Values in Science: The Distinction between the Context of Discovery and the Context of Justification

Monica G. Aufrecht

A dissertation
submitted in partial fulfillment of the
requirements for the degree of

Doctor of Philosophy

University of Washington

2010

Program Authorized to Offer Degree:
Department of Philosophy

University of Washington

Abstract

Values in Science: The Distinction between
the Context of Discovery and the Context of Justification

Monica G. Aufrecht

Chair of the Supervisory Committee:
Professor Arthur Fine
Department of Philosophy

Hans Reichenbach coined the distinction between “the context of discovery” and “the context of justification” in 1938 to distinguish the actual development of scientific theories from their rational reconstructions. My dissertation explores the role of this “context distinction” in analytic philosophy of science. I show how ambiguous uses of the distinction have masked underlying disagreements about discovery, evidence, justification, observation, and objectivity.

The context distinction initially played a major role in shaping the goals of philosophy of science. For example, it was often contended that the historian may ask what life experiences led Einstein to Relativity, but the philosopher examines only the theory itself, with the aim of determining whether it is justified. However, after Thomas Kuhn’s

Structure of Scientific Revolutions in 1962, critics challenged the context distinction. In addition, as Paul Hoyningen-Huene notes, many distinctions are in use: between the fields of history and philosophy; creativity and logic; and historical contingencies and timeless scientific facts.

In this project I argue for four claims. First, we should not search for a single best version of the context distinction. Hoyningen-Huene suggests there is a core distinction between descriptive and normative perspectives. However, I show how Reichenbach's original distinction is actually between two descriptions: the thinking processes of scientists versus their "cleaned-up" arguments for public presentation. Secondly, I argue that we should approach the many versions of the context distinctions as tools and we should evaluate them by their usefulness for any given aim. Thirdly, many versions of the context distinction are independent of each another. For instance, Kuhn has been charged with rejecting the context distinction in general, but I show how he accepts some versions of it (e.g., thought processes vs. justification, Is vs. Ought), while rejecting others (e.g., values vs. logic, history vs. philosophy). Thus, one can use some versions without being committed to others. Finally, these ambiguities often mask underlying disagreements. Clarifying these ambiguities does not resolve debates; however, it does allow stalled-out debates to continue in more fruitful directions.

TABLE OF CONTENTS

Page

List of Figures…………………………………………………………………………..... ii

1. Introduction………………………………………………………………………..… 1

2. Genesis: Hans Reichenbach…………………………………………………………..… 19

3. Initial Challenges: Kuhn and Normal Science………………………………………...... 47

4. The Structure of Philosophical Revolutions: Values, Rationality, and Theory Choice... 89

5. Recent Uses: Conflicts over Feminist Epistemology…………………………………….... 119

6. Conclusion: Suggestions for Future Use.…………………………………..………..…. 149

References………………………………………………………………………………. 163

LIST OF FIGURES

Figure Number Page

1. Reichenbach’s Three Tasks of Epistemology 45

2. Reichenbach’s Distinction and Lean DJ 46

3. Three Levels in Philosophy of Science 88

4. Versions of the Context Distinction at Different Levels 88

5. Twelve Versions of the Context Distinction 162

ACKNOWLEDGEMENTS

Philosophy is a series of conversations across space and time. When you are lucky, however, you get to engage in real time and real space with teachers, colleagues, and friends. I would like to thank Arthur Fine for our countless conversations, and for his endless patience, insights, and encouragement that made this project possible. I am grateful to every member of my committee, to Stephen Gardiner, Lynn Hankinson Nelson, Andrea Woody, and Alison Wylie, for their feedback and support, and Ben Kerr for his enthusiasm and confirmation that this is a project that does engage with and would be of interest to its subject: working scientists.

The UW philosophy department has been a wonderful place to "grow up" as a philosopher, as Andrew Light would say. Thanks go to Ken Clatterbaugh, Bev Wessel, Gina Gould, Sara Caka, and Annette Bernier for creating such a welcoming space, and to my dear friend Barb Mack who made it feel like home.

This project has grown out of conversations with countless members of this community. Feedback from many members proved invaluable, including conversations with Sara Goering, Ann Baker, Michael Rosenthal, and Carole Lee. Thank you to Karen Mazner Emmerman, my partner every step of the way, and to Kristen Intemann for showing me it could be done. Thanks as well to Christine Porter, Brandon Morgan-Olsen, Asia Ferrin, Andrea Sullivan-Clarke, Rachel Fredericks, Jeremy Fischer, Joe Ricci, Isabel Guerra Bobo, Aaron Hebble, Renee Conroy, Lars Enden, Elizabeth Scarbrough, Aditya Ganapathiraju, Marley Banker, and our dearly remembered friends Saikat Guha and Ty Mears.

Chapters of this project have benefited greatly from feedback at various conferences. I am grateful to the participants of the Integrated History and Philosophy of Science conference at the University of Notre Dame in 2009, and especially to Don Howard and Flavia Padovani for their valuable feedback, not all of which I was able to address here. I also benefited from lectures by Alan Richardson and Jutta Schickore at the History and Philosophy of Science conference at the University of British Columbia in 2008. My approach to this material is informed by their work. I would also like to thank the participants of the 2009 Society for Philosophy of Science in Practice conference, the Columbia History of Science Group, and FEMMES (Feminist Epistemologies, Methodologies, Metaphysics, and Science Studies), and especially Hasok Chang, Mathew Lund, Stephanie Solomon, and Kevin Elliott.

The final stages of the project were completed at the Max Planck Institute for the History of Science in Berlin, Germany. Thanks to Lorraine Daston, Sandra Mitchell, John Christie, Fernando Vidal, Jean-Baptiste Gouyon, Brigit Ramsingh, Francis McKay, Estelle Blaschke, Nasser Zakariva, Thomas Sturm, Ariane Sadjed, and Irene Pakuscher for our wonderful conversations on the context distinction.

I am grateful to Paul Hoyningen-Huene for the invitation to discuss this work. I continue to think through our delightful discussions, not all of which are captured in the final revisions here.

For reading drafts, helping me struggle with half-formed ideas, providing much needed inter-disciplinary and inter-generational perspectives, as well as valuable friendship and support, I thank Elisa Rassen, Leah Bricker, Julie Homchick, Sara Diaz, Kathryn Schild, Heidi Guetschow, Joan Aufrecht, Steve Aufrecht, Joel Aufrecht, Gisela Silton, Ilonka Aufrecht, Frank Vadasz, Janelle Taylor, Michael Libes, Oliver Mazner, and Negin Almassi.

In between the conversations, there must be a pause for the very lonely act of writing. Yet it is also true that it takes a community of support to create the lonely space that makes writing possible. My endless thanks to the community of IHOT for supporting me the whole way: Neena Makhija, Emily Lynch, Aura Weinbaum, Ben Dunlap, Matt Freedman, Christine Underwood, Neeta Makhija, Brooks Miner, Ken Roeder, Tiberio Simon, Dave Citrin, Ellie Miner, Rama, Kirra Swenerton, Davida Finger, Sydney Lewis, and the wonderful Jennifer Drake.

For keeping the lonely space in my head clear and inspiring me to think of new possibilities, I am grateful to Vernor Vinge, Ursula K. Le Guin, O. S. Card, George R. R. Martin, Octavia Butler, and Dan Simmons.

From start to finish, this project is a product of walks, talks, lunches, and emails with my dear friend and colleague Ben Almassi. His insightful questions, helpful examples, and gentle way of pointing out devastating criticism made it possible for this Phoenix to rise up again and again. Our dissertations truly are twin projects and I look forward to forging many more such projects in the future.

DEDICATION

To my teacher Janis Fleischman, who started me on this journey when she stopped in the middle of a classroom lecture on electron orbitals and started laughing. "For all you know," she said, "I could be making this all up!"

1. Introduction

For talking about complex meanings, uses: A single text cannot be everywhere at once. It cannot do everything all at the same time, nor tell all… The short list that follows does not claim to catch everything… Our list does not present a history of the literatures, the field, or the problem, but instead it is special in its character. It reflects a desire to make a space, define outlines, sketch contours – and then to walk through what has been laid out.

- John Law and Annemarie Mol, *Complexities: Social Studies of Knowledge Practices*

One morning, walking on the bluff, the idea came to me…

- Henri Poincaré

Physicist Henri Becquerel forgets a piece of uranium in a drawer, which leads to the discovery of radioactivity. Friedrich Kekulé, Henri Poincaré, and Archimedes each make their big breakthroughs while letting their thoughts wander. Such stories of fortuitous scientific discovery are linked to "beds, bicycles, and bathrooms," as Thomas Sturm and Gerd Gigerenzer phrase it. Of these instances, one can ask: Is the origin of each idea relevant to whether we should accept the idea? That is, is its so-called "context of discovery" relevant to its "context of justification"?

What about more complex stories of theory generation? Inspired by sunworship, Johannes Kepler places the sun at the center of the earth's orbit, and then labors to derive the shape of this orbit. Driven to feed the masses and guided by Marxist theories, farmer Trofim Lysenko develops new ways to grow wheat, which will end in failure. Primatologists bring expectations of male aggression and female passivity to their observations of primates in the wild (Haraway 1989), while Darwin's theory of natural selection is inspired by the Malthusian models of competition for scarce resources and linked to capitalism, and Einstein's obsession with trains and simultaneity is shaped by the key questions of his time (Galison 2003). Here we ask a different question: Do the values, hopes, assumptions, and commitments of each idea's author have any bearing on its validity? These are not simple stories of *Eureka*-like inspiration, followed by value-free inquiry, but rather in each case the investigation itself was infused with the themes of the day and the concerns of the researcher. Does that compromise the conclusions, or is that the way science is always done, and always should be done? Some argue that the values and other personal factors in these stories of generation are irrelevant to the justification of the conclusions. Indeed, blocking values from scientific justification is one of the main contemporary uses of the context distinction.

These sets of examples mark just two ways of characterizing the distinction between the context of discovery and the context of justification. Countless other characterizations can be found in the philosophical literature in the second half of the 20th century. Attempts to explore the role of values in science often get entangled with these debates over these other characterizations. Thus, to further investigations into the role of values in science, it is worth clarifying and distinguishing the most prominent alternative characterizations. These include the demarcation between the fields of history and philosophy, between creativity and logic, and between contingent historical occurrences and timeless scientific facts. In this project, I explore various meanings of the distinction between the context of discovery and the context of justification (or simply, *the context distinction*). Rather than trying to determine the meaning and merits of the context distinction by examining the context distinction itself, however, I focus on how the distinction is used in particular debates; I let the participants of each debate define the distinction each time. Thus, I do not offer a complete definition of the distinction here.

However, it would be fruitful to review some of the previous attempts to challenge the distinction or at least attempts to disambiguate it. Challenges to the context distinction have taken at least four different forms:

> 1) Some argue that the context distinction is misleading or false: the context of discovery is actually *relevant* to the context of justification. One of the most common arguments for this claim is the recognition that the actual processes of generating, testing, and justifying are often intertwined in scientific practice. I will explore Theodore Arabatzis's defense of this claim below. Other defenders include Feyerabend (1975), Kuhn (1962), Gigerenzer and Sturm (2007), Hankinson Nelson (1995a), Anderson (2004b), and Campbell (1998).
>
> 2) Others agree that the context of discovery of an idea is irrelevant to the context of justification of that idea, but maintain that the context of discovery nonetheless is worthy of philosophical attention. These 'friends of discovery,' as Gary Gutting calls them (Gutting 1980c), differ as to whether they think discovery has a logical and a rule-like method (Hanson 1958b), or is simply a collection of rational, interesting techniques that are worthy of study (Nickles 1980).
>
> 3) Others also keep the distinction between discovery and justification, and add more distinctions to mark the different stages of research, including the context of generation and initial thinking (Hanson 1960); pursuit; preliminary investigation; preparation (Burian 1980); understanding (Finocchiaro1980); decision (Siegel 1980a); application (Koertge 2003); and appraisal (Kordig 1978).
>
> 4) Similar to point 3, some add their own distinction, but do so by rejecting the context distinction altogether and replacing it with other suggestions, such as normative/descriptive (Hoyningen-Huene 2006), or invention/appraisal (McLaughlin 1982b).

My own project does not clearly fit into any of these categories, though it comes closest to the fourth. I suggest that we view the context distinction as a tool, and examine the ways it has been used, and whether it has been successful in achieving each of those aims. In doing so, I illuminate and explicate more versions of the distinction (see Figure 5) with the aim of uncovering what authors in any given debate are disagreeing about. These articulated versions prove to be very useful when navigating debates.

Before giving an overview of my project, I will review previous research that disambiguates and/or challenges the context distinction. This research has been invaluable for the undertaking of this investigation.

Laudan – One History of the Distinction

Larry Laudan explores the origin of the context distinction. Rather than being seen as a simple, self-evident distinction, as it is presented in Reichenbach's *Experience and Prediction*, Laudan suggests that the context distinction is actually the taking of a stand on an ongoing debate between so-called *consequentialists* and *generators.* The question at stake for both sides is "How do you justify a scientific claim?" That is, the question is firmly in the context of justification, and normative. The consequentialists had the view which is prominent now, but was the minority view in the 17th and 18th centuries: A claim is justified by comparing its consequences with observation (Laudan 1980, p. 176). That is, the claim is considered to be a final *product that must pass a test* at the end.

In contrast, the generators, with adherents as famous as Francis Bacon, René Descartes, Gottfried Leibniz and Isaac Newton, held that "theories could be established only by showing that they followed logically … from statements which were directly gleaned from observation" (Laudan 1980, p. 176). Claims and theories were justified if they were developed using a justifiable *process* (a view that Popper derides as 'proof by pedigree' (1963)). The search was for mechanical rules that could be followed that would generate scientific claims with certainty from a set starting point. For instance, one must derive theories from carefully cataloged observations, following designated steps, without making any hasty generalizations (Bacon 1620, Nickles 1980, p. 3).

Thus, by focusing on the so-called context of justification and declaring the processes of discovery as irrelevant, Reichenbach and other logical empiricists are taking a definitive stand as consequentialists and against generators. (One great source of confusion, which Laudan notes, is that Reichenbach himself is an inductivist, and N. R. Hanson, one of the first people to challenge the new distinction, adheres to abduction and evaluates claims by testing them.)

Laudan presents two clarifications of this suggestion. First, generators and consequentialists seem to have different things in mind when they conceive of the object of a scientific discovery. If you think of the object of a scientific discovery as a single proposition or law ("All planets move in ellipses" or "All gasses expand when heated" are his two examples), then it seems plausible that such a generalization or law could be derived or induced from observations: we observe several gasses, and then draw our conclusion. It seems plausible that one could develop mechanical rules for discovering and justifying these claims. However, if one considers the object of justification to be a complicated theory, rather than a general law, then the process of discovering and articulating the theory plausibly involves a more creative process; it is no longer clear that a simple set of rules could suffice. Thus, post hoc testing seems more plausible in comparison.

Laudan's second clarification is that generators are committed to scientific certainty. They long since recognized that after-the-fact testing, especially in the form of verificationism, is fallible, since one can never be sure that there aren't other, unthought-of theories that would make the same predictions as the theory being tested. Only once the generators' project was recognized as fallible, too, and infallibilism was abandoned, then it became plausible to be a consequentialist.

If we step back from Laudan's history and think of where the context distinction stands now, we see that we have come full circle. One of Kuhn's motivations for claiming to challenge the context distinction is to reject the notion that there are sets of rules one could follow that would provide a *test* for final claims (so he seems to reject the consequentialist notion). Yet he also rejects that there is a set of rules of discovery from which one could *build* a justified claim (the generators' quest). So Kuhn refuses both sides of the dichotomy. It should come as no surprise, then, that Kuhn's views on the context distinction appear confused and contradictory, since he strikes out in a new direction, or as he often says, rejects the framework from which the distinction is posed.

