain makes possible new forms of action. The things described in the theory acquire new potentialities as a consequence, even before action has taken place. They acquire new possibilities for interaction with us and with other things. Dewey does regard this as a change made by the new theory to its subject matter. (Godfrey-Smith, 1996, 159-160)

Godfrey-Smith, on the other hand, holds out “for a change made to intrinsic properties...before thought has changed the external world”(ibid.). I am with Dewey in thinking that a concern for “intrinsic properties” is misplaced, part of an invidious distinction drawn in the context of the quest for certainty. Even if we have a well-motivated and warranted account of intrinsic properties, though, the view that changes to intrinsic properties are somehow more “real” than alterations of potentialities for change or possibilities for interaction is arbitrary. It is impossible in practice to account for the myriad interactions of any object on the basis of intrinsic properties alone; to understand anything in science is to understand the relations it enters into. Interactions with human society and processes of inquiry are no different, and perhaps more significant than many other interactions, given the major impact such associations have on the subsequent career of many objects.

So transforming experience is also a change to the world. Likewise, changing the world necessarily transforms our experience. There is no justification for a deep division between the world we live in and our experience of it. The flaws in the Cartesian notion of a mental theatre of experience are so familiar, the critique so widespread that we need not rehearse them here. Our active, participatory nature puts paid to any notion of such a division. Or experience is *in* and *of* the *world*, not in our *heads* and of our *experiences*. The significant changes made by inquiry in the world are changes to the constituents of experience, and *they must be registered as such*, or they can have no role in moving inquiry towards its close.

## 7.4 Objects, Events, and Meaning

This takes us deep into one of the most difficult corners of Dewey’s thought, one commonly misinterpreted, as John Shook points out:

Throughout Dewey’s entire career he held that objects of knowledge are created by the process of knowing. This primary epistemological thesis

should not be confused with any metaphysical stand, despite the obvious temptations. For instance, if one assumes (as many realists do, but Dewey did not) that knowing is solely a subjectively mental affair, then one could infer that Dewey thought that objects of knowledge were also mental. Alternatively, if one assumes (as many realists and idealists do, but again Dewey did not) that reality consists of nothing but objects of knowledge, then one could infer that Dewey believed that reality is created by human knowing. Going even further, if one assumes contra Dewey that knowledge is the only relationship we can have with reality (as some relativists and idealists hold), then his epistemology appears to imply, as no two people come to knowledge in identical ways, that we each live in a different known world. (Shook, 2000, 7)

But since Dewey did not make these assumptions, none of these consequences follow. It is important to keep in mind Dewey's actual views about objects. Two points are key. First, a point he makes in *Experience and Nature*: "objects are events *with* meanings..." (LW 1:240). And indeed, it is always *objects* in this sense that we experience, not bare events (experienced temporal processes are objects, too, in this sense); the content of experience is always laden with meaning, even if those meanings are obscure and uncertain. Likewise, it is important to keep in mind Dewey's understanding of "meaning," which has to do with what events, qualities, and objects *signify* for future outcomes and actions.

Another key point about objects is made in the *Logic*:

The name *objects* will be reserved for the subject-matter so far as it has been produced and ordered in settled form by means of inquiry; proleptically, objects are the *objectives* of inquiry. . . things exist *as* objects for us only as they have been previously determined as outcomes of inquiries. (LW 12:122)

Inquiry aims to settle and arrange its subject-matter into objects; together with the prior point, this means inquiry aims to establish the meanings of things. And again in *Experience and Nature*:

Object is...that which objects, that to which frustration is due. But it is also the *objective*; the final and eventual consummation, an integrated secure independent state of affairs. The subject is that which suffers, is subjected and which endures resistance and frustration; it is also that which attempts subjection of hostile conditions; that which takes the immediate initiative in remaking the situation as it stands. (LW 1:184)

Objects exert two pressures on us: first, the stock and store of objects which we encounter in experience help form the conditions and resources which we must work with, including those conditions of tension that lead to problematic situations. On the other hand, inquiry aims at *new* objects which will be more integrated and secure.