Other attempts to make sense of the context distinction

Hoyningen-Huene – Many Meanings of the Distinction, but One Core

Paul Hoyningen-Huene's 1987 paper is the most authoritative paper on the subject. In it, he articulates seven versions of the context distinction that can be found in the literature. The seven versions distinguish between:

a) Temporal Processes (i.e., discovery happens first, and is followed by justification)
b) Factual/Normative Processes
c) Empirical/Logical Factors
d) The Fields of History, Psychology, Sociology / The Field of Philosophy
e) Types of Questions
f) External/Internal Factors
g) Descriptive/Normative Perspectives

Hoyningen-Huene also catalogues five common types of attacks on the distinction. I have drawn from his list of five attacks to formulate the list of challenges above, and so will not repeat them here. While Hoyningen-Huene finds much to criticize in these seven distinctions, he recommends a core context distinction that should be acceptable to all parties. In 2006, he expands on this recommendation. He suggests that at its core, the context distinction is about descriptive and normative perspectives from which to ask questions about scientific theories. In addition, Hoyningen-Huene offers an analysis of Kuhn's relationship with the distinction, concluding that Kuhn challenged the Empirical/Logical distinction, but not this core distinction.

Hoyningen-Huene's contributions are invaluable in that they articulate several versions of the distinction that had been run together, and connect each one with authors who supported or challenged the given view. This project draws heavily from these accounts, as will be evident throughout.

Nickles v. Hanson – There are Logics of Discovery

One of the claims surrounding the context distinction is that the context of justification (whatever it might be) is the proper subject matter for philosophers, and that philosophers should not investigate the context of discovery. N. R. Hanson is seen as one of the major challengers of this claim when he argues that discovery is philosophically interesting and has a logic (Hanson 1958). He distinguishes between the reasons for *accepting* a hypothesis, and the reasons for *suggesting* a hypothesis in the first place. He then argues that philosophers have neglected the latter topic, to their detriment.

It is one of the great confusions of this topic that Hanson is heralded as the first 'friend of discovery' and an advocate for promoting a Logic of Discovery while on closer examination he is much closer to the camp of those who make ever finer distinctions. As Thomas Nickles notes, the true friends of discovery try to challenge the assumption that discovery is an unanalyzable, a-rational, creative process best left to psychologists and historians. They should try to create a space for philosophical analysis. Yet Hanson simply reinforces the view that discovery is unanalyzable: he clarifies that by 'reasons for suggesting a hypothesis,' he means reasons for 'investigating a hypothesis *once* it has been thought of,' rather than the actual generation process. He describes Newton's thoughts while puzzling out the Law of Gravitation as "irrelevant," and writes that, "Kepler's *De Motibus Stellae Martis* faithfully records his every insight, bewilderment, and blunder" but does not help answer the questions of logic and evaluation of argument that concern philosophers (Hanson 1967, p. 103-4.)

Thus, Hanson sets aside investigation of the initial generation and articulation of an idea in favor of the question of whether an idea is worthy of pursuit:

> "The Logic of Discovery'" was meant to attend not to the processes genetically responsible for [a given hypothesis] H, but rather to such justification as there might be for suggesting H, even before H has been subjected to experiment. The argument was that, just as one can give good reasons for accepting an H after it has proved successful in predictions (and fits into extant theories), so one can give good reasons for the original suggestion of an H before theoretical or experimental scrutiny has begun. (Hanson 1960, p. 183)

So here we see one challenge to the context distinction, namely that the processes involved in evaluating the initial merits of an existing claim have a logic, and not just the process of justifying that claim in the end. In making this clarification, Hanson makes it clear that he views *the logical reasons for putting up an untested hypothesis for consideration* as worthy of investigation, but not the *creative generation of a hypothesis* itself.

Nickles objects to this setting aside of hypothesis generation. He maintains that, for instance, the characterizations we saw earlier of sudden inspiration are caricatures[1] and the actual thinking up of an idea is much more complex. Granted, he admits, this process does not have a specific logic or set rules, but routines have been developed that aid in the generation of theories more likely to be fruitful. It is to these routines that philosophers should turn their attention (Nickles 1980). As Martin Curd, another friend of discovery, quotes Pierre Duhem:

> The ordinary layman judges the birth of physical theories as the child the appearance of the chick [from an egg]. He believes that this fairy whom he calls by the name of science has touched with his magic wand the forehead of a man of genius and that the theory immediately appeared alive and complete, like Pallas Athena emerging fully armed from the forehead of Zeus. (Duhem 1954, p. 211)

If the generation of ideas in the context of discovery is taken seriously, it is argued, the philosopher would find much to investigate beyond this caricature.

Arabatzis and Steinle– *Discovery and Justification are intertwined*

A recent anthology edited by Jutta Schickore and Friedrich Steinle, *Revisiting the Context Distinction,* offers several interesting analyses of the distinction and its history. In this volume, Theodore Arabatzis presents one of the more recent attempts to challenge the distinction. He focuses on what it means to "discover" something, and notes that the word is used for the discovery of at least seven different types of things, ranging from 1) directly observable entities, to 2) properties of established entities, to 3) scientific laws. By

---

[1] There is very interesting research in psychology on the importance of day-dreaming to help the unconscious process complex ideas. Even the Kekulé case has even been examined to show that the account of Kekulé's inspiration is more complicated than Kekulé himself relates, and that there is reason to think he had been exposed to the idea before hand (see, for instance, Schaffer 1994).

analyzing, for instance, the 4) discovery of a phenomena during controlled experiments (the Zeeman effect) and 5) an unobservable entity (the electron), he highlights the many ways in which investigations involve justifying the validity of the results along the way, and how this justification provides clues for the next steps of investigation (Arabatzis 2006, p. 222). This leads him to conclude that the contexts of discovery and justification are "inextricably linked" (Arabatzis 2006, p. 217).

However, Arabatzis uses a different understanding of discovery, distinct from 'generation of ideas.' He consciously treats 'discovery' as what Curd has called a 'success term' (Curd 1980, p. 201), meaning that one cannot claim to have discovered something unless one has reason to believe it actually exists. The terms 'generation' and 'construction' leave open whether the idea is justified, Arabatzis writes, while "'Discovery,' on the other hand, implies truth" (Arabatzis 2006, p. 218). Curd and others explicitly avoid using 'discovery' in this sense, and focus instead of the notion of generation of ideas. Arabatzis sees this as a mistake, since most philosophical discussions about discoveries refer to successful ideas (e.g., he suggests that no one refers to phlogiston as a discovery), and that it is philosophically fruitful to study the testing involved in creating a discovery. This redefinition of the context distinction, where the context of discovery now refers to the testing of a hypothesis, allows Arabatzis to say convincingly, "The context of discovery is 'laden' with the context of justification because 'discovery' is a term which refers to epistemic achievement" (Arabatzis 2006, p. 217).

Arabatzis's focus on the process of developing and testing an idea with specific case studies is very helpful. It also allows him to challenge a common version of the context distinction, namely that there are distinct temporal processes of theory testing and theory justification. However, I am unconvinced that he has shown good reason to set aside questions of idea generation. In doing so, he is no longer offering the challenge to the context distinction that he initially appeared to be. He admits that,

> Despite critical remarks that have been raised against the distinction between discovery and justification, one can still distinguish between the original historical mode of hypothesis generation and the "final" form of justification. These two aspects of the discovery process need not coincide. (Arabatzis 2006, p. 218)

It seems that there might still be opportunity to find a process of *generation* that involves justification.

Friedrich Steinle, in the same anthology, offers such a process in the case of Charles Dufay's development of two electricities in the 18th century (phenomena corresponding to what we would now think of as positive and negative polarizations). Steinle adds a new category to our list of processes, namely 'exploratory experimentation.' He argues that such experimentation leads to new idea generation through "systematic variation of experimental parameters" to produce new effects and formulate new empirical laws or regularities to explain these effects (Steinle 2006, p. 186). This process often involves the creation of new concepts and the challenging of old concepts. Thus, "as a means to grasp the process of science, the DJ [discovery/justification] distinction is useless and may even hinder understanding" (Steinle 2006, p. 187).

Here it is important to pay attention to Steinke's aim: to understand the process of science. As we will see later, this is a different aim from, say, determining whether a scientific claim is justified or developing a methodology for scientific justification in general.

Thus, there is reason to think that even in the generation of an idea, justification is present. But what about the other way around? Once an idea has been developed, must one always refer to its development process? Can one evaluate its merits after-the-fact? Can the context of justification involve the context of discovery? Or can justification stand on its own? As Steinle quotes one of his professors saying: "I do not care about how Newton historically found his law of gravitation; I'm just interested in why it is valid." (Steinle 2006, p. 188).

Steinle suggests that this distinction might be possible when the concepts are fixed, although the separation is less plausible during times when concepts are changing. He writes,

> Justification and validity can be separated from genesis only if the conceptual framework on which they rely is taken for granted and left untouched. At the moment, however, when the conceptual framework is taken into account and open for discussion, the genesis and the historicity of concepts come in and remain there irreducibly. In this perspective, justification and genesis can no longer be neatly separated. (Steinle 2006, p. 192)

This separation between two periods is reminiscent of Kuhn's separation between normal science and paradigm shifts, and as we will see in Chapters 3 and 4, Kuhn offers a similar view: versions of the context distinction hold during normal science when the conceptual framework is fixed, but not during periods of revolution that involve changes in frameworks.

Okruhlik – Discovery affects the content of science, not just justification

Kathleen Okruhlik takes a feminist philosophy of science perspective to explore how male bias can enter the content of science. Okruhlik does not challenge the context distinction itself but rather argues that if one of its aims is to keep science objective and free from bias, the distinction will fail at that aim. She notes that if one is expected to test one's theory directly against nature, then it makes sense to claim that the generation of an idea is irrelevant to its justification. Without endorsing the view, she describes it in the following way:

> [The general idea is that] if you arrived at your hypothesis by reading tea leaves, it doesn't matter so long as the hypothesis is confirmed or corroborated in the context of justification. You test the hypothesis in the tribunal of nature and if it holds up, then you're justified in holding on to it – whatever its origins. (Okruhlik 1994, p. 200)

However, with a new conception of science, this version of the context distinction no longer applies in the same way. First, theory evaluation is now recognized as a comparison between two or more rivals, not a direct comparison with nature. She cites Clark Glymour in noting that "Confirmation is a three-place relation, not a two-place relation" (Glymour 1980, p. 151). Thus, one is always choosing theories from the ones available. If one also admits that values play a role in theory generation (as most players in these debates do, with the notable exception of Koertge), then values will already be embedded in the theories one has to choose from. This holds even if one grants for the sake of argument that the context of justification (i.e., theory choice) is value free.

> If our choice among rivals is irreducibly comparative, as it is on this model, then scientific methodology cannot guarantee (even on the most optimistic scenario) that the preferred theory is true – only that it is epistemically superior to the other *actually available* contenders. But if all these contenders have been affected by sociological factors, nothing in the appraisal machinery will completely 'purify' the successful theory. (Okruhlik 1994, p. 201)

If all the available theories are based on, say, similar sexist premises, then the unwanted biases will sneak in through theory generation and will not be filtered out during theory choice. For example, many theories have been proposed about the source of spatial ability, and why this ability varies among individuals. Suggestions for its cause include high levels of prenatal androgen, low levels of estrogen, increased laterization of the brain, decreased laterization of the brain, and so on. All of the proposals share the assumption that men in general have better spatial ability than women, and that this ability is based in biology, not society. Thus, proposals that reject these two assumptions are not under consideration, and their merits cannot be considered along with the rest.

This is not a challenge to the context distinction *per se*, but rather to how it is used. If all theories generated are based on sexist assumptions, then "non-sexist rivals will never even be generated" and so there won't be any non-sexist theories to choose from. As a result, "the very *content* of science will be sexist, no matter how rigorously we apply objective standards of assessment in the context of justification" (Okruhlik 1994, p. 202). If we want to ensure that the content of science is free from sexist bias, then, we turn out attentions to theory generation, and not just theory justification.

This brief survey provides a sampling of the diverse sorts of debates about the context distinction, its merits and its drawbacks. Though many themes reoccur, the role of values being a prominent one, there are no clearly fixed debates or camps. Authors who find themselves on one side of the debate for one particular issue often find themselves disagreeing on other issues (for instance, both Kuhn and Siegel agree that there is an interesting "context of decision" separate from "context of discovery," but they disagree on what makes it interesting. And while Koertge can be interpreted as a 'friend of discovery' because she searches for methods of discovery, she would disagree strongly that one should collapse the distinction). This project aims to navigate a path through the debates.

Overview of the Chapters

In their book, *Complexities*, John Law and Annemarie Mol define complexity as whatever refuses to be organized, to be fully described by a list, or compartmentalized into exhaustive categories. While in this project I do attempt to find organizing principles from which to make sense of various uses of the context distinction, these uses and the accompanying meanings are overlapping and by no means mutually exclusive or all-encompassing. Each use of the context distinction is ever so slightly focused on a different object of study, and each recognizes a different type of answer. As Law and Mol would say, each brings a different aspect to the foreground while letting other items slip away unnoted.

In this project I offer vignettes to illustrate different uses of the context distinction in key conversations of philosophy of science during the twentieth and twenty-first centuries. I begin with Logical Empiricist Hans Reichenbach, who first coined the distinction between "the context of discovery and the context of justification" in his 1938 *Experience and Prediction.* I compare Reichenbach's definition of the distinction with the recent attempts to define it. I discover that the meaning has changed substantially in the last 70 years. Paul Hoyningen-Huene (2006) suggests that the distinction is between descriptive and normative accounts of scientific theories. In contrast, I argue that Reichenbach distinguishes between two types of descriptions: the actual thinking processes of scientists and their "cleaned-up" arguments for public presentation, and that this does not match up with current uses of the distinction.

Next, I turn to Thomas Kuhn's influential theory of "paradigm shifts" (1962), which was largely considered to be a challenge to the context distinction and inspired many philosophers to abandon the distinction completely. If Kuhn is right that a scientist's judgment is dominated by the norms of the paradigm in which she works, then it would be impossible to separate the evaluation of evidence for a theory from the historical situation in which the theory was developed. That is, one would need to take into account the historical context in so far as the paradigm is considered part of the historical context, and the norms needed to evaluate the claim are part of the paradigm. Yet Kuhn's relationship with the context distinction is much more nuanced than this implies. In this chapter I specify three major questions in philosophy of science and show how the context distinction manifests itself differently depending on which question one is concerned with. Critics have used each manifestation to argue against Kuhn, all under the same labels "context of discovery" versus "context of justification," although they are actually separate criticisms. Once the framework of the three questions is in place, it will be easier to see how and why Kuhn's views on normal science led him to reject some versions of the context distinction while, despite the claims of his critics, he still adhered to others.

Turning from normal science to paradigm shifts, one notes how Kuhn challenged philosophers to find a place for values in scientific inquiry during periods of revolution and theory choice. In the following chapter I review the familiar account of Kuhn's suggestion that epistemic values such as simplicity and coherence are crucial for choosing between paradigms. The inclusion of values was seen as a challenge to objectivity and rationality in science, as well as a violation of the context distinction. I show how ambiguity between various versions of the context distinction, such as between what I call the Psychological Distinction and the Values Distinction, led to confusion around Kuhn's argument. Clarifying these distinctions can help us understand what is at stake in the question of whether values are relevant to justification. For instance, one might accept the Psychological Distinction while rejecting the Values Distinction. That is, you could accept that the thought processes of the original scientist who developed and tested a theory are irrelevant to whether the theory is in fact justified, while still maintaining that when someone goes to determine whether a theory is justified (regardless of whether she developed the theory herself), her values play a legitimate role in theory justification.

One of the more striking uses of the context distinction is in arguments to undermine feminist approaches to philosophy of science. Noretta Koertge charges scholars such as Helen Longino and Lynn Hankinson Nelson with violating core tenets of philosophy when they reject the context distinction. I argue that Koertge is best interpreted as charging feminist approaches with making a category mistake; yet whether a violation of the context distinction is indeed a category mistake depends on views about the very nature of justification; the connection between philosophy and other fields such as history and sociology (e.g., when philosophers should look to scientific practice); and on notions of objectivity. I argue that the ambiguity of the context distinction in these debates masks these underlying disagreements about the proper role of values in justification, what justification consists of, and what objectivity can do for us.

This is a story about placing order on complexity. Whether one is trying to order a complex world, a set of ideas, or an approach to inquiry, one must offer simplifications, categories, and distinctions. In this project, I explore the attempt to use a seemingly simple distinction to place order on how we should approach the natural world.

I also explore the themes of multiple-levels of inquiry. At the highest level, the story itself is an attempt to place order on complexity, namely the complexity of the distinction, its many meanings, and the diverse uses to which it has been put. It is not a complete history, since many more stories could be told about the distinction. Rather, I offer vignettes on uses that have been fruitful, and others that have not faired as well.

At another level, this is also a story about the aims and methods of philosophy of science. Regarding method, how should philosophers go about studying science? How should they engage the work of historians, sociologists, and psychologists, if at all? Should philosophers *become* historians, sociologists, etc.? For instance, the context distinction, a seemingly innocent attempt to organize complexity, has been used to argue that philosophers should restrict historical settings and events to be illustration and inspiration for understanding science, and not use them as evidence for any given methodology.

In this project, then, I show how one's use of the context distinction reveals underlying commitments about the proper aims and methods of a philosopher, and I explore to what extent these various methods are compatible, and where they conflict.

2. Genesis: Hans Reichenbach

The scientist may use platonic class constructions, complex numbers, divination by inspection of entrails, or any clap-trappery that he thinks may help him get the results he wants. But what he produces then becomes raw material for the philosopher, whose task it is to make sense of all this: to clarify, simplify, explain, interpret in understandable terms. The practical scientist does the business but the philosopher keeps the books.

- Nelson Goodman, *Problems and Projects*

I. Introduction

How is philosophy of science distinct from other fields of science studies? One traditional answer is that philosophy addresses issues about evidence and justification for scientific claims, whereas other fields study the contingent events and attitudes leading up to those claims. The former is traditionally referred to as the "context of justification" and the latter as the "context of discovery," and the distinction between them as "the context distinction" or more recently as "DJ." For example, the arguments and evidence for the theory of relativity would be considered part of the context of justification, while the biographical details of Einstein's life would be part of the context of discovery.

For many years, DJ was taken to be a starting point for philosophy of science, delineating the job of philosophers from historians, sociologists, etc. (Steinle and Schickore 2006). After Kuhn, the distinction was reexamined; some scholars threw it out entirely, arguing that it is too restrictive to focus on the content of a scientific argument while setting aside or ignoring the historical context in which the science was developed. Debates ensued with little resolution. (A colleague, reluctant to reopen this can of worms, recently called DJ "the dreaded distinction"). Some philosophers continued to focus on the content of scientific theories without reference to the historical background; others explored historical cases in more detail (Nelson 1990, Anderson 1995). Unfortunately, discussion across the divide is limited.

Recently, however, some have begun to reexamine the basis for both the rejection and the acceptance of the distinction (Schickore and Steinle 2006, Kellert 2008). In this paper, I examine one early and extremely influential description of the context distinction. Hans Reichenbach introduced the phrases "context of discovery" and "context of justification" in his 1938 *Experience and Prediction,* one of the first books he wrote in English after being exiled from Germany.[2] I compare his description of the distinction with one recently proposed by Paul Hoyningen-Huene, and I demonstrate that the two are incompatible. Hoyningen-Huene suggests that the context of discovery corresponds to a descriptive perspective on science and the context of justification corresponds to a normative perspective. As I will argue, however, Reichenbach's context distinction does not line up in this way; for him the context of justification is not normative after all, but is simply a

[2] While the idea has arguably been around much longer, Reichenbach clearly coined the English phrases "context of discovery" and "context of justification." He mentions the context distinction at least once in German in a 1935 letter to *Erkenntnis*, where he distinguishes between "Auffindungsverfahren" (discovery processes) and "Rechtfertigungverfahren" (justification processes). However, he offers very little explanation, instead directing the reader to his forthcoming book. Some attribute the distinction to Karl Popper, who first mentions something like it in *Logik Der Forschung* (1934). Referring to Kant's *quid facti* and *quid juris*, Popper distinguishes between "Tatsachenfragen" and "Geltungsfragen," which he translates as "questions of fact" and "questions of validity" respectively for the English edition (1959). Reichenbach and Popper corresponded on these ideas at the time, so some overlap is to be expected even as their conceptions of the distinction differed.

sophisticated description of the context of discovery. That is, both the context of discovery and context of justification are descriptive (although very different kinds of descriptions). My aim here is not to evaluate the heuristic usefulness of this recent proposal, but rather to show that it is incompatible with at least one influential version of the context distinction. My findings suggest that common ground is more elusive than many think; when we uncover the many meanings and uses of the context distinction, we find fewer, not more, points of agreement.

In the following section, I briefly sketch Paul Hoyningen-Huene's recent suggestion for thinking about the context distinction in philosophy of science. It will serve as a point of contrast from which to ask questions about and understand the nuances of Reichenbach's own use of the distinction. Then, I turn to Reichenbach's own text. Before examining his account of the context distinction, however, I offer a close reading of the passages in which the context distinction first appears. Prominent in these passages is his notion of *rational reconstructions*. Since my reading suggests that his notion of rational reconstructions is importantly different from more well-known accounts of rational reconstructions, such as those put forth by Rudolf Carnap (1928) and Imre Lakatos (1970), I will take a brief detour to compare his account with those other accounts. I will then define Reichenbach's context distinction based on my textual analysis of the passage and his notion of rational reconstruction. This analysis will allow me to return to Hoyningen-Huene's Lean DJ to see how it fits with Reichenbach's DJ.

II. Hoyningen-Huene's "Lean" DJ distinction

One promising line of research is to catalogue the different uses and meanings of the distinction. If we can see that scholars in the debate mean different things, this will help us to identify common ground. Paul Hoyningen-Huene has attempted to do just that. After offering an impressive catalogue of the arguments in the early debates over the distinction, Hoyningen-Huene argues that he has found one universal point of agreement:

> Actually, I do believe that there is a core of the DJ distinction that has, to the best of my knowledge, never been attacked in the discussion about it. …What I have in mind is the distinction between the factual on one hand, and the normative or evaluative on the other hand. … From the descriptive perspective, I am interested in facts that have happened, and their description. Among these facts may be, among other things, epistemic claims that were put forward. From the normative or evaluative perspective, I am interested in an evaluation of particular claims. … By using epistemic norms we can evaluate particular epistemic claims according to their being justified or not. (Hoyningen-Huene 2006, p. 128-9)

Here Hoyningen-Huene suggests that the controversies surrounding the distinction focus on extraneous features that various authors have added to the distinction and that at its core the distinction is much less controversial. He offers a "lean" DJ distinction that purportedly isolates the core features of DJ: rather than thinking about the contexts of discovery and justification as processes or events in time, we should think of them as *perspectives*. The part of the context of discovery on which everyone would agree corresponds to *a perspective from which one asks about the facts of a scientific case*. The core part of the context of justification corresponds to *a perspective from which one asks about the justification of a science claim*. As Sturm and Gigerenzer put it:

> The point of [Hoyningen-Huene's lean] version … is that we should distinguish between different types of *questions*: For any given claim p, we can always ask, "How did someone come to accept that *p*?" This question, which may be understood

> as a question about the generation or actual acceptance of a claim, differs in principle from the question, "Is *p* justified?" (Sturm and Gigerenzer 2006, p.134)[3]

While many disagree on what is relevant to the "context of discovery," for example, these authors argue that most agree that there is a useful distinction between the descriptive and the normative (Hoyningen-Huene 2006, p. 129). This view suggests that discussion of historical context and biographical information is a red herring, since the real issue is the difference between *describing* a scientific discovery and *evaluating* that discovery. Once we agree that the key distinction concerns descriptive v. normative, we then ask whether historical information is necessary for evaluating the scientific discovery or whether the two are intertwined (Hoyningen-Huene 2006, p. 129).

I applaud Hoyningen-Huene's efforts to identify the true points of agreement and disagreement behind the vague label "contexts of discovery and justification," and I am tempted by the tidy new version of the distinction he has offered. However, I suspect that even this distinction between the descriptive and normative is not without its problems. At the very least, I argue, the "core" identified by Hoyningen-Huene is not universal to all versions of the context distinction after all. Reichenbach's use of the distinction is extremely influential, if not foundational, to the use of the distinction in philosophy of science and yet, as I will demonstrate, Reichenbach's context distinction does not line up with Hoyningen-Huene's descriptive v. normative distinction.

---

[3] This description is even more specific than Hoyningen-Huene suggests, but I think it nonetheless nicely captures the general attitude of the proposed distinction.

III. Reichenbach's Four Tasks

As a founding member of the Berlin Circle, the intellectual cousin of the Vienna Circle, Reichenbach was associated with logical empiricism and its aim to develop methodological principles for characterizing scientific theories; these principles were to be informed by and based on the best science of the time.[4] Where other proposed principles include verificationism and Popper's falsificationism, Reichenbach defends his own proposal that induction is the basis of all science. The bulk of *Experience and Prediction* is dedicated to surveying rival proposals and defending his own, especially against Hume's problem of induction. The context distinction appears briefly at the beginning and the end of the volume. It is significant, I think, that the distinction is relegated to the margins of his broader project. While in many ways it serves as a foundation, the distinction is not the focus of Reichenbach's attention. He offers it in passing and with little argumentation to "clear up much confusion" (6).[5] He assumes, as others did for years following, that once the distinction between discovery and justification was made, it would be accepted as obviously useful.

Yet the meaning of the distinction is far from obvious. Since the context distinction has taken on so many meanings over the years, it is important not to read anachronistic meanings into Reichenbach's DJ. Thus, I seek to explain in detail the context of the passage in which he introduces the phrases "context of justification" and "context of discovery" in order to allow us to see the role these contexts play for him rather than for us.

[4] To what extent the philosophy was to inform the science, or the science was to inform the emerging philosophy, is a matter of dispute. See Friedman (1996, p. 182-185).
[5] All page references to Reichenbach refer to (1938), unless otherwise noted.

In the first few pages of *Experience and Prediction*, Reichenbach establishes what he takes to be the three tasks of epistemology (or philosophy [6]): the descriptive, critical, and advisory tasks. His aim is to avoid "many false objections and misunderstandings" by clearly demarcating the job of the philosopher from that of the psychologist (6). Philosophers of science, Reichenbach charges, should first take care to *describe* the body of knowledge presented by scientists by looking at the arguments these scientists use to reach their conclusions, not at the conclusions themselves (5). Second, philosophers should *criticize* or analyze those arguments to see whether they can be interpreted as proper scientific arguments when measured against the preferred meta-methodology (for Reichenbach, induction) (5-6). Finally, philosophers should *advise* scientists on the logical consequences of the decisions they must make. All three tasks are contrasted with the single *task of psychology*. I will examine each of these four tasks in turn, and identify the role that the context distinction plays in these tasks.

1. Task of Psychology

The task of psychology is to describe the actual thinking process of the scientist. To return to a previous example, the psychologist would study Einstein to understand his thought process when he developed the theory of relativity. (It is unclear how this is supposed to happen: perhaps by talking to him, reading his correspondence, or observing his work habits.) A full account of the thought process might include, for example, the trial-and-error of discovery or how Einstein drew associations between clock synchronization at train

[6] Reichenbach's use of "epistemology" is much closer to common use of "philosophy" today. See Uebel (2007, p. 6). Here I use both interchangeably.

stations and conceptions of space and time.[7] The goal would be to understand how scientists think, using, say, Einstein as a case study.

2. The Descriptive Task

The task of psychology contrasts most clearly with the philosopher's first task, the descriptive task. Both the psychologist and the philosopher describe aspects of scientific research, but each describes a different aspect (see Figure 1). Whereas the psychologist describes the actual thinking process of the scientists the philosopher should be interested in the *idealized version* of that thinking process. One might be tempted at this point to conclude that, according to Reichenbach, the psychologist studies how scientists actually think while the philosopher studies how scientists ought to think. This conclusion is not entirely wrong, but it is misleading in subtle, and important, ways. To understand why, we must first grasp Reichenbach's particular conception of rational reconstruction.[8]

The object of the rational reconstruction process is itself a process, namely a thought process (5).[9] The product is a series of logical symbols. Reichenbach writes,

> Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system… Epistemology thus considers a logical substitute rather than real processes. For this logical substitute the term *rational reconstruction* has been introduced. (5)

The philosopher begins with a scientist's thought process, then writes down an idealized

---

[7] Although he mentions his friend and mentor Einstein frequently, Reichenbach does not offer examples as explicit as this. I draw upon (Galison 2003) for this example.

[8] Unfortunately, the phrase rational reconstruction refers to a product, as well as a process, just like the words "film production," and "test," and even the adjective "objective" (see Fine 1998).

[9] Much confusion arises out of this, since the philosopher does not have direct access to the scientist's thought process. At best, she has an oral account or a written document produced by the scientist, describing what he takes to be his own thought process.

version of that thought process, thereby creating a written chain of reasoning that can be subjected to logical evaluation: "rationally reconstructed knowledge can only be given in the language form … for thinking processes enter into knowledge ... only in so far as they can be replaced by chains of linguistic expressions" (16-17). I imagine that Reichenbach has in mind something like the following: In describing his own thinking process, Einstein writes that considerations of Maxwell's equations led him to reconsider the nature of gravity. Seven years passed between this realization and his formulation of the general theory of relativity because "it is not so easy to free oneself" from traditional notions of space and time (Einstein 1979, p. 63). A rational reconstruction of this process would result in a logical formulation of the evidence for relativity without necessarily mentioning Einstein's detours on this path to developing the theory. Note that the rational reconstruction must be performed with the end goal in mind, which is to evaluate the scientist's argument using the prescribed logical system. This means that in constructing the rational reconstruction, the philosopher must highlight important features necessary for induction and eliminate distracting features such as "abbreviations and silently tolerated inexactitudes" (Reichenbach 7).

I have described three features of Reichenbach's rational reconstruction: the input, the output, and the process that creates that output. The rational reconstruction must take a scientist's own thinking and transform it. Although many transformations are possible, the required one will be that which best prepares it for logical analysis. At first glance, this seems like a typical description of a rational reconstruction. But the fourth feature of Reichenbach's rational reconstruction, I think, is what distinguishes it from other versions. Namely, Reichenbach requires that a rational reconstruction must adhere closely to the original thought process. It must *not* be transformed beyond all recognition. Now, the philosopher may change the argument, adding logical steps that were hidden in an enthymeme, or adding whole new steps, but these changes must be performed with caution. The philosopher is required to stay true to the original meaning. Reichenbach writes, "The construction given is not arbitrary; it is bound to thinking by the postulate of correspondence" (6). The scientist must always be able to recognize the cleaned-up version, to look at it and say, *Yes, that is what I meant all along*. It must not be transformed beyond all recognition (6). Thus, although many different reconstructions are possible, not all are permissible.