So, combining Dewey's understanding of objects with the points made previously about how inquiry transforms experience through establishing new meanings, and changes the world by modifying situations, we can see the sense in which the process of knowing (inquiry) creates the objects of knowledge. Take the following passage, for example:

An object...is a set of qualities treated as *potentialities* for specified existential consequences. Powder is what will explode under certain conditions; water as a substantial object is that group of connected qualities which will quench thirst, and so on...With the progress of technology, clay and iron have acquired new potentialities...When it was discovered that wood-pulp could be used for making paper...the *significance* of certain forms of lumber as objects changed. They did not become *entirely* new substantial objects because old potentialities for consequences remained. But neither was it the same old substance. The habit of supposing that it is the same all the time is the result of hypostatizing the logical character of being a sign of having significance into something inherent. Being a substantial object defines a specific function. (LW 12:132)

The key here is to understand that "object" in Dewey's vocabulary is not a classical substance, but rather a *logical* category—it refers to a certain subject-matter or content having a certain *significance* and being a *sign* of certain further possibilities

and consequences. After certain discoveries in the areas of metallurgy and chemistry, new properties accrue to iron. Even as it sits underground as iron ore, it has a new potential, to be smelted, to be made into steel. It has a new meaning, and when we come across iron ore, it signifies new possibilities. The physical career of much of the iron ore close to the surface of the earth is irrevocably altered by the course of inquiry, as its new meaning makes it much more valuable to extract, process, and use as material for various artifacts. Dewey's claims about objects are not mysterious metaphysical excesses; rather, they are rather unmysterious but important semantic and causal claims.<sup>1</sup>

## 7.5 Existence, Value, and Criticism

It is important to mention explicitly two further features of the picture I've been developing. First, inquiry and experience are both deeply and inherently *social* affairs. The meanings that accrue in the process of inquiry are not private mental items; they are social by nature. The subject-matters which inquiry investigates and the data which inquiry produces are publicly available. The theories, ideas, and concepts taken up, developed, and invented by inquiry are symbolic cultural artifacts. Even in the legendary historical epoch in which the lone scientist supposedly labored in the laboratory in solitude, running experiments and gathering knowledge, that knowledge had to be produced in a form that was socially available. This knowledge was quickly disseminated, and the ultimate test of its status *as* knowledge depended in part on its applicability by others.

A later and related aspect of the image of science I've been constructing is what Dewey called "the continuum of inquiry." While it is useful for many purposes

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<sup>1</sup>We should not make too much of the distinction between semantic and causal claims. Both are ways that our knowledge alters objects, and perhaps the two are not as distinguishable in most concrete cases as they are in the simplified examples given above.

to focus on the level of individual inquiries,<sup>2</sup> it is important to keep in mind that no inquiry happens in isolation. Any inquiry draws on the results of past inquiries<sup>3</sup> and has implications for future inquiries. It is in how it bears out in future inquiries that the results of the present inquiry are finally judged (hence the connection with the social); warranted assertions ought to have staying power beyond the immediate situation, insofar as they are applicable.<sup>4</sup>

It is not just *my* world that is transformed by the processes of inquiry that I engage in. It is *our* shared world. If we must evaluate and criticize technology because of the ways it is used, so much the more we must evaluate and criticize scientific inquiry because of the way that it shapes and transforms our world. It is no accident that the final chapter of *Experience and Nature*, Dewey's metaphysical treatise, is entitled "Existence, Value, and Criticism." There Dewey sets out his conception of philosophy as a form of *criticism*,

having its distinctive position among various modes of criticism in its generality; a criticism of criticisms, as it were. Criticism is discriminating judgment, careful appraisal, and judgment is appropriately termed criticism wherever the subject-matter of discrimination concerns goods or values. (LW 1:298)

Dewey's metaphysics is meant to be "a ground-map of the province of criticism," to indicate the "generic traits of nature" that make values and their precariousness possible (LW 1:308–9). Dewey's logic, philosophy of science, and theory of inquiry form another core part of this project, as helping to make clear the methods and strategies necessary to the securing and developing of goods and values. Despite the occasional use of the term "instrumentalism" to refer to these ideas, Dewey makes

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<sup>2</sup>Not least as a corrective for a common tendency to focus at a scale much larger (e.g., paradigm shifts, research programmes) and thus miss the trees for the forest, or at a scale much smaller (individual techniques of data gathering) and thus miss the tree for the leaves.

<sup>3</sup>Though it rarely takes them as-is without any changes.

<sup>4</sup>The obvious analogy to legal precedent is quite helpful here. A judgment that is subsequently reversed no longer has the force of law; one that is very quickly reversed can only barely be said to have ever had it.

clear that means and ends lie on a continuum. Logic cannot merely deal with the efficient means to realizing values, but must confront values themselves.

Dewey's philosophy of science places a heavy burden on the scientist and on the philosopher of science. Scientists and philosophers of science need to be more thorough in bringing the rich results of science back into daily life in a way that can enrich it. Too often have philosophers used the results of science to diminish daily life and make it *less* worthwhile or comprehensible.<sup>5</sup> The dissemination of the results and methods of science should increase our powers of prediction and control, impart new meaning to our experiences, deepen our appreciation of values and qualities, and improve our ability to resolve problems. This is a much more worthy task for "naturalists" than denying the existence of values and meaning, denying the importance of experience and knowledge, or belittling the ignorance of common people.

Further, we must be careful to ensure that the way that science shapes our world serves our interests (rather than, say, the interests of those who can better pad scientists pockets). Scientists bear a deep responsibility for the future. Philosophers of science must be *critics* of science, not in the sense of being science's detractors, but in analogy to the relationship of the art critic to art, or the literary critic to literature. Critics are not *against* art or literature, but they do apply judgment and careful appraisal to particular works. Likewise, philosophers of science should not merely describe episodes in science, nor defend it as a whole; they should also apply discriminating judgment and careful appraisal to science, especially where values enter in and goods are at stake. It is my hope to have made a small contribution towards such a new direction in philosophy of science.

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<sup>5</sup>Some striking examples: skepticism, moral non-cognitivism and subjectivism, stark reductionism and eliminativism, denials of the reality of values and qualities of all sorts, epiphenomenalism, verificationism, fatalism. The common tendency could almost be interpreted as a vicious streak amongst a certain sub-population of philosophers.

# Appendix A

## Epilogue: Science as Distributed Cognition

I want to make plausible the following claim: *Analyzing scientific inquiry as a species of socially distributed cognition has a variety of advantages for science studies, among them the prospects of bringing together philosophy and sociology of science.* This is not a particularly novel claim; indeed, Paul Thagard (1993; 1994; 2004) has been suggesting something like this for well over a decade, while philosophers like Ronald Giere and Barton Moffat have been stumping for the distributed cognition approach in more recent years, and Nancy Nersessian's research group at Georgia Tech (Nersessian et al., 2003b) has been fruitfully applying this approach to the study of research laboratories and other scientific institutions.

I will retrace some of the major steps that have been made in the pursuit of a distributed cognition approach to science studies, paying special attention to the promise that such an approach holds out for bridging the rift between philosophy and the social studies of science. Such an approach is not without its pitfalls, and I will consider several problems, both for distributed cognition as a theory and for its applications to science. I will argue that there is a path out of the woods, and try



to point the way. Ultimately, I argue that we will have to widen the scope of the distributed-cognition approach.

## A.1 What is D-Cog?

Distributed cognition (d-cog) is a radical theory in cognitive science, created primarily by researchers at the University of California, San Diego, which maintains that one can fruitfully analyze activities taking place between one or more people along with technological artifacts as *cognitive* in the same way that traditional cognitive science has analyzed certain intrapersonal processes. The beginnings of the approach can be seen in the Parallel Distributed Processing (PDP) research group's work on connectionist (neural-network) models of cognition. When it came to an explanation of how a neural-network architecture can do science, mathematics, and logic, they made an intriguing suggestion:

If the human information-processing system carries out its computations by “settling” into a solution rather than applying logical operations, why are humans so intelligent? How can we do science, mathematics, logic, etc.? How can we do logic if our basic operations are not logical at all? We suspect the answer comes from our ability to create artifacts—that is, our ability to create physical representations that we can manipulate in simple ways to get answers to very difficult and abstract problems. (Rumelhart et al., 1987, p. 44)

This is quite the break from classical cognitive science research in two ways. First, cognitive science has traditionally treated “cognition” as a matter of computational operations on symbolic states, not unlike the operations of logic. The move towards a connectionist architecture, where the basic processes are more like pattern-recognition than applying logical rules, is the radical step that the PDP group was most keen to argue for. Second, the cognitive sciences ordinarily focus on what goes on with an individual person, and “cognition” is what goes on in their head. It is this second break that spurs d-cog.

D-cog takes a wider perspective than classical cognitive science. It is concerned with use of “cognitive artifacts” such as pen and paper, longhand mathematical calculation, and digital computers. It is also interested in the cognitive role of social interactions, cultural institutions, and forms of social organization. D-cog attempts to move the boundaries of “cognitive activity” out from the head and into the world of social and technological interactions. The foundational text for research in distributed cognition comes from *Cognition in the Wild* (Hutchins, 1995a), the work of another UCSD scientist, Edwin Hutchins, who was trained in cognitive anthropology.

There are two ways we might understand the project of distributed cognition research. A more cautious definition—the one preferred by Giere, for example—would be that some socially and technologically distributed activities can profitably be understood as “cognitive,” while allowing that many elements of cognition—including agency—remain in the realm of the individual. The more radical definition of d-cog that one could adopt, and the one most supported by Hutchins’ own statements, is more sweeping; as Hutchins says, “I hope to show that human cognition... is *in a very fundamental sense* a cultural and social process” (Hutchins, 1995a, xiv, emphasis mine).

Two examples have become pervasive in papers about d-cog.<sup>1</sup> Nevertheless, it is worth briefly discussing each. The first originates in the *PDP* chapter quoted above. When we multiply large numbers, we rarely if ever do it in our heads. With a pencil and paper, multiplying even very large numbers is transformed into a simple task, requiring no more than the ability to do one-digit multiplication and addition. A nearly impossible task for an individual human cognitive system becomes perfectly easy when distributed across the human-pencil-paper system. (See Figure 1.)