So we see that Reichenbach's rational reconstruction is constrained from two sides. On one hand, the process is constrained by the tools and standards of the evaluation process (in this case, rules of logic), and so must transform a scientist's thinking into a format that makes it susceptible to, and ready for, evaluation. On the other hand, the product must remain true to the input, that is, to the scientist's actual thought process. One can take an argument with obvious but unspoken premises and add them; however, one cannot take an invalid argument and transform it into a valid one if that would risk losing some of the original meaning.

These constraints have significant consequences. In particular, the end product (the rational reconstruction) may yield a claim that is not justified. Reichenbach writes:

> It may happen that the description of knowledge leads to the result that certain chains of thoughts, or operations, cannot be justified; in other words, that even the rational reconstruction contains unjustifiable chains. … This case shows that the descriptive task and the critical task are different; although description, as it is here meant, is not a copy of actual thinking but the construction of an equivalent, it is bound to thinking by the postulate of correspondence and may expose knowledge to criticism. (8)

The process of the rational reconstruction might reveal a justified final claim, or it might not, instead revealing an unjustified final claim. As I will argue below, this is in contrast to more prominent accounts of rational reconstruction, such as those of Carnap or Lakatos, in which the end product of a rational reconstruction is, by design, logically or rationally justified.

b. Contrast with Carnap's Rational Reconstruction

Perhaps one of the better known accounts of rational reconstruction appears in Rudolf Carnap's 1928 *Logical Structure of the World* (The *Aufbau*). Reichenbach refers to this *rationale Nachkonstruktion* (Reichenbach 5) although, as we will see, his notions and uses for rational reconstructions differ from Carnap's in important ways.

In the *Aufbau*, Carnap sets out to create a construction theory in which one builds "the rational reconstruction of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given" (Carnap 1969, p. v). Although Carnap offers several changes to his account over the years, he generally aims to translate direct sensory experiences into linguistic form so that scientific knowledge can be objective.[10] For Carnap, objectivity requires intersubjective agreement; different people must be able to have the same experience. Yet how do we know that intersubjective agreement has been reached? Since no one can share their direct sensory experiences, they must share instead the language to talk about their experiences. (Carnap 1969, § 3 p. 7; Uebel 2007, p. 16). Both constructions and rational reconstructions, for Carnap, are the translation of direct experiences into linguistic form (Carnap 1969, p. 308).

---

[10] See (Uebel 2007, p. 19-24, 54-60) for an account of those changes. See also Friedman (1996), Richardson (1996) and (2000) for challenges to traditional interpretations of Carnap.

At first glance, Reichenbach's reference to Carnap seems perfectly on target. Carnap offers a precise definition of rational reconstruction that fits very well with Reichenbach's usage: *"… an inferential procedure whose purpose it is to investigate whether or not there is a certain logical dependency between certain constituents of the experience*" under consideration (Carnap 1969, p. 310). This definition includes the necessary emphasis on the logical relationship/logical dependency between the experiences, and at the same time excludes the subjective perception of those experiences (the evaluation of which belongs to psychology).

Moreover, we can also use the definition of *construction* to make sense of *rational reconstruction* if we keep in mind that a reconstruction is simply an after-the-fact construction (*Nach*konstruktion):

> To *construct a* out of *b, c* means to produce a general rule that indicates for each individual case how a statement about *a* must be transformed in order to yield a statement about *b, c*. (Carnap 1969, §2, p. 6)

If a concept *a* can be constructed out of concepts *b* and *c*, then *a* is also reducible to *b* and *c* and all the information about *a* can be expressed in statements about *b* and *c* alone (Carnap 1969, §2, §35 p. 6, 61). Carnap builds his whole system on the notions of construction and reduction, his goal being to recognize that scientific theories reduce to certain key concepts, and to articulate how that works. He writes,

> It is in principle possible to place all concepts in all areas of science into this [constructional] system, that is to say, they are reducible to one another and ultimately to a few basic concepts. (Carnap 1969, p. 308)

and

> It is the goal of each scientific theory to become, as far as its content is concerned, a pure *relation description*. (Carnap 1969, §10, p. 20, emphasis added)

Carnap wants to create a constructional system in which scientific concepts are described *in relation to* more basic concepts. Carnap has a very particular notion of construction in mind, and a *re*-construction is just an extension of that. Specifically, a rational reconstruction is a process that creates statements about a certain concept out of other concepts to which the first one reduces, after the actual thinking process has already occurred. Each step is logically related to the last step. As Richardson puts it, Carnap aims to construct "the purely mathematically expressible relations of physics" which then "takes us beyond the merely qualitative and private relations of sense-experience" (Richardson 1996, p. 314).

Carnap's notion of rational reconstruction is actually a combination of two ideas: 1) the construction of concepts consisting of mathematical relations 2) and after-the-fact logical reorganization of concepts.[11] Reichenbach's notion of rational reconstruction involves only the second idea, since his after-the-fact reorganization can apply to other logical relationships besides Carnap's notion of construction. For example, Reichenbach writes,

> In being set before the rational reconstruction, we have the feeling that only now do we understand what we think; we admit that the rational reconstruction expresses what we mean, properly speaking. (Reichenbach 1938, p. 6)

Here Reichenbach suggests that any sort of thought can be rationally reconstructed. He continues with this idea when he suggests that "even the rational reconstruction [can] contain unjustifiable chains" and that it "may expose knowledge to criticism" (Reichenbach 1938, p. 8). For Reichenbach, then, rational reconstructions are not limited to constructing valid logical inferences, and so cannot be limited to constructing valid mathematical relations.

---

[11] I do not mean to overemphasize the temporal aspect here. Although the German certainly suggests this notion of a sequence in time, the important aspect is the separation between the actual thought processes and logical orderings of those thought process, not the idea that one happens before the other.

Although Reichenbach might aim to create such relations, for him the rational reconstruction may often fall short of that goal, since giving the best version of an *actual thought process* restrains one to be true to the original process. So while Carnap's rational reconstructions always contain logical relations on which we can base the objectivity of science, Reichenbach's rational reconstructions provide organized versions of thought, faithfully maintaining any insurmountable failures of logical relations and thereby allow us to judge those thoughts against Reichenbach's proposed methodology of science.

c. Contrast with Lakatos's Rational Reconstruction

Another prominent version of rational reconstruction was developed after Reichenbach's *Experience and Prediction*. Like Reichenbach, Lakatos aims to contrast rational reconstructions against proposed methodologies of science. Like Reichenbach, he is also more flexible than Carnap in the type of relations allowed in his notion of rational reconstruction. For Lakatos, the methodology under consideration shapes the relations that should be chosen in a rational reconstruction. For example, if one wants to evaluate Popper's falsificationism, then one should create a rational reconstruction of Einstein's experiments in which you highlight bold conjectures and record which conjectures are falsified and which ones are not (yet) falsified. If one evaluates a different methodology instead, such as Lakatos' own methodology of research programmes, then one creates a rational reconstruction that highlights researchers' resistance to *ad hoc* adjustments to theories, and that shows how certain claims are fruitful or can lead to further possible experiments. Relations that seem irrational in one kind of rational reconstruction can be very rational in another kind of rational reconstruction (Lakatos 1970a, p. 112-113).

In many ways, then, Reichenbach's notion of rational reconstruction is more similar to Lakatos' than to Carnap's. However, Lakatos famously shares Carnap's desire to move away from reconstructions that reflect actual thinking processes and towards reconstructions that embody the ideal thinking process. He writes,

> In constructing internal history, the historian will be highly selective: he will omit everything that is irrational in the light of his rationality theory. (Lakatos 1970a, p. 106)

If a scientist fails to follow the thinking process that a given methodology requires, Lakatos advocates replacing it with the "correct" thinking process in the rational reconstruction (Lakatos 1970a, p. 107, 1970b, p. 146).[12] Lakatos considers the rational reconstruction to contain the *internal* thinking, the epistemically important relations (the thinking that *should have* occurred), rather than the external thinking (the thinking that actually occurred).

This external/internal distinction that Lakatos employs is close to the descriptive/normative distinction that Hoyningen-Huene proposes. It is notable, then, that when Lakatos identifies *rational reconstruction* with *internal relations*, Lakatos is not in a position to "expose" the rational reconstruction "to criticism" in the way that Reichenbach requires.

There is also textual evidence that Reichenbach's notion of reconstruction does not follow the internal/external distinction. Reichenbach shares Lakatos' definition of internal/external relations. External relations involve, for example, the extracurricular activities and social status of scientists. The sociologist might note that,

> Astronomers are frequently musical men, or that they belong in general to the bourgeois class of society; if these relations do not interest epistemology, it is because

[12] This controversial view did not go unnoticed. For objections to it, see (Kuhn 1970, p. 256) and (McMullin 1970).

> they do not enter into the content of science – they are what we call external relations. (Reichenbach 4)

In contrast, internal relations involve epistemic relations such as "the content of knowledge" and the "system of connections as it is followed in thinking" (Reichenbach 4, 5).

Reichenbach distinguishes the sociologist, on one hand, from the psychologist and the philosopher, on the other. The sociologist studies external relations of knowledge, while the psychologist and philosopher both study the *internal* relations of knowledge. For Reichenbach, philosophers and sociologists differ in *what* they study, while philosophers and psychologists study the same thing but differ in *how* they study it. Philosophers and psychologists emphasize different parts of thought processes. My reading of Reichenbach thus resists putting together *the internal, normative,* and *context of justification* on the one hand and *the external, descriptive,* and *context of discovery* on the other.

We have seen that while Reichenbach appears to use a familiar concept in philosophy of science, he maintains a distinct conception of it. For Carnap and Lakatos, rational reconstructions contain completely logical or rational relations, respectively. For Reichenbach, rational reconstructions might contain logical fallacies. Carnap also used rational reconstructions to provide a basis for objectivity in science. In contrast, Lakatos and Reichenbach both connect the concept of rational reconstructions to competing methodologies of science, though in different ways. For Lakatos, a methodology is judged by whether it produces fruitful rational reconstructions.[13] His methodology of research programmes is judged the best. For Reichenbach, the judgment goes the other way – the

[13] That is, Lakatos suggests that adopting this methodology as a way of approaching science will allow philosophers to successfully pursue interesting questions. Another benefit of this methodology is that it explains scientific activities that otherwise do not appear rational. I will return to this latter argument in Chapter 3.

rational reconstruction, not the methodology, is judged. The methodology of induction provides the rubric for judging or evaluating an episode in science, but instead of judging that episode directly, we evaluate a rational reconstruction of it. Constructing that rational reconstruction is part of the descriptive task. We are now in a position to see that the evaluation of the rational reconstruction does not happen until the next task; determining whether the scientific claims are justified is part of the critical task.

3. The Critical Task

Philosophers of science, Reichenbach charges, should first take care to *describe* the body of knowledge presented by scientists. They should not look at conclusions, but rather at the arguments these scientists use to reach their conclusions (5). Second, philosophers should *criticize* or analyze those arguments to see if they are indeed good science (5-6). Do they contain valid or invalid reasoning? Ultimately, can they be interpreted as proper inductive arguments? The bulk of the work for the analysis of science occurs within the critical task, and the rest of the book is dedicated to elaborating on this second task; Reichenbach explains what he means by induction and what we can reasonably expect to be able to know through induction (a lot, but nothing with certainty) (87). The critical and descriptive tasks overlap, as I will explain below, but they are not co-extensive. It is the critical task, then, that can be called normative.

4. The Advisory Task

Despite its name, I contend that the advisory task is not normative, as I will explain.[14]

[14] For a different view on the role of the advisory task, see (Howard 2006, pp. 7-8).

As the epistemologist's third task, the *advisory* task, does not entail telling scientists what to do *in general* (Reichenbach thinks they are doing a fine job on their own); rather, it involves helping scientists foresee the logical results of different decisions. For example, a scientist must choose a measurement system (metric, English, etc.), one of which might have practical advantages over the others (13). In discussions of space-time, the scientist must choose a geometry (Euclidian or non-Euclidian), from which certain philosophical consequences will follow (Reichenbach 1938, p. 14 and 1928, ch. 1).

> Some decisions are bound together; one decision, then, involves another, and though we are free in choosing the first one, we are no longer free with respect to those following. (Reichenbach 1938, p. 13)

There are points at which a scientist can make a decision, but once made, certain consequences logically follow.[15] In a move that reminds one of Carnap's principle of tolerance, Reichenbach refrains from saying that philosophers should advocate one choice over the other. The philosopher's task is simply to make clear what conclusions follow, so that the scientist can make his decision with as much information as possible: "we leave the choice to our reader after showing him all the factual connections to which he is bound" (14).

Although Reichenbach names this third task the "advisory" task, his purpose in drawing attention to it is not to guide scientists but rather to respond to the view that scientists often make arbitrary decisions and therefore scientific knowledge is arbitrary. Although the decisions at times may be guided by practical considerations and so may be arbitrary in some sense, he contends that the consequences of those decisions are constrained by logic and nature and so the knowledge is not arbitrary (15).

---

[15] This section is similar to the debate between Andrew Pickering in *Mangle of Practice* and Ian Hacking in *The Social Construction of What?* over decision points in scientific research.

Despite the name, then, on my reading this task does not fulfill a normative task in the sense conveyed by Hoyningen-Huene. Rather, the normative evaluation occurs in the critical task alone.

IV. Lean DJ and Reichenbach's Contexts and Tasks

So, as we have just seen, Reichenbach proposes a procedure for the evaluation of scientific theories. First, the scientist thinks. The psychologist investigates this thought process for a study on how people think. Meanwhile, the philosopher engages in the descriptive task of epistemology: she also investigates the scientist's thought process. She then changes the words, like a good editor. She clarifies vague passages, eliminates unnecessary steps, and presents the passage in its best light. She thereby creates a rational reconstruction of the original thinking process. This document is then ready to be submitted to the next task, the critical task, where the revised scientific argument is evaluated for logical rigor and either passes the test or fails. (The advisory task is beyond this process.)

If we now recall Hoyningen-Huene's Lean DJ distinction between the descriptive and the normative, Hoyningen-Huene suggests that contemporary philosophers will recognize his new distinction as identifying the shared core and clarifying the common ground amidst the confusion. But is the normative v. descriptive distinction at the core of what Reichenbach had in mind? Is his DJ captured by Lean DJ, or, at the very least, is his DJ consistent with the Lean DJ? I contend that it is not. I will consider where each of Reichenbach's tasks would fit into the categories created by the Lean DJ distinction. I will then compare this categorization with Reichenbach's own placement of the tasks in his original distinction between the context of discovery and context of justification.

Hoyningen-Huene's suggestion is to identify the Lean Context of Discovery (LD) as descriptive and the Lean Context of Justification (LJ) as normative, according to which each context is really a perspective from which to ask a question. Sturm and Gigerenzer identify the appropriate question of the LD as: "How did someone come to accept that *p*?" For example, Shapin and Schaffer (1985) ask about the personal and political influences that drew Boyle to accept experiments as evidence for his natural gas law. In contrast, the question of the LJ is: "Is *p* justified?" Is Boyle's law justified?

With this distinction in mind, let us return to Reichenbach to determine where each of his tasks would fall under Lean DJ. Reichenbach's psychologist's task and the descriptive task are both descriptive, whereas the critical task is normative. The psychologist's task and the descriptive task each describe different aspects of the scientific theory and its development; in their "retelling," both the psychologist and the philosopher must remain true to the original object of their inquiry. That means, for example, there must be a correspondence between the rational reconstruction created with the descriptive task and the actual thought process it is meant to repackage. The critical task is more obviously normative. Reichenbach describes it as follows: "The system of knowledge is criticized; it is judged in respect of its validity and its reliability" (7). Finally, as I briefly argued earlier, the advisory task is not normative in the sense Hoyningen-Huene describes. If this is right, then the advisory task is best understood as outside of Lean DJ. So at first blush, it might appear that the LD aligns with both the psychologist's task and the descriptive task, since both are descriptive, and the LJ encompasses the critical task, since it is normative.