A second key example comes from Hutchins’ work on ship navigation in the US Navy (1995a). Navigation on a large naval vessel is not the job of a single

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<sup>1</sup>See Hutchins (1995a); Giere and Moffatt (2003); Magnus (2007).

$$\begin{array}{r}
 12044 \\
 \times \quad 432 \\
 \hline
 24088 \\
 36132 \\
 48176 \\
 \hline
 5203008
 \end{array}$$

Figure A.1: Longhand Multiplication

individual (as it is for Hutchins' contrast case of the Micronesian canoer), but rather the work of a team of people performing various jobs using various instruments. Here is a somewhat simplified account: A crewman (called a pelorus operator) is given a landmark to identify by a plotter in the pilothouse. The pelorus operator then uses a piece of equipment called an "alidade" to determine the bearing of the landmark. Generally, there is more than one pelorus operator, and they all relay their information to a plotter. The plotter uses that information to determine the location of the ship and its bearing. The plotter relies on a specially structured map, compasses and protractors, etc. in order to use the information about the bearing of landmarks to compute the ship's location and bearing.

In this example, it is physically impossible for a single human being (given the size of the ship, the location of various vantage points, and the time in which the task must be completed) to do the cognitive work of figuring out the location and bearing of the vessel. Of course, on a *different* kind of ship, it is possible for a single person, and indeed, in the case of the Micronesian navigator, it is possible for a lone individual to do so without instrumentation. Nevertheless, the navigation team on a large naval vessel completes the cognitive task as a team using artifacts. The essence of a d-cog analysis is in treating this network of individuals and artifacts as a single

cognitive system.

## A.2 Science as D-Cog

Can science be analyzed using d-cog? Consider a case discussed by Giere and Moffatt (2003), originally due to Bruno Latour (1986). Chemical formulae were originally introduced and developed in the nineteenth century. A formula like the one in Figure 2 allows one to do theoretical chemistry by manipulating such symbols on paper, replacing the need to directly manipulate chemicals. All one needs to do in order to determine what is going on in a reaction, knowing something about the products and the reactants, is assume conservation and balance the equation. Just as doing long multiplication by hand transforms a complex calculation into a set of simple pattern-matching problems, so the use of chemical formulae as a cognitive artifact transforms the complex theoretical analysis into a simple exercise in pattern matching.

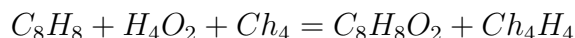


Figure A.2: Chemical Formulae

Consider another example, the Hubble Space Telescope, an important piece of scientific equipment in contemporary research in astronomy, astrophysics, and cosmology (Giere, 2006, 99–100). The telescope is a large and complex instrument that must be operated remotely. It is used by an organized group of people, and that use is mediated by further instruments and computer equipment on earth. To draw out the sense in which d-cog analysis is appropriate, think of the telescope as the eyes of a large cognitive system that also includes the group of scientists and the earth-bound computer equipment. Just as cognitive science can study ordinary perception, distributed cognitive science can look at this distributed system of “perception.”

A final example, due to Nancy Nersessian and her collaborators at Georgia Tech (Nersessian et al., 2003a) comes from the cognitive-ethnographic study of research in a biomedical engineering laboratory. Nersessian discusses how certain lab equipment are used to *model* actual biological processes. For example, the lab she studies uses a piece of equipment called a “bioreactor,” which, among other things, models blood flow in a way which can be used to study arterial cells and biomedical devices. Nersessian’s work explicitly treats the bioreactor, along with the skills one needs in order to use it in certain ways as a “mental model” for the distributed system of the biomedical laboratory. Doing so reveals interesting facts about the system that aren’t available if you treat it just like a device or an instrument.<sup>2</sup>

### A.3 The Cognitive and the Social

In 1986 Latour and Woolgar issued their famous “ten-year moratorium on cognitive explanations of science,” promising “that if anything remains to be explained at the end of this period, we too will turn to the mind!” (Latour and Woolgar, 1986, 280), quoted by Giere and Moffatt (2003, 301)). Of course, the moratorium has run out, much remains to be explained, and they never turned solely to the mind to provide the missing explanations—but that’s not the point. What is interesting are the motivations and implicit assumptions behind this rhetorical flourish.

Part of the reason they issued such a moratorium, as Giere and Moffatt (2003, 301), Nersessian (2005, 18), and others have argued, is that they held to a rigid dichotomy of cognitive and social factors. Because their primary goal was to get a serious sociology of science going, they regarded such a moratorium as necessary. In order to make room for social explanations of science, we must, they thought, bracket all cognitive issues and explanations.

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<sup>2</sup>I will return to this case in further detail below, to indicate some of the major gains of such an analysis.

D-cog provides an alternative to this way of thinking. It shows us how to treat the cognitive and the social as the same thing for certain purposes. Because cognitive structures need not exist only in the mind (and perhaps never do so, if the radical version of d-cog is correct), but instead can exist in the complex interactions of social groups and technological artifacts, one can study social groups cognitively, or cognitive systems sociologically. There need be no unbridgeable divide between social and cognitive explanations.<sup>3</sup>

What's most interesting about the possibility of seeing the social in terms of the cognitive and vice versa is that it might just help heal the wounds of the Science Wars and bring the various parts of science studies which are often at loggerheads—especially philosophy and sociology of science—together towards a more common purpose. Because of the perceived incompatibility of the cognitive and the social, the terms of analysis of much recent sociology of science—negotiation, authority, power, mobilizing resources—seem to have a cynical cast, dismissive of the virtues of science. By contrast, the normative concerns of philosophers of science—justification, realism, objectivity—seem divorced from the obvious social reality of science.

There are plenty of philosophers nowadays—such as Helen Longino and Philip Kitcher—trying to reconcile the cognitive and the social, the normative issues of philosophy of science with descriptive sociological analyses. Their arguments are mainly about the very possibility of such a reconciliation, and focus more on the reformulation of traditional philosophical issues (e.g., objectivity) in ways that involve social relations and institutions rather than focusing on the properties of individual scientists or the *abstract structure* of science. D-cog presents more than the mere possibility of a post-hoc reconciliation. It allows one to re-interpret the excellent and extensive body of sociological and historical studies in line with cognitive and epistemic concerns.

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<sup>3</sup> If I read him correctly, Bruno Latour has come around to this more sophisticated view of the cognitive-social relation. See Latour (1993, 1996).

Consider again the case of chemical formulae. Giere and Moffatt (304–5) take this example from work by Bruno Latour (1986), who has emphasized the importance of such innovations in the history of science. According to Giere and Moffatt, Latour thinks that something like chemical formulae are important because they concentrate information in a way that

confers authority and power on those who control it. And it leads others to align themselves with such powers, thus increasing still further their authority and power. In a struggle for dominance, whether in science, politics, or war, those with the most and strongest allies win. (Giere and Moffatt, 2003, 305))<sup>4</sup>

What d-cog allows Giere and Moffatt to do is to look at the specifics of Latour's analysis of the social-technological aspects of science and point out the *cognitive* function of various parts of the process. What might be cast by a sociologist in terms of exerting power and gaining allies can be cast in terms of improving cognitive capacities of a distributed system over a naked cognitive agent. As Giere and Moffatt say about this particular case,

The invention of new forms of external representation and of new instruments for producing various kinds of representations has played, and continues to play, a large role in the development of the sciences. From a cognitive science perspective, both sorts of invention amount to the creation of new systems of distributed cognitive system. So, for us, the notion of distributed cognition brings under one category such things as Cartesian coordinates and the telescope, both of which are widely cited as major contributions to the Scientific Revolution. (305)

Even more promising than the idea of reinterpreting sociological and historical work in cognitive-epistemic terms is empirical work being done by cognitive scientists,

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<sup>4</sup>Having read Latour, this seems a slight exaggeration of his point. Already he seems to recognize the d-cog perspective when he says, "An average mind or an average man, with the same perceptual abilities, within normal social conditions, will generate totally different output depending on whether his or her average skills apply to the confusing world or to inscriptions" (Latour 1986: 22). I take this to imply that inscriptions function as a cognitive artifact that change the functioning of the mind independent of the particular agent's perceptual abilities.

philosophers of science, and researchers in science studies *using* the methods and theoretical frameworks of d-cog to analyze science. To return to another example from above, Nersessian and her collaborators have been studying work in a biomedical engineering laboratory, applying cognitive, historical, and ethnographic methods and understanding the organization of the lab and the function of artifacts within the lab as parts of a distributive cognitive system. Such an analysis allows researchers to understand how a bioreactor is both a “significant cultural artifact. . . [and] a locus for social interaction” (Nersessian, 2005, 50) with a history of different kinds of roles in the culture of the laboratory, and also a model that plays a role in distinctive types of representation and reasoning. Without such an analysis, the fact that a single object plays both of these roles (and the pervasiveness of such objects in science) is a colossal coincidence and a total mystery. One might even be led to deny that the object has an important cognitive side (as sociologists of science are often led to do) or to claim that its cultural history and social roles are inessential to its role in representation and reasoning (as philosophers have often done). D-cog analysis makes better sense of what is going on in such cases, and makes better sense of how the social and the cognitive are integrating in science as a whole.