Alternatively, since LD and LJ are perspectives from which to ask questions, then perhaps we should actually consider Reichenbach's descriptive task as part of LJ. If we were concerned with the question of LD, "How did someone come to accept that *p*?" then we would perform the psychologist's task to get the answer. But if we were concerned with the question of LJ, "Is *p* justified?," then we would need to perform *both* the critical *and* descriptive tasks. Reichenbach acknowledges a connection between these tasks: "This [critical] task is already partially performed in the rational reconstruction, for the fictive set of operations occurring here is chosen from the point of view of justifiability" (7). It appears that the two tasks partially overlap, since the rational reconstruction within the descriptive task identifies and organizes information about some scientific claim *P* in the service of precisely asking: "Is *p* justified?" That information is sent to the critical task to be evaluated, that is, to be used in answering the normative question. Thus, placing Reichenbach's descriptive task within LJ, even though the task is indeed descriptive, might seem reasonable since in performing the normative task the philosopher must do some amount of describing so that she knows what she is evaluating. Further argument would be required to definitively place the descriptive task in either LD or LJ. However, such certainty on the descriptive task is not needed for our purposes, since, as I will show, the Lean DJ fails to match up with Reichenbach's context distinction in either case.

We have seen that if we apply Hoyningen-Huene's formulation to Reichenbach's tasks, the psychologist's task is part of LD, the critical task is part of LJ, and the descriptive task is arguably part of LD or LJ (See Figure 2). So if Hoyningen-Huene is right that most scholars will recognize his Lean DJ as the core of the context distinction, and given that Reichenbach's work is an influential original formulation of the context distinction, then we should expect to find Reichenbach's own contexts of discovery and justification aligned in the same way. That is, if Hoyningen-Huene were right, then we should expect to find

Reichenbach distinguishing between the psychologist's task in the context of discovery on one hand, and the descriptive and the critical tasks in the context of justification, on the other.

But we do not. Instead, I argue, we find the context distinction drawn between the psychologist's task and the descriptive task, with the critical task left out of the contexts all together. This does not necessarily mean that we should reject Hoyningen-Huene's Lean DJ, but it does suggest that the distinction between normative and descriptive has not been lurking in the original context distinction all along, but rather has been read into it more recently. [16]

To show this, it is important to examine the setting of the original context distinction. Reichenbach introduces the famous phrases in the first pages of his book, in the opening section titled "Three Tasks of Epistemology" where he first states that systems of knowledge are sociological facts. He explains, "If knowledge were not incorporated into books, speeches, and human actions, we would never know it" (3). Therefore to study knowledge the philosopher must in part study "features of sociological phenomenon." Here Reichenbach names the first task of epistemology, the descriptive task, as part of sociology. He warns, however, that there are two kinds of social: internal relations and external relations. The descriptive task of epistemology concerns only the internal relations, specifically the "system of connection as it is followed in thinking" (4). Noticing that his definitions of sociology and thinking might differ from the norm, he emphatically warns against confusion: he means thinking at its logical best, not actual thinking. So Reichenbach introduces the task of psychology to distinguish it from the descriptive task, and the term

[16] One should not overlook Popper's influence here, which is of course also worth further study.

rational reconstruction to indicate the proper logical substitute for real thinking. He says that philosophers should study this rational reconstruction and not actual thinking.

In the next paragraph, where he offers a clarification of rational reconstruction, Reichenbach introduces the words “context of discovery” and “context of justification.” He describes the meaning of the distinction, and then labels it accordingly. Note that we are still in the section dedicated to the descriptive task:

> If a more convenient determination of this concept of rational reconstruction is wanted, we might say that it corresponds to the form in which thinking processes are communicated to other persons instead of the form in which they are subjectively performed. The way, for instance, in which a mathematician publishes a new demonstration, or a physicist his logical reasoning in the foundation of a new theory, would almost correspond to our concept of rational reconstruction; and the well-known difference between the thinker’s way of finding this theorem and his way of presenting it before a public may illustrate the difference in question. I shall introduce the terms *context of discovery* and *context of justification* to mark this distinction. Then we have to say that epistemology is only occupied in constructing the context of justification. But even the way of presenting scientific theories is only an approximation of what we mean by the context of justification. Even in the written form scientific expositions do not always correspond to the exigencies of logic or suppress the traces of subjective motivation from which they started. (6-7, Emphasis in the original)

Reichenbach’s aim in this paragraph is to distinguish between actual thinking processes and rational reconstructions. To help clarify what he means by rational reconstruction, Reichenbach offers the analogy with scientists publishing their results. Actual thinking is like “the thinker’s way of finding his theorem” and the rational reconstruction is like “his way of presenting it before a public.” “Context of discovery” is coined to correspond with the former (actual thinking), and “context of justification” with the latter (rational reconstruction). Reichenbach describes the context of justification as “constructed” and emphasizes that scientists’ public presentation is “only an approximation of what we mean by the context of justification” because, he explains, scientists are generally not philosophers and might make logical mistakes despite their best attempts.[17] Having now associated the context of justification with rational reconstruction, Reichenbach reminds us that "the rational reconstruction of knowledge belongs to the descriptive task of epistemology" and that rational reconstructions are bound to actual thinking (7).

So far the text is consistent with our discussion of Lean DJ. We expected to find the LJ perhaps encompassing both the descriptive and critical tasks, so it is not surprising that Reichenbach's own context of justification includes the descriptive task. However, when we turn to the critical task, our expectations are confounded. Reichenbach's context of justification does not include the critical task! The very next sentence is the start of a new paragraph in which Reichenbach begins a new section: "In addition to its descriptive task, epistemology is concerned with another purpose which may be called its *critical task*" (7). Reichenbach has just shifted gears to the next task, starting a new section. Nowhere in that section (7-12) nor the following one on the advisory task (12-16) does he mention his new terms "context of discovery" and "context of justification."[18] Indeed, he appears not to mention those terms again until three hundred pages later, when he returns to them briefly to make a separate point (381-384).[19]

Given Hoyningen-Huene's Lean DJ, we should find Reichenbach's context of discovery and the context of justification distinguishing between the psychologist's task on one hand, and the descriptive and the critical tasks, on the other. But we do not. Instead, we

---

[17] Note that for this reason "the way a scientist presents his theorem" is offered as an analogy only.

[18] He continues to refer to "rational reconstructions," however, in order to clarify the differences between the critical and descriptive tasks.

[19] This latter text is well worth study. Here Reichenbach defends his meta-methodology, arguing that even if scientists do not consciously use induction, any good scientist is implicitly relying on it.

find Reichenbach using DJ to distinguish between the psychologist's task (concerned with actual thinking) in the context of discovery and the descriptive task (concerned with rational reconstruction) in the context of justification. The critical task is left out of the contexts altogether (see Figure 2).[20]

V. Conclusion

I am not suggesting we should return to Reichenbach's original meaning. Nor do I mean to say that Hoyningen-Huene is wrong; his Lean DJ may prove promising and he may be right that critics of DJ in the 70s and 80s such as Kuhn and Feyerabend would agree to Lean DJ. I *do* suggest, however, that the meaning of the words "context of discovery" and "context of justification" in Lean DJ shifted away from Reichenbach's meaning as he originally presents them in *Experience and Prediction*. For Reichenbach, the context of discovery refers to the actual thought process of a scientist, whereas the context of justification refers to the rational reconstruction of that thought process. Crucially, however, for Reichenbach a rational reconstruction does not necessarily contain valid or logical connections. The context of justification is simply a sophisticated *description* of what is in the context of discovery; it is *not an evaluation* of that content. Instead, the evaluation occurs in the critical task which, I am arguing, occurs outside of the two contexts. This means that for Reichenbach, the context distinction does not, in fact, distinguish between descriptive and normative perspectives. Thus, Hoyningen-Huene's Lean DJ contexts do not align clearly with Reichenbach's original contexts.

Beyond the interpretive issue, this discovery about the shifted meaning should serve as a call for caution. As we renew the debate on the context distinction and cast around for common ground and common definitions from which to frame the debate, we should recognize that even a proposal as seemingly lean as Lean DJ between descriptive and normative perspectives on science can be contentious and far from universally maintained.

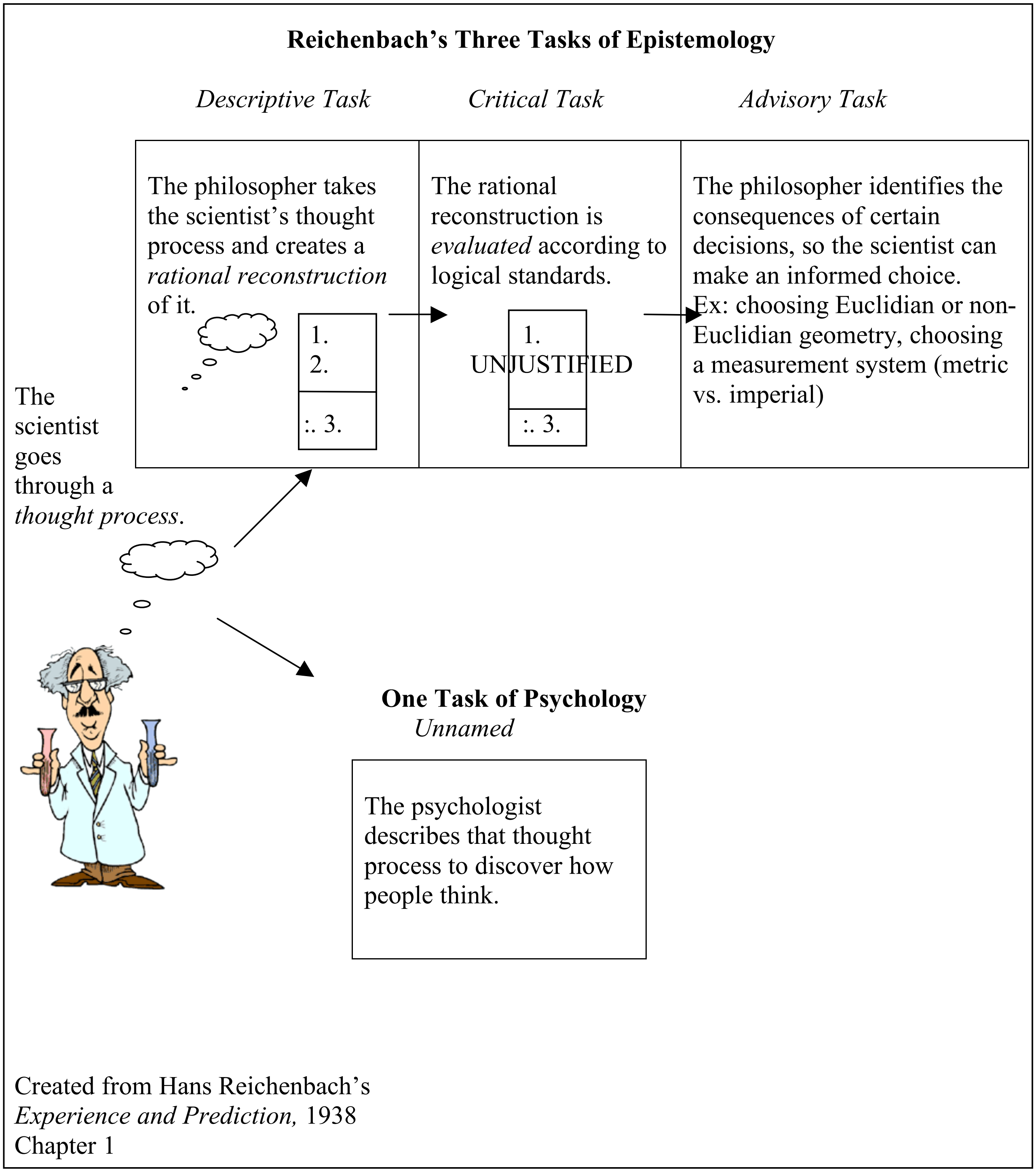

Figure 1 Reichenbach's Three Tasks of Epistemology

Reichenbach's Tasks for Psychology and Epistemology

		Psychologist's Task	**Descriptive Task**	**Critical Task**	**Advisory Task**
Hoyningen-Huene's Lean-Context of Discovery (LD) or Lean-Context of Justification (LJ) ?		Lean-Context of Discovery	Lean-Context of Discovery (or arguably Lean-Context of Justification)	Lean-Context of Justification	n/a
Reichenbach's Context of Discovery or Context of Justification?		Context of Discovery	Context Of Justification	n/a!	n/a
Does Hoyningen-Huene's Lean DJ match Reichenbach's DJ?		Yes, they match	Maybe	**No, they do not match**	n/a

Figure 2 Reichenbach's Distinction and Lean DJ

3. Initial Challenges: Kuhn and Normal Science

I. Introduction

In the 1960s, the context distinction came under attack, leading to lively debates and, ultimately, a stalemate. Some philosophers of science continue to endorse versions of the context distinction, while others have abandoned it completely, considering it to be a relic of failed logical positivism. Within these debates, Thomas Kuhn is often considered to be one of the major figures challenging the distinction (Siegel 1980a p. 304, Kuhn 1962 p. 8-9, Kuhn 1973 p. 327, Bird 2000 p. 70, Nickles 2003 p. 149). Kuhn argues that the *historical development* of scientific claims, not just the claims themselves, can hold philosophically interesting content. Moreover, if a scientist's judgment is dominated by the norms of his or her paradigm, then it is impossible to separate the evaluation of evidence for a theory from the historical situation in which the theory was developed (at least to the extent that the "historical situation" involves specifying the specific paradigm). By many accounts, including Kuhn's own, these two views offer a direct challenge to the context distinction, which is intended to distinguish "the social and psychological facts surrounding the discovery of a scientific hypothesis from the evidential considerations relevant to its justification" (Salmon 1970 p. 68). In this chapter, I argue that Kuhn's view does not actually challenge the context distinction as much as it initially appears, although his view does challenge some versions of the distinction. In particular, Kuhn's view violates what I call the *Historical context* Distinction and the *Values* Distinction, but not what I call the

Psychological Distinction, and arguably not the *Is/Ought* Distinction. In addition, Kuhn's view does challenge underlying assumptions about observation that often accompany use of the context distinction and serve to make these various distinctions appear interchangeable.

When Kuhn first wrote *The Structure of Scientific Revolutions* (*SSR*), his goal was not to attack the context distinction. Although he knew he was fighting standard philosophical theories in general,[21] by his own account he did not consciously recognize that his theory of paradigms counters the distinction in particular. Rather, this issue was pointed out to him by Stanley Cavell,[22] who read the manuscript, and later, by Alexandre Koyré,[23] who read the first edition of *SSR*. Neither of them saw it as a problem, but rather as a refreshing revisit to a longstanding logical empiricist assumption. Others, however, did not receive Kuhn's "revisit" with such joy, but rather with puzzlement or even dismay.

> Kuhn's … critique of the context distinction has caused some to shrug in puzzlement. Herbert Feigl, for example, was surprised that such brilliant and knowledgeable scholars as N.R. Hanson, Thomas Kuhn, Michel Polyani… and others hold the distinction of being invalid or at least misleading. (Hoyningen-Huene 1993, p. 248)

Feigl even maintained that Kuhn is confused about what the distinction means, or is almost deliberately refusing to understand.