## A.4 Challenges

Applying Hutchins’ d-cog theory to the study of science is not without its problems. I will focus on two major challenges to the applicability of the theory.

The first problem is that d-cog looks like a theory applicable to fairly static systems. The paradigm applications of d-cog in Hutchin’s work (Hutchins, 1995a,b)—airplane cockpits determining their speed, crews of Navy ships navigating through a harbor, even pencil-and-paper multiplication—respond to dynamic situations where “problem-solving situations change in time” (Nersessian, 2005, 36), but the organization and nature of the technological artifacts in play are treated as static. The



analysis is thus “dynamic but largely *synchronic*” (ibid.) But really, these systems are evolving, if slowly, both from external pressures (invention of new technology, new safety protocols, changes in policy) and internal developments (shifts in Navy culture, new pilots gaining skills, invention of new techniques). Further, many other kinds of cognitive activities are much more *diachronically* dynamic, involving creativity, innovation, and rapid changes in technology and social structure.<sup>5</sup> This is especially true of systems like scientific laboratories, where innovation, new discovery, and creative problem-solving are essential parts of the activity. Another aspect of this problem is that d-cog analyses tend to treat relatively well-bounded systems, with low-bandwidth information flow from outside the system and high-bandwidth information flow within the system. In order to straightforwardly apply Hutchins’ d-cog framework, the nature of the task at hand and the system that carries it out must be rather well-bounded. On the other hand, many activities, including scientific activities, have quite vague and porous boundaries. What counts as part of the system might change rapidly as the activity goes on.

The second problem comes from a direct critique of Giere’s appeals for treating science as d-cog by P.D. Magnus (2007). Magnus’s critique turns on a particular move in Hutchins’ (1995a) account of d-cog, where he relies on Marr’s tripartite distinction between *computation*, *algorithm*, and *implementation* (50–52), which Magnus simplifies into the distinction between *task* and *process* (following Ron McClamrock), where *task* is an abstract description of the computational goal or behavior that the cognitive system is to satisfy, and the *process* is just a specification of how the task is to be accomplished. This furnishes Magnus with a compellingly succinct definition of d-cog:

An activity counts as d-cog only if the process is not enclosed by the epidermis of the people involved in carrying out the task. The implementation uses tools and social structures to do some of the cognitive work.

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<sup>5</sup> “Although there are loci of stability, during problem-solving processes the components of the systems undergo development and change over time” (Nersessian 2005, 36).

(Magnus, 2007, 299)

Where the task in question “would be cognitive if the process were contained entirely within the epidermis of one individual” (300).

So, an activity is d-cog only if the process is not located inside the skin of an individual carrying out a cognitive task. Is science like that? It is easy enough to see that on Magnus’s interpretation, if we are going to be able to analyze scientific activity as a species of d-cog, we must be able not only to analyze the scientific *process*, but we must also be able to specify the *task* of science. This poses two types of concern. First, at a local level, can we always abstractly specify a *task* for science? Does a biomedical engineering laboratory have a well-specified task? Does a physics journal? What about a conference on global warming? While it seems likely that there are some scientific activities which might be amenable to such an analysis, it seems dubious that one could specify the kind of *computational* task necessary for d-cog analysis for all or even most scientific activities.

The second worry that Magnus raises is whether one can specify a *global* task for science, and thus do a d-cog analysis of science “writ large.” That is, “Can we understand science altogether as one giant, distributed cognitive enterprise?” Such an interpretation is already suggested by Hutchins (1991, 288). It would be a lucky thing if we can do so, for we could then give a clear explanation of the common view in science studies that it is the large-scale institution of science, rather than individual scientists, which produce or are responsible for scientific knowledge. To this end, Magnus analyzes three candidates for giving a *task* analysis for science-as-a-totality: Merton’s *ethos* of science, Philip Kitcher’s ideal of the distribution of cognitive labor, and his more recent image of well-ordered science.

As you might imagine, the prognosis is dire; Magnus is rightfully pessimistic about the possibility of specifying the task of science-as-such. After all, the range of activities of science, the differences in approaches in different research traditions, the variety of uses to which science is put, and so on make it highly unlikely that

there is one simple task that all of science aims at. One need only compare high-energy physics to molecular biology to pharmaceutical trials to see how unlikely such a project is.<sup>6</sup> Given the poor prospects of rescuing d-cog analyses of science in this way, I will suggest we look elsewhere.

## A.5 Prospects for a D-Cog Theory of Science

Here's where we stand: d-cog holds great promise for analyzing science in a way that makes the relation of the social-technological nature of science to its cognitive-epistemic virtues most perspicuous, and thus joining together what the Science Wars hath put asunder, of healing the rift between philosophy and sociology of science. However, using d-cog to analyze science faces some severe difficulties: it treats systems whose basic structures and resources are fairly static, while science is not only synchronically but diachronically dynamic. Scientific systems evolve, and d-cog provides little in the way of resources for analyzing that evolution. D-cog applies to well-bounded systems, whereas the boundaries of science aren't so clear. D-cog analysis requires the specification of a computational task that can be implemented by a distributed process, while it seems doubtful that a global task can be specified for science, and even unlikely that a more local task can be specified for many important cases. Does this spell doom for the d-cog approach to science studies? Is there any way forward?

I think there is, and that way depends most importantly on going well beyond Hutchins' work from the mid-1990's. Of course, Hutchins himself is an active researcher and has gone beyond that work himself, into areas like conceptual change, learning, and the embodiment of cognition. Likewise, Nersessian, for example, relies on d-cog, but has taken it beyond Hutchins' original formulations. There are also traditions and research programs related to d-cog, such as neo-Vygotskian psychol-

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<sup>6</sup>See Cetina (1999). Cf. Giere (2002).

ogy, cultural-historical activity theory, and situated action theory, that have things to offer a *broadly* d-cog account of science. On the basis of the criticisms so far discussed, I will conclude by indicating the ways we must modify our understanding of d-cog in order for it to have positive prospects as an account of science.

The first important point to make, as against Magnus's interpretation and some of Hutchins' formulations of d-cog, is that cognition is *not* computation. Certainly, computation is one kind of thing that cognitive agents and cognitive systems do, but it isn't the case that cognition is identical to computation. Cognition is not a single algorithm or program, though it may use algorithms. Human cognitive capacities at their best are flexible and responsive to particular situations, creative and dynamic. Cognition is a multi-purpose capacity in humans, and likewise in any other sort of cognitive system.<sup>7</sup> While this may be a controversial points in some circles in cognitive science, those circles are shrinking precipitously. It is hard today not to agree with the point that was radical when proposed by the PDP group decades ago, that human cognition bears little if any resemblance to classical computation.<sup>8</sup>

While certainly a controversial approach in many circles, there may be some valuable lessons to be learned from cultural-historical activity theory (CHAT) for providing a d-cog analysis of science.<sup>9</sup> CHAT provides a tripartite distinction between *operations*, *actions*, and *activities* that adds a useful layer to the talk about task vs. process. Operations are the basic components of actions; they are generally routinized human behaviors or mechanical operations, carried out under certain conditions, instrumental to engaging in some action. Actions are conscious, goal-directed processes, undertaken by individuals or small groups. For example, Leont'ev (1978, p. 66) describes learning to drive a car with a manual trasmission. At first, all the

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<sup>7</sup>Even those who regard cognition as having a modular architecture must admit that the human cognitive system at large is a complicated, multi-purpose, dynamic, and flexible system.

<sup>8</sup>Even if one does not accept PDP-type models.

<sup>9</sup> See Leont'ev (1978); Engeström (1987); Cole and Engeström (1993); Cole (1988); Engeström et al. (1999).

processes of driving the car—breaking, using the clutch, shifting gears—require conscious attention. They are the focal, goal-directed activities. For the accomplished driver, these processes become unconscious, subordinated to actions like speeding up, driving up a steep incline, driving to work. In the end, the unconscious operations are actually off-loaded to a machine, the automatic transmission.

Beyond the level of action is the activity. Actions are goal-directed, relatively short-lived and well-bounded in time and space. Activities exist in and evolve over longer periods of time; they have a history. They are associated with a culture or a community, and they are often embedded in institutions or forms of social organization. While actions are simply goal-directed, activities are aimed at a more general, less-bounded, and changeable object or motive. While the particular actions of a welder in a factory have a quite well-defined goal (joining two metal pieces together), the activity of the whole factory has a more nebulous object of gaining profit, and the way that motive is conceived over time may change (for example, a change to a more socially-conscious, “green” corporate mission may alter both the ways that profit is got and the way that gaining profit is understood). The object of the activity system need not be at all available to the individual members of the system; indeed, the worker need not have any ideas about the economic purposes of the factory—he need only be in it to get a paycheck for himself.

This set of distinctions may prove fruitful for thinking about science as d-cog. In particular, the *task* as Magnus seems to understand it seems identical to the *goal* to which actions are directed. The task-process distinction may thus make perfect sense at the level of action, but to get the whole sense in which science is a d-cog *activity*, we may need to think of it at the level of activity directed at an object which is partially constituted by the evolution of the activity itself.