> I confess I am dismayed by the amount of – it seems deliberate – misunderstanding and opposition to which this distinction has been subjected in recent years. (Feigl 1970a, p. 4)

---

[21] Kuhn was heartened to discover a fellow outsider in Ludwik Fleck (1939). He states in an interview, "I don't think I *learned* much from reading the book…. But I certainly got a lot of important reinforcement… It made me feel, all right, I'm not the only one who's seeing things this way" (Kuhn 2000, p. 283).

[22] Kuhn's explicit references to the distinction in the introduction to *SSR* (see below) were added at Cavell's prompting. See (Hoyningen-Huene 2006, p. 124) in which Hoyningen-Huene offers a careful study of Kuhn's manuscript and retracts assumptions he had made in (1993, p. 245). See also (Kuhn 2000, p. 285).

[23] "You have brought the internal and external histories of science, which in the past have been very far apart, together" (Koyré as recounted by Kuhn 2000, p. 286).

How can such a simple distinction lead to such confusion and disagreement?[24] Others have noted the ambiguities in the distinction, including Hoyningen-Huene (2006) and (1987), Kordig (1978), and Arabatzis (2006); what I want to address here is *why* there are so many different meanings. In some philosophical debates, when the ambiguities and cross-talk are cleared away, one discovers that everyone was more or less in agreement, simply arguing over terminology. Hoyningen-Huene has argued that the same is true for the context distinction: underneath the disagreements, all parties share a commitment to a descriptive/normative distinction (Hoyningen-Huene 2006). While in the end, this level of agreement might indeed be possible, in this chapter I highlight many intractable disagreements along the way. Once we begin to clarify seemingly simple terminology, in some areas we reveal deep divisions. One of my central aims in this project as a whole is to show how the distinction between the context of discovery and context of justification is, in fact, tightly bound to some other fundamental debates in philosophy. For instance, in a later chapter I show how one disagreement involving the context distinction is actually based in debates on the nature of justification. In this chapter, I show how the context distinction is also tied to debates about observation and *Weltanschauung*. So it should be of little surprise that the arguments turn out to be difficult to resolve.

Kuhn's view appears to have violated the context distinction, but whether it actually does depends on which version of the context distinction you are considering. As discussed in the Introduction, to help navigate the many ambiguities, some have offered alternative

[24] Scheffler has also written that Kuhn's replies to criticisms "seem to me so inadequate as to suggest that he, and therefore others as well, may have failed to grasp their full import" (Scheffler 1972, p. 366).

distinctions. For instance, Kordig suggests divisions between initial thinking, plausibility, and acceptability (Kordig 1978), while McLaughlin suggests replacing "discovery" and "justification" with "invention" and "appraisal" (McLaughlin 1982). In this chapter I take an approach closer to McMullin's; rather than focusing on the distinction itself, he turns his attention to philosophy and creates a taxonomy of the aims of analytic philosophy of science (McMullin 1970). Here I offer something similar. A second goal in this project is to demonstrate that one cannot simply reject or accept the context distinction without considering what it is being used for in any given debate. To that end, I distinguish between various aims and questions of philosophers of science generally, and of Kuhn in particular. I then show how debates about the context distinction have fit in with those questions, and how Kuhn might be understood to fit into those projects. Once these further distinctions have been made, it becomes easier to see why there are these ambiguities, and what Kuhn's stance on the context distinction must be.

II. Three levels or questions of philosophy of science

It can be useful to consider the different levels at which philosophy approaches the sciences. For instance, some have noted that at times philosophy of science operates on a *meta*-level (Feigl 1970b p. 7, McMullin 1970). When the object of study is science and scientists, *meta*-questions include: What is rationality? What is the nature of justification? What are proper methods for evaluating evidence? In addition, there is a *meta-meta*-level: philosophy of science as a discipline is also engaged in examining *itself* (this project is an example of such an examination). At the *meta-meta* level, philosophers search for the proper method for answering the earlier questions (i.e., through *a priori* introspection, or empirical research, or some combination).

Thus we can distinguish at least three empirical questions that have concerned philosophy of science (this list is not meant to be exhaustive):

a. At the object-level (science):
   For a given scientific claim, H, is it justified?

b. At the meta-level (phil. of science, reflecting on scientific methods):
   In general, by what criteria should we determine whether a scientific claim is justified?

c. At the meta-meta-level (phil. of science reflects on its own methods):
   By what criteria should we choose criteria?

The answers to question *a* are determined by *b;* to determine whether a given hypothesis is justified, we must first decide what general criteria we should use to answer such questions. Likewise, *b* is determined in part by *c;* to choose general criteria for analyzing hypotheses, we must first decide how to choose these criteria: whether they are derivable *a priori*, or whether we should distill them from actual practice, or some combination thereof.

I argue that the context distinction has been operating at all three levels in debates in philosophy of science, which could explain some of the confusion. Given certain logical positivist assumptions that justification is timeless, objective, and purely logical, the answers to these questions are tied together in such a way that the ambiguity is unnoticeable, and indeed unimportant. However, it is widely noted that Kuhn, along with others including Hanson, Feyerabend, and Toulmin, challenged those assumptions. I maintain that this underlying challenge was often misinterpreted as a challenge to the context distinction itself.[25] I will survey the levels in reverse order, clarify how the context distinction manifests for each level, and then locate Kuhn in relation to it (see Figure 4).

III.

c. At the meta-meta-level (philosophy of science reflects on its own methods). By what criteria should we choose criteria?

At the meta-level, which deals with *Question b*, the philosopher's aim is to find a methodology that details how science should be conducted. (Prominent candidates for methodology have included Verificationism, Falsificationism, Kuhn's paradigms, and Lakatos's Research Programmes). In contrast, at the meta-meta-level, which deals with *Question c*, the aim is to determine which tools or standards philosophers should use to evaluate those methodologies. In particular, should a methodology be based on history, or should it be justified by ahistorical standards (such as logical standards)?

The view that the methodology should be justified by history has been characterized as a violation of the context distinction. Even McMullin, who challenges some uses of the context distinction, claims that history is irrelevant to evaluating the validity of scientific claims. He writes,

> A deductive inference rests in no way for its validity upon experience or history. It is valid not because it has been followed innumerable times with success, but because it has the total transparency that makes reference to history unnecessary in its support. (McMullin 1970 p. 59)

If the aim of the methodology is to evaluate individual claims,[26] then many conclude that the proper way to do this is using logic, not history. As Salmon writes,

---

[25] My argument is a different take on a similar point made by Hoyningen-Huene (1987 and 2006). I will discuss his other argument below.

> The philosopher of science, consequently, finds himself attempting to cope with problems on which the historical data may provide enormously useful guidance, but the solutions, if they are possible at all, must be logical, not historical, in character. The reason, ultimately, is that justification is a normative concept, while history provides only the facts. (Salmon 1970, p. 74)

Thus, the argument implies that philosophers may use history of science as illustration or inspiration, but they may not use it as evidence or data for what a methodology should be. That is because using it as evidence relies on an assumption, namely that it would be acceptable to justify a specified scientific behavior on the basis that 'scientists have always behaved in that way'; in fact, the argument goes, this assumption is false and any justification should be logical. That is, one would being committing the Is/Ought fallacy (the fallacious claim that the way things *are* dictates the way they *ought to be*). I call this version of the context distinction the Is/Ought Distinction; the "is" (context of discovery) refers to historical accounts of scientific activity and is distinguished from the "ought" (context of justification), which refers to the way philosophers should determine scientific methods.

Reichenbach employs the Is/Ought version of the context distinction to argue that one cannot object to his methodology by pointing to scientists who do not actually use his methodology. If they are not using his methodology, he argues, then they are not really doing science; and, conversely, if they are really doing science, then they are using it after all (perhaps without realizing it) (Reichenbach 1938 p. 382-3). As Scheffler writes, "We acquire with this [context] distinction a strategy by which the historian's alleged counterexamples can be dealt with: they are, if possible, to be relegated to the domain of discovery and consigned thenceforth to psychology for further study" and thereafter set aside

---

[26] This assumption is part of a particular approach to philosophy of science that has been challenged. See section VI of this chapter for more discussion of this issue.

by philosophers (Scheffler 1967 p. 73). Thus we see that discussions involving the Is/Ought Distinction quickly move from making a simple distinction to making the claim that philosophers of science should not use history of science as evidence. I call this latter claim the Historical Evidence Distinction; according to this version of the context distinction, historical examples of science cannot serve as evidence for nor against any given methodology.[27]

Thus, according to this view, one could uphold the Is/Ought version of the context distinction by turning to something other than history to provide the justification for one's methodology. Yet Kuhn famously claims that history should be "viewed as a repository for more than anecdote or chronology" and that philosophers of science should use history of science as "a source of problems and data" (Kuhn 1962 p. 1 and Kuhn 1968 p. 13).[28] Kuhn's view has been seen as a challenge to this whole approach to developing methodologies in philosophy of science. At the beginning of SSR, Kuhn writes,

> Undoubtedly, some readers will already have wondered whether historical study can possibly effect the sort of conceptual transformation aimed at here.
> History, we too often say, is a purely descriptive discipline…yet at least a few of my conclusions belong traditionally to logic or epistemology…. I may even seem to have violated the very influential contemporary distinction between 'the context of discovery' and 'the context of justification.' (Kuhn 1962 p. 8)

When Kuhn claims that his idea of paradigm shifts came from doing history of science, he is not simply claiming that he was inspired by history, but also that this history serves as data

[27] For more defenses against this charge beyond what I address here, and other arguments for using history as evidence in philosophy of science, see (Giere 1973), (McMullin 1974), (Burian 1977), and (Nickles 1985).
[28] For an account of what Kuhn means by "history," see Hoyningen-Huene (1992 and 1993).

for his idea.[29] If, as some interpret him, Kuhn is relying on historical accounts of how science has actually been practiced in order to give a normative account of how science should be practiced, then he seems very much to be violating the meta-meta-level version of the context distinction, what we have called the Is/Ought Distinction. Critics often seem to charge Kuhn with advocating the view that *how something has been done in the past is a reason for continuing to do it in the future*. Does Kuhn really reject this Is/Ought fallacy, which at times he appears to (see Kuhn 1962 p. 207-208)? If not (and I maintain that he need not), then the puzzle becomes: how can Kuhn recommend using history as evidence (and not just illustration) without violating the Is/Ought fallacy? How can he violate the Historical Evidence Distinction without violating the Is/Ought version of the distinction?

Several readers of Kuhn have offered criticisms that could be seen as answers to this question. One possible answer has been that Kuhn, whether he likes it or not, is simply offering a descriptive account of science, not a normative methodology, and so is not violating the Historical Evidence Distinction. As Feyerabend writes,

> Whenever I read Kuhn, I am troubled by the following question: are we here presented with *methodological prescriptions* which tell the scientist how to proceed; or are we given a *description*, void of any evaluative element, of those activities which are generally called 'scientific'? Kuhn's writings, it seems to me, do not lead

[29] That Kuhn uses history of science as data also explains an otherwise puzzling exchange between him and Siegel (1980b p. 369-370). Kuhn articulates the difference between context of pedagogy and context of discovery in order to clarify that philosophers should use context of discovery (i.e., more nuanced historiographies) rather than the context of pedagogy (cartoon over-simplifications and false versions of events) from which to base their normative methodologies of science (Kuhn 1973, p. 327-8). Although he is not particularly clear on this, Kuhn seems to be suggesting that if philosophers base their normative methodologies on cartoon histories such as those seen in textbooks, the methodologies will likewise be cartoonish and unrealistic, i.e., completely false. To the extent that these pedagogical examples are rational reconstructions, then, they are misleading and inappropriate rational reconstructions, rather than helpful ones.

> to a straightforward answer. They are *ambiguous* in the sense that they are compatible with, and lend support to, both interpretations. (Feyerabend 1969 p. 198)

That is, Kuhn seems to be giving a provocative account of how scientists have or do work, but Kuhn has failed to provide a rubric that explains how science should be done.[30]

Another answer has been that Kuhn is truly offering a normative methodology, but in doing so has violated the Is/Ought fallacy after all. Often Kuhn seems to admit to this interpretation himself. For instance, when Lakatos criticizes Kuhn's approach on the grounds that "Kuhn's conceptual framework … is socio-psychological: mine is normative" (Lakatos 1964 p. 177), Kuhn's defense is that Lakatos's framework is also socio-psychological. "If I differ from Lakatos (or Sir Karl, Feyerabend, Toulmin, or Watkins), it is with respect to substance rather than method" (1964 p. 126). Kuhn rejects "the perceived differences in our methods: logic versus history and social psychology; normative versus descriptive" (Kuhn 1969c p. 125), suggesting that at the meta-meta-level, his view is consistent with those of his critics. This is hardly reassuring to Kuhn's critics, since he argues that they are also using history as data and so are also blurring the distinction between normative and descriptive. Kuhn writes,

> All of us, unlike the members of what has until recently been the main movement in philosophy of science, do historical research and rely both on it and on observation of contemporary scientists in developing our viewpoints. In those viewpoints, furthermore, the descriptive and the normative are inextricably mixed. (Kuhn 1969c p. 125)

In this passage, Kuhn concedes that his view is inconsistent with the context distinction. In the post-script to SSR, he reiterates this view.

---

[30] One of the most common criticisms of Kuhn is that, as a descriptive account, Kuhn's view fails: science does not, in fact, proceed through a serious of incommensurable paradigms punctuated by paradigm shifts. (See Watkins 1965, Feyerabend 1969). I will set this objection aside.

> A few readers … have noticed that I repeatedly pass back and forth between the descriptive and the normative modes, [when I open a passage with], 'But that is not what scientists do,' and close by claiming that scientists ought not do so. (Kuhn 1969a p. 208)

Kuhn reiterates that his theory of methodology is indeed intended to be normative, and, like other methodologies, it is supposed to provide a basis for how scientists ought to behave, while at the same time he admits to drawing upon historical examples as evidence for his theory. Kuhn continues with the conclusion that his view might violate the "time-honored philosophical theorem" that " 'Is' cannot imply 'ought,'" but that this theorem is probably false (Kuhn 1969a p209).

However, I believe that Kuhn is giving in to his critics much too quickly here. In later years, Kuhn returns to the question of using history as data for a normative theory, and concludes that this was a weakness he never needed to have admitted to.

> My generation of philosopher/historians saw ourselves as *building a philosophy on observations of actual scientific behavior. Looking back now, I think that that image of what we were up to is misleading.* Given what I shall call the historical perspective*, one can reach many of the central conclusions we drew with scarcely a glance at the historical record itself.* The questions which led us to examine the historical record were products of a philosophical tradition that took science as a static body of knowledge and asked what rational warrant there was for taking one or another of its component beliefs to be true. Only gradually, as a by-product of our study of historical "facts," did we learn to replace that static image with a dynamic one, an image that made science an ever-developing enterprise or practice. And it is taking longer still to realize that, with that perspective achieved, *many of the central conclusions we drew from the historical record can be derived instead from first principles*. (Kuhn 1991 p. 111-112, emphasis added)

Kuhn claims that his earlier reliance on history of science was unnecessary, and that he could have derived his methodology from first principles, without relying on history as evidence.

Yet what are these first principles? Can both first principles and historical evidence provide independent evidence for a methodology? In particular, how can one draw from history without claiming that science should be practiced in certain way simply because it has, until now, been practiced in that way? Here I will explore a way in which Kuhn can be seen as offering evidence in support of a normative methodology of science, where the evidence comes at least partially from the history of science, and yet he would not be committing the Is/Ought fallacy, and so would not be violating the context distinction at the meta-meta-level.