Another potentially necessary turn is to supplement Hutchins’ cognitive ethnography with Nersessian’s cognitive-historical method. Nersessian is keenly aware of the problem of evolving systems for Hutchins’ (1995a; 1995b) account, especially

as applied to science. Indeed, the argument that Hutchins' account does not naturally accommodate the evolution of cognitive systems I gave above is her argument. In their own d-cog research on biomedical labs, Nersessian and her collaborators combine ethnographic investigation of the particular system with *cognitive-historical* analysis, which looks at different scales of history to understand the evolution of problems, concepts, cognitive artifacts, etc.

So, *is* science a distributed cognitive system? This has been challenged on the basis of it being an evolving, messy, less-bounded system. Magnus has challenged it on the basis of whether there is a particular task that science carries out. But *what is a cognitive system* anyhow, even in the traditional sense of "cognitive system?" This shouldn't stand or fall on the details of a certain framework of cognitive analysis. After all, presumably, *I* am some kind of cognitive system, even though I am not built to carry out one specific and well-bounded task, even though my cognitive activities evolve, and aren't always as well-bounded as certain cognitive theories might presuppose. Certainly, the limitations of a particular approach to d-cog shouldn't disqualify the more general notion. Rather, this points the way towards the need for better, more complex models of distributed cognition that might do a better job of applying to science. I have gestured towards some possibilities that seem particularly fruitful in the face of these difficulties. There is much more work to be done, and the possibilities are inspiring.

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# Index

- antifoundationalism, 22, 52
- breakdown, 132
- Cartwright, Nancy, 10, 52, 97, 105–107, 110, 122
- Chang, Hasok, 91, 94, 97
- Churchland, Patricia, 166
- Churchland, Paul, 30, 143, 166, 200
- conceptual, *see* ideas, hypothesis
- context, 22
- data, *see* facts
- Doppelt, Gerald, xii, 16
- doubt, 133
- doubt-belief schema, 43
- Earth-2, 18
- empiricism, *see* immediate empiricism
- existential, *see* facts
- experience, 199
- experiment, 60, 101
- fact, 34
- facts, 42, 49–53, 181, 200
  - as conditions, 49, 52–54, 57, 82, 184
  - constructed, 51
  - coordination with ideas, 55
  - generic, 53
- fallibilism, 22
- Feyerabend, Paul, xvii, 1, 14, 16, 55, 87, 123, 129, 140, 141, 149–152, 154, 162, 187, 188, 190–192, 194–196, 200
- Giere, Ronald, 1–4, 11, 13, 140, 209
- given, 51
- Hegel, 140
- Heidegger, 132
- Hoyningen-Huene, Paul, 16, 73, 141
- hypothesis, 34, 52, 54, 76
- idealism, 202
- ideas, 54, 76
  - as possibilities, 54, 57
  - coordination with facts, 55
- ideational, *see* ideas, hypothesis



- immediate empiricism, 31
- inquiry, 22, 44, 67, 134, 158, 179, 202
  - defined, 42
  - pattern of, 49, 50
- judgment, 62
  - of practice, 65, 67
- Kuhn, Thomas, xii, 1, 14, 16, 36, 74, 87, 125, 140, 168, 187, 188, 193, 200
  - vs. pragmatism, 74
- Lewis, David, 61
- logic
  - experimental, 7, 115
  - formal, xi, 15, 16, 36, 115
  - of science, 36
- logic, aesthetics of, 132
- logical empiricism, 1
- logicism, xvi, 13, 15, 16
- Luria, Salvador, 128
- medicine, 132, 202
- naturalism, 30
- Nietzsche, 68
- nomological machines, 52
- normativity, 47
- Oberheim, Eric, 73
- object, 62, 205
- observation, 90
- Peirce, C.S., 43, 131, 132, 140
- perplexity, *see* situation, problematic
- pragmatism, 1
- problem
  - genuine, 134, 136
- problem-solving, 22, 42, 202, 217
- problems, 43, 77, 119, 137
  - institution of, 51
  - philosophical, 6, 71
  - scientific, 44, 71
- Quine, W.V.O., 1, 11, 13, 30, 41, 61, 167, 170, 171, 184
- selective emphasis, 62
- Sellars, Wilfrid, 167, 170, 171
- Shaviro, Steven, 17
- situation, 22, 24, 25, 34, 42, 43, 45, 46, 54, 100, 101, 131, 136, 200
  - equilibrium, 189
  - indeterminate, 43, 44, 133, 137, 179, 180, 198
  - problematic, 6, 43, 44, 51, 54, 82, 114, 119, 134, 157, 158, 161, 185, 193
  - qualitative character, 43, 132

transformation of, 44, 61, 62, 67,  
84, 92, 132, 179, 202, 205

skepticism, 22

suggestion, 54, 76

testing, 52, 54

theory, 42, 54

vs. observation, 53

values, 23, 63, 101

warranted assertibility, 62, 158, 189,  
207

Whitehead, 17