The key is to show that Kuhn is not invoking history in virtue of its being history, but rather for some other reason. That is, I suggest that one reason for accepting history as evidence is not simply because *what scientists have done* is *what scientists should do*[31], but rather that what scientists have done *indicates or stands-in for some other reason*, and invoking that other reason does not violate the context distinction. Just as Socrates asks Euthyphro, "Is something pious because the gods love it, or do the gods love it because it is pious?", an analogous question could be asked here: "Is something the proper scientific methodology because scientists do it, or do scientists do it because it is the proper methodology?" To argue for the former, *something is a proper methodology because scientists do it,* is to directly violate the is/ought version of the context distinction. However, if one argues for the latter, that *scientists do something because it is the proper methodology*, one can remain consistent with this version of the context distinction. Notably, if the latter is true, and one has independent good reason to believe that scientists have somehow identified

[31] I am using history as roughly interchangeable with "what scientists do." But of course there are many issues here, including: Which scientists? Should we be looking at the activities of all scientists? Should we be looking at only the best scientists, in which case how do we determine which ones they are? There is even a demarcation problem lurking here – who counts as a scientist, verses a bad or pseudo scientists, craftsman, historian, or something else. I set these important questions aside, since they do not seem to make a difference to the current discussion.

and used the proper methodology, then it would acceptable to use their actions as evidence for what a proper methodology is. Thus, one could use scientists' actions as evidence without violating this version of the context distinction.

To see if this Euthyphro strategy is consistent with Kuhn, then, one would need to identify what good reason he has to think that scientists have indeed found the proper methodology. An excellent contender for this reason would be an argument from success like the one employed for arguments in favor of realism: scientists' theories are successful at predicting events and building reliable technologies, so whatever methods they are using must be good ones. At times, Kuhn does seem to have a success of science argument in the back of his mind. He often refers to science as very successful and claims that is why he studies it. Consider, for instance, his admission that,

> The question that more than any other has guided and motivated me is … why the special nature of group practice in the sciences has been so strikingly successful in resolving the problems scientists choose. What is it about what scientists do, I have been asking, that makes their output knowledge? (Kuhn 1983, p. 28)

In many ways, however, the argument from success does not seem available to Kuhn, since he draws extensively from episodes in history that are not successful by today's standards, most notably Aristotle's physics.

Lakatos at first seems to offer a reason that would allow philosophers to use history of science as evidence: namely, that doing so leads to a more fruitful philosophical methodology (Lakatos 1969). However, this reason is also not available to Kuhn, since ultimately Lakatos has a different notion of "history", which Kuhn rejects. Lakatos maintains that the data philosophers should use are rational reconstructions of episodes in science, not the descriptions of those episodes themselves.

Lakatos and Kuhn share a different argument, however, for why one should use history. Occasionally Kuhn presents what I am calling an Inference the Best Explanation argument for using history. Kuhn's methodological theory is correct, he argues, because it would explain otherwise irrational behavior:

> I began as an historian of science, examining closely the facts of scientific life. Having discovered in the process that much scientific behavior, including that of the very greatest scientists, persistently violated accepted methodological canons, I had to ask why those failures to conform did not seem at all to inhibit the success of the enterprise. When I later discovered that an altered view of the nature of science transformed what had previously seemed aberrant behaviour into an essential part of an explanation for science's success, the discovery was a source of confidence in that new explanation.
> (Kuhn 1965, p. 236)

Kuhn claims his view can make sense of otherwise irrational-looking behavior. The implication is that the best explanation is that scientists are indeed behaving according to the methodology he prescribes. In later interviews, Kuhn explains how he was surprised that Aristotle continually made bizarre and empirically inaccurate claims about nature (Kuhn 2000). Aristotle was clearly a brilliant thinker, so Kuhn searched around for an explanation for his odd scientific behavior. Under the methodology that Kuhn developed, Aristotle's behavior suddenly made perfect sense: Aristotle is operating under a paradigm that is incommensurable with our own. So history served as both 'innocent' inspiration for Kuhn's methodology and as 'pernicious' evidence. This Inference to the Best Explanation argument did not seem to strike Kuhn as itself a violation of the Is/Ought Distinction. That is, he claims that he does not rely on historical cases *simply because* they happened:

> My criterion for emphasizing any particular aspect of scientific behavior is therefore not simply that it occurs, nor merely that it occurs frequently, but rather that it fits a theory of scientific knowledge. (Kuhn 1965, p. 236-7)

Kuhn does consider Aristotle's activities to be evidence for his own methodology. However, the reason they are evidence is that they fit with Kuhn's methodology, not because 'whatever Aristotle did is right.'[32] Kuhn acknowledges this argument as (innocently) circular – the data supports the theory because the theory supports the data (Kuhn 1965, p. 237, Kuhn 1969a p. 208).

Yet, although he claims that his reason for using an example from history is not "simply that it occurs," it is not entirely clear that he is right about this. Yes, Kuhn's methodology can explain scientists' behavior in a way that makes it rational. However, this alone still commits the Is/Ought fallacy, since it assumes that their behavior is rational in the first place. The whole reason for invoking an Is/Ought Distinction here is to avoid this kind of circular argument ('this methodology is right because scientists use it, and they use it because it is right'). Although Kuhn claims this circularity is not a problem, it does open him back up to the claim that he violating this version of the context distinction, and so at the very least it is a problem for avoiding the Is/Ought Distinction.

This is precisely the move that he tries to take back in 1991, when he suggests that he could have derived his methodology without reference to history at all. What could possibly be an argument for such a claim? Although Kuhn does not specify, I suggest that a plausible candidate here is a version of "ought implies can." That is, scientists do and ought to operate within paradigms because that is the only way they can. There are two reasons to associate this idea with Kuhn. The first is that he adopts Hanson's view that all observation is theory-laden (Hanson 1958). The second is that he insists that pre-paradigm science is simply

[32] To properly evaluate this argument, one would need to consider the objection mentioned earlier in footnote 7 that Kuhn's descriptions of history are inaccurate. Again, I set this objection aside, since it does not directly bear on the question of the context distinction.

fruitless activity. Together, these two claims suggest that there is no productive way to do science outside of a paradigm.

For instance, Hanson argues that our sensations of the world become information only after we place them in the context of world-views. We do not directly see the sun, for example, but rather see patches of light that we learn to associate with the concept "sun." If all observation is steeped within a theory, then there can be no observation or "raw material" for scientific arguments outside of a theory.

Kuhn's examples of people working outside of paradigms back up this view. Kuhn is not claiming that no investigation of the natural world can be done outside of a paradigm, but rather that any such investigation is haphazard, piecemeal, unable to make predictions or identify underlying laws, and unworthy of the name "science." This is not an issue of human psychology, for Kuhn, but rather one of logic. If one accepts that all useful knowledge is necessarily mediated through a *Weltanschauung*, which for Kuhn is a paradigm, then it is logically incoherent to ask one to perceive the world from no perspective at all. It is akin to asking what you see when you look without your eyes. Paradigms are our conceptual eyes; they are that which allow us to conceive of anything at all.

If we see the debate about the context distinction at the meta-meta-level as a debate about theory-laden observation and *Weltanschauung*, then part of our puzzle has been solved. Kuhn's paradigm concept is one of the many ideas that make use of the *Weltanschauung* concept. Other such ideas include Lakatos's Research Programmes and the notion of theory-laden observation in general. It now becomes more evident why "such a simple distinction" has caused such extended debate. The issue at this level is not whether "how an idea occurs to a man" is relevant to "whether that idea is justified." Instead, the issue is whether it is possible to make meaningful observations and to evaluate scientific claims outside of a given historically situated paradigm (or *Weltanshauung*).[33] I will return to this point in Section V.

In this section we have seen how the context distinction has been used at the meta-meta-level to answer the question: How should we develop a philosophy of science? The answer is that one should not violate the Is/Ought Distinction. This has been taken to imply that one should not use history of science as evidence for one's methodology in philosophy of science (the Historical Evidence Distinction). Both the Is/Ought Distinction and the Historical Evidence Distinction are often used interchangeably, but I argue that one could plausibly accept one while rejecting the other. I have shown that Kuhn's view could be consistent with the Is/Ought Distinction while violating the less pernicious Historical Evidence Distinction. Unfortunately, although Kuhn's view is *consistent* with the Is/Ought Distinction, it remains unclear whether he in fact *accepted* this Is/Ought Distinction, both in the abstract and when providing evidence for his methodology. At times he seemed to reject this distinction, while other times he seemed to accept it. To reject it, Kuhn would need to have some other reason for relying on history as evidence. Ultimately, I argue that Kuhn's other reason could be a variation of 'ought implies can.' That is, 'this is what scientists do because there is no other way to do science.' I argue that this option is open to him because of his well-known stance on controversial views about how humans perceive and process the world, namely through *Weltanschauungen* and theory-laden observation. I have suggested that these different views about observation undermine the connection between the Is/Ought

[33] This might clarify Kuhn's debates with people such as Popper and Feigl. However, it is worth noting that this recognition won't help in debate with Lakatos, since both Lakatos and Kuhn share this assumption that being part of a *Weltanschauung* is essential for scientific activity.

and Historical Evidence versions of the context distinction, so if someone were to disagree with Kuhn about observation, it makes more sense why they would consider him to be confused about the context distinction. Below I show how these controversial views about observation are entangled with the context distinction for Kuhn at the meta-level, as well.

IV.

b. At the meta-level (philosophy of science, reflecting on scientific methods).

In general, by what criteria should we determine whether a scientific claim is justified?

Is there a proper method for evaluating scientific claims, and if so, what is it? For much of the twentieth century, this was a central question in philosophy of science. Though the focus of philosophy of science has since shifted, with some philosophers questioning whether there even is such a thing as one single scientific method,[34] this question of proper method raised much debate among philosophers. Famous answers included confirmation, falsification, induction, hypothetical-deductive methods, etc. For example, a naïve inductivist view would suggest that the proper way to do science is to make several observations of related events and look for a pattern; these patterns will suggest a scientific claim (the label of this approach indicates its popularity). If we accept Popper's Falsificationism, on the other hand, our answer would be that when science is done well it consists of proposing bold conjectures and trying to falsify them. It is important to clarify that the question at this meta-level is a normative question: what is the *proper* method for evaluating scientific claims, not the actual method employed by scientists? This is where the context distinction becomes relevant.

---

[34] (Fine 1998, Feyerabend 1969 and 1977). I return to this point later.

Connection with the Context Distinction:

"Factors **related to the discovery of a claim are irrelevant to its justification"**

Factor 1: the work leading up to the justification of a claim

In one version of the distinction, invoking the distinction between the *context of discovery* and the *context of justification* amounts to the claim that ***the factors relating to the genesis of an idea are not relevant to the idea's justification.*** Many factors have been considered here, including the work of the individual scientist who developed the idea, the historical context of the idea, and any cultural, political, or social values of the scientists. This version of the context distinction has been invoked to illustrate how to ensure that one's methodology is indeed normative and not simply descriptive (Reichenbach 1938 p. 382, Scheffler p. 1967 p. 73). Although there are slightly different views on this, I will continue with Reichenbach's view.

As I showed in the previous chapter, when Reichenbach invokes the context distinction at the beginning of *Experience and Prediction*, he focuses on one factor: namely the work of an individual scientist. He states that ***the actual thought process of the individual scientist*** **is irrelevant to the justification of that scientist's ideas,** and argues that this distinction is crucial when developing a methodology for science. I will call this the Psychological Distinction. Reichenbach uses the Psychological Distinction to clarify that he does not aim to determine the method by which science is actually practiced.

One might think, then, that his goal is to determine how science *should be* practiced. But this isn't quite right either. 'Science as it should be practiced' is still part of the context of discovery for Reichenbach. Instead, the goal is to determine the *rational reconstruction of* 'science as it should be practiced', and that rational reconstruction is the context of justification. One's methodology should fit this rational reconstruction. The claim is that the actual practice of science, even when it is being done properly, cannot or need not be performed according to the proper methodology. It is only necessary that the work can later be rationally reconstructed to conform to the proper methodology. So the philosopher's logical analysis should be applied to a rational reconstruction or logical idealization of that actual thought process. As I argue in the previous chapter, for Reichenbach this idealization of proper science is the context of justification. The question for the philosopher is now: what method characterizes the logic in this rational reconstruction?

For example, consider Popper's methodology of falsificationism with regards to Einstein's Theory of General Relativity (Popper 1963 p. 34-36). If this theory is correct, then light from distant stars should bend around the sun, and this displacement will be observable during an eclipse. This is a bold conjecture. During the 1919 eclipse, Eddington observed light from stars bending to the degree predicted, so this conjecture is not (yet) falsified. This is a rational reconstruction of the Eddingtion Expedition, and it need not reflect Eddington's actual thinking and testing processes. Instead it is an idealized account of these processes that I have constructed to highlight how it conforms to the Falsificationist methodology. Although the Eddington Expedition is considered by many (though not all[35]) to be an excellent example of how science should be done, nonetheless, the goal of the methodology is not to capture how Eddington actually practiced science, but rather to capture a logical abstraction of that practice. Similarly, with regards to the results of the Michelson-Morley experiment, Feigl notes that "in a logical reconstruction of the special theory of relativity,

[35] See (Waller 2002).

those results play the role of confirming evidence, or (if with Karl Popper we wish to put it the other way around) of disconfirming the ether hypothesis" regardless of whether Einstein was in fact aware of these results when constructing his theory (Feigl 1970b p. 4).

One more clarification is in order that reflects a disagreement among some proponents of the context distinction. We have seen how Reichenbach uses the context distinction to clarify that the methodology should not reflect actual scientific practice, but rather a rational reconstruction. But a rational reconstruction of what, exactly? In Chapter 2 I argue that Reichenbach takes it to be a rational reconstruction of actual scientific practice. Yet Lakatos, most famously, declares that rational reconstructions can be of fictional scientific practice; they need not be constructed from actual science, no matter how exemplary (Lakatos 1969).

Kuhn explicitly disagrees with Lakatos's use of these sorts of rational reconstructions, arguing that philosophers of science should not rely on fictional episodes of science, either as evidence or as illustration. When replying to Lakatos's version of the history of the Bohr atom, Kuhn writes,

> My version [of events], like his or like any other bit of historical narrative, will be a rational reconstruction. But [unlike Lakatos] I shall not ask my readers to apply 'tons of salt' nor add footnotes pointing out that what is said in my text is false. … [Lakatos's 'case histories'] illustrate the differences between the way philosophers and historians usually do history…. A historian would not *include in his narrative* a factual report which he *knew* to be false. If he had done so, he would be so sensitive to the offense that he could not conceivably compose a footnote calling attention to it. (Kuhn 1969c p. 151 f. 32)

In this section, therefore, I focus on Reichenbach's Psychological interpretation of the context distinction as applied to Question b, rather than Lakatos's, in order to demonstrate that Kuhn's view is consistent with it: One's normative methodology of science should be consistent with the context of justification (the *rational reconstruction* of actual, not fictional, scientific practices), not context of discovery (a *description* of actual thoughts of scientists, with all their meandering false starts, creativity, and faulty reasoning).[36]

Kuhn's Paradigms and Four Stages of Science

Before showing how Kuhn's proposed methodology is consistent with this use of the context distinction, let's highlight the aspects of Kuhn's view that are relevant to the discussion. Kuhn famously proposes that science operates in cycles made up of four stages. The first stage is pre-normal science, in which each practitioner is pursuing independent lines of research, with no common assumptions. Although periods of pre-normal science are successful at generating esoteric and unrelated claims about the world, they are unsuccessful in producing coherent, productive claims such as laws or statements of underlying cause. Not until the second stage, that of normal science, does true advancement of knowledge occur.

Normal science is characterized by two things. In early writings, Kuhn confusingly refers to them both as paradigms (Kuhn 1962), but in later writing he distinguishes them by the labels 'disciplinary matrices' and 'exemplars' (Kuhn 1969a, Kuhn 1974 p. 297 and p. 306, see also Masterman 1966). A disciplinary matrix consists of the features that define a particular scientific community. It is made up of the norms and assumptions that define a community, set its goals, and establish proper methods for reaching those goals. Disciplinary

[36] Kuhn's view actually splits on this issue, depending on whether one is discussing periods of normal science within a given paradigm, or periods of revolution/choice between two paradigms. In this chapter, I focus on the former.

matrices are often referred to as "world views," "conventions," or "frameworks," though the terms fit only loosely.

The primary tool of a disciplinary matrix is an exemplar. Exemplars provide students of a given disciplinary matrix a model to follow. They are prime examples that illustrate the type of problem that is acceptable to work on and how one goes about finding a solution. Within a Newtonian disciplinary matrix, for example, one exemplar used is the calculation of trajectory of a ball thrown into the air. This example illustrates a goal of this community (predict the movement of massive objects), and how to achieve it (using a force diagram).

During a period of normal science, scientists primarily engage in puzzle solving activities. That is, they work within a given disciplinary matrix to identify and pursue puzzles that can by solved by applying the exemplars and following the method they illustrate. Kuhn rejects the idea that communities follow a series of rules, arguing instead that the exemplars allow scientists to model and mimic behavior with a flexibility and scope that rules cannot allow (Kuhn 1974 p. 306-319).[37] After a while, puzzles start to pile up that defy solution. These eventually get recognized as anomalies, which kicks off the third stage of science: crisis. Crisis refers to a break down of the disciplinary matrix as assumptions are challenged and new assumptions and exemplars are presented. Eventually the fourth stage is reached, revolution, in which the old disciplinary matrix is rejected, and an entirely new one is accepted instead (this is often referred to as a "paradigm shift"). For our current purposes, I will focus on what occurs during normal science. I will return to periods of crisis and revolution in a later chapter.

[37] For a nice illustration of the success of exemplars over rules, see John Seely Brown's account of using "stories" instead of rulebooks to train Xerox repair technicians (Brown 2002).

Kuhn's Normal Science as Activities Focused on the Context of Justification

In a nutshell, then, Kuhn claims that the proper method for science is for scientists to engage in puzzle-solving activities within a disciplinary matrix (Kuhn 1962 p. 35-42). If the question at the meta-level is "In general, by what criteria should we determine whether a scientific claim is justified?" then Kuhn's answer is that a scientific claim should be accepted by the community when it succeeds in solving a puzzle, using the exemplar as a model and following the norms and assumptions of the community's disciplinary matrix.[38]

We can see that in many ways this is quite different from other proposals, such as Falsificationism. Falsificationism invokes a logical analysis of the claim and the evidence against it, whereas Kuhn invokes a community of people with norms and values. This difference itself has caused many people to charge Kuhn with violating the context distinction, claiming that social factors belong in the context of discovery while logic belongs in the context of justification (Popper, Lakatos).

Yet Kuhn distinguishes between the social influences of one *individual* from those of a *community*, agreeing that individual psychology is irrelevant to justification:

> To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has endless fascination. What one must understand, however, is the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive. (Kuhn 1969a p. 200)

[38] Kuhn actually separates this question into two: *what justifies accepting a new belief within a given paradigm?* And *what justifies changes in existing beliefs, i.e., paradigm shifts?* (Kuhn 1991 p. 112). I deal with the question of changes in belief and paradigm shifts in the next chapter.

By distinguishing between individual and group psychology, Kuhn allows himself to align with the Psychological version of the context distinction. Later Kuhn connects his view explicitly with the context distinction:

> Again and again [Sir Karl] has rejected “the psychology of knowledge” or the “subjective” and insisted that his concern was instead with the “objective” or the “logic of knowledge” … Until very recently I have supposed that this view of the problem must bar the sort of solution I have advocated. But now I am less certain… When he rejects ‘the psychology of knowledge,’ Sir Karl’s explicit concern is only to deny the methodological relevance to an *individual’s* source of inspiration or of an individual’s sense of certainty. With that much I cannot disagree. It is, however, a long step from the rejection of the psychological idiosyncrasies of an individual to the rejection of the common elements induced by nature and training in the psychological make-up of the licensed membership of a *scientific group*. One need not be dismissed with the other. (Kuhn 1970 p. 291)

Here we see Kuhn explicitly accept the specific version of the context distinction we started with in this section (*the actual thought process of the individual scientist is irrelevant to the justification of that scientist’s ideas*). At the same time, however, we see him reject a different version of the context distinction, one based on the psychology of groups. We will return to that second version in the next chapter. First, however, I want to offer more support for the claim that Kuhn accepts this version of the context distinction. As biographers of Kuhn note,[39] (and as we can see in this passage) Kuhn’s expressions of his view change over time. Yet, I maintain that his key ideas remain more or less the same.

To do this, let us return to the Psychological Distinction that we reached at the beginning of this section: one’s normative methodology of science should be consistent with the context of justification (the rational reconstruction of the best scientific practices), not

[39] “Kuhn’s own understanding of how best to characterize these episodes of [change in science] itself underwent a number of significant shifts” (Conant and Haugeland 2000 p. 1).

with the context of discovery (the actual scientific practices, with all their meandering false starts, creativity, and faulty reasoning).

Let's apply this version of the context distinction to Kuhn's model of science and see what it would mean to violate it. Suppose a solution to a puzzle has been found and discovered through some creative process of false starts and interesting conjectures. Now suppose that someone insists that an account of the process she went through to find the solution is still needed. Although Kuhn does not mention the context distinction with regard to this question, he makes it clear such an insistence would be inappropriate. He writes, "Once the research is published, the original pictures may even be destroyed" (Kuhn 1969b p. 342). In this passage, Kuhn compares science to art, and he points out that one major difference between the two is that, in art, preliminary sketches have value. Preliminary sketches can provide insight into the final artwork and increase our understanding of it, and they can even have aesthetic value in their own right. Evolving scientific ideas, however, are valued only in their final form. Once a proof has been formulated and accepted by the community, any preliminary ideas leading up to the proof are no longer needed or relevant. Here Kuhn offers a clear acceptance of the psychological version of the context distinction: how an *idea* is developed is not relevant to its justification.

Kuhn continues to endorse this notion that the proofs are of philosophical interest, while the thoughts are not:

> Members of a scientific community share, both in their own eyes and in the public's, a set of problem solutions, but their aesthetic responses and research styles, often painfully eliminated from their published work, are to a considerable degree private and varied. (Kuhn 1996b p. 343)

The implication here is that this privacy is appropriate; the mention of *variety* suggests that there are many ways to reach the proper solution. So it is the solution that matters, not which contingent path one takes to get there.

The scientist's aim, according to Kuhn, is to find one best solution to a puzzle. Once the puzzle is found, he writes, certain things become irrelevant, including: "all earlier attempts"; "traces of private and idiosyncratic factors"; "out-of-date theories"; "original formulations of the current theory." We can note that each of these things are what others have identified as part of the context of discovery, though Kuhn does not mention the context distinction here. Kuhn continues by saying that these things "lose their felt relevance to research"; "go into discard"; "and illuminate only their author's intellectual biography, not the solution of his puzzle" (Kuhn 1996b p. 346-7). The implication is that, unlike in art, the genesis of the scientific puzzle solution does not "guide the viewer to a fuller appreciation" the way the creation story of a painting would help us to better appreciate the painting. "It is why science, as a puzzles-solving enterprise, has no place for museums" (unlike art museums, which house preliminary sketches and descriptions of previous artists) (Kuhn 1996b p. 346).

In this article, Kuhn ends up endorsing an interpretation of the context distinction, though he never mentions the distinction explicitly here. The Psychological Distinction is used to illustrate how a methodology of science aims to prescribe the after-the-fact rational reconstruction of exemplary scientific practice, not a prescription of how scientists should actually behave. For Popper, the proper scientific methodology is to make bold predictions and try to falsify them, and a scientific claim is unjustified if it predicts an observation that does not occur. If a particular scientist fails to follow this procedure when pursuing his ideas (context of discovery), yet in the end his work can be rationally reconstructed in a way that follows this procedure (context of justification), then it was still in accordance with this methodology.[40] Similarly, for Kuhn, the meandering discovery process that a scientist engages in (context of discovery) is irrelevant for determining if she followed his methodology. What matters is that, in the end, her claim is justified if it can be reconstructed (context of justification) as a solution to a puzzle in her disciplinary matrix that adheres to the model illustrated by the exemplar. Thus, at the meta-level, the Psychological Distinction emphasizes the *justification* of a claim as being distinct from the work leading up to the claim. Kuhn is explicitly in agreement with this version of the context distinction.

Connection with the Context Distinction continued

Factor 2: Historical Context[41]

Yet there is another use of the context distinction that often gets employed at the meta-level. The charge is that **whatever method you prescribe for evaluating scientific claims, the method cannot allow values, cultural setting, or historical context to play a role in justification.** These factors are irrelevant at best (Reichenbach 1938) and pernicious at worst (Koertge 2003, Scheffler 1967).

Before turning to Kuhn's complicated relationship with this version of the context distinction, it is helpful to once again recall Kuhn's distinction between normal and revolutionary science. Kuhn offers two different accounts of justification: one is an account

---

[40] The aim of this question is not to determine if the method is correct. That happens at the meta-meta-level, c. Instead the aim is to determine what a given method is, and what it means to follow it.
[41] Factor 3, Values, will be addressed in more detail in the next chapter.

of how a claim is justified within a disciplinary matrix during a period of normal science. Kuhn's other account of justification focuses on choosing between two claims in conflicting disciplinary matrixes – in essence, the justification of one disciplinary matrix over another, during periods of revolution. Kuhn is most loudly charged with violating the context distinction with regard to his claims about periods of revolution, in particular that by allowing values to play a normative role during these periods, he turns science into an irrational process. I will discuss the role of values during revolution in the next chapter. For now I turn to Kuhn's view on the relevancy of historical contexts to justification during the periods of normal science.

In this version of the context distinction, the claim is that **'The historical context of an idea (context of discovery) is irrelevant to the justification of that idea (context of justification).'** I call this the Historical context Distinction. For Kuhn, the relevant historical information when evaluating a given scientific claim is: what disciplinary matrix is this claim a part of? So for Kuhn the role of the context of *discovery* in this version of the distinction is filled by a given *disciplinary matrix*.

The other side is trickier to see, but I argue that for Kuhn the context of *justification* corresponds with the *exemplar* of that disciplinary matrix. The context of justification correlates with Question b at the meta-level, which asks, "By what method can one determine if a general claim H is justified?" To some extent, Kuhn appears to not have an answer to this problem. Indeed, Kuhn explicitly suggests:

> [we should abandon] the view of science as a single monolithic enterprise, bound by a unique method. Rather, it should be seen as a complex but unsystematic structure of distinct specialties … dedicated to changing beliefs … in ways that increase its accuracy and other standard criteria that I mentioned. (Kuhn 1991 p. 119)

Other methodologies, such as Verificationism and Falsificationism, can offer clear methods that one should follow for any given hypothesis H. In contrast, the best Kuhn can do is point to a particular paradigm/disciplinary matrix and say, "Solve the puzzle using whatever tools are available to you within this paradigm." But, Nickles suggests, this is itself a type of method:

> We can interpret *Structure* and the related articles as advancing an alternative conception of scientific methodology rather than as a complete abandonment of the idea of method. To be sure, if we define method as a set of rules, then there is no scientific method, according to Kuhn … [but] …
> insofar as exemplars largely replace rules in our best account of scientific practice, *why not speak of a methodology based on puzzle matching, problem reduction, and the like?* (Nickles 2003 p. 156 emphasis added)

Nickels highlights how, for Kuhn, justification is provided by the exemplar. Nickles writes,

> Exemplars are not merely abstract models but also contain the primary computational resources relevant to solving the new problems with which they are matched. One or more exemplars, suitably adapted, provide a model of one's current puzzle and the sought-for solution. One figures out how to solve the current puzzle by finding sufficiently close matches to puzzles solved previously. (Nickles 2003 p. 149)

> Kuhn makes the strong claim that the paradigm of a mature science 'guarantees,' to the skilled practitioners, the solvability of legitimate problems (149) … [which in the strong sense means that] … every acceptable solution is expected to consist in the application of some combination of extant exemplars by means of standard practices. (Nickles 2003 p. 150)

The goal in normal science, according to Kuhn, is to solve puzzles. The exemplars provide the rubric by which one solves these puzzles by demonstrating what acceptable puzzles consist of, how one will find a solution, and, importantly, how one will recognize the solution once one has found it.

If the context of discovery is fulfilled by the disciplinary matrix definition of paradigm, and the context of justification by the exemplar definition of paradigm, then for Kuhn the context of discovery *is* relevant to the justification of the claim. That is, Kuhn is violating the *Historical context* Distinction if it is taken to mean that the historical context of a claim is irrelevant to its justification.

From Kuhn you seem to get a reverse hierarchy, where the context of discovery (disciplinary matrix) *contains* the context of justification (exemplar), rather than the context of justification being 1) outside of the context of discovery and 2) universal and unchanging. The context of justification is one of the tools available within the context of discovery. It is of course not a one-to-one correspondence, since each concept is uniquely messy, but this might explain some of the confusion and the illusion that the notion of paradigm obscures or collapses the distinction. Once one disambiguates 'paradigm' into distinct parts (disciplinary matrix and exemplar), we can see that some of the meanings of the context distinction serve the functions described by those parts.[42]

Kuhn's view indicates that for the *Historical context* Distinction, the context of discovery is relevant to the context of justification, meaning in this case Kuhn is indeed guilty of violating the context distinction. However, this account could help defend Kuhn against another charge. There is a much weaker version of this context distinction that Kuhn is also accused of violating. A weak version of the context distinction would state that discovery is *distinct* from justification, while a strong version claims discovery is *irrelevant* to justification. Kuhn's view seems consistent with the weaker version here. That is, instead of saying that the two contexts are irrelevant, some claim that these two contexts are merely

[42] Notably, Hoyningen-Huene offers a different reading of Kuhn. He allows that Kuhn might have intended for "disciplinary matrix" and "exemplars" to refer to different aspects of paradigms. However, Hoyningen-Huene maintains that such a position won't succeed, and that such a sharp distinction between these two concepts cannot be made (Hoyningen-Huene 1993, p. 157-8). Although this interpretation might better explain Kuhn's views, part of my concern here is with how Kuhn was read in the 1960s and 1970s, and Hoyningen-Huene's interpretation was not available then.

distinct from each other. They then say that Kuhn "blurs the distinction" between discovery and justification. According to this version of the distinction, it is not blurry. The disciplinary matrix (historical context) admittedly has a complicated relationship with the exemplar (justification), but they can be distinguished as separate things.

There is another, more serious objection that Kuhn often faces at this point. Scheffler, for instance, concedes that Kuhn might be right that scientists working within a given paradigm use the exemplar to evaluate their research claims; however, he continues, what philosophers really care about is whether such research claims are *in fact* justified, not whether they appear justified from within the paradigm. Scheffler writes,

> Proponents of different paradigms, says Kuhn, acquire different criteria for relevant problems and solutions…. This argument, however, confuses *internal* criteria[43], by which paradigms determine problems and solutions, with *external* criteria by which they themselves are judged. The latter are independent of the former, and, hence, the argument that paradigms must inevitably be self-justifying collapses. (Scheffler 1972 368)

And to this Siegel adds:

> Scheffler criticizes this view of Kuhn's by arguing that it blurs a crucial distinction by failing to distinguish between criteria *internal* to a paradigm and criteria *external* to a paradigm. Kuhn is perhaps correct that each paradigm sets criteria such that within a paradigm, certain pieces of evidence or experimentation can only be interpreted or evaluated according to the criteria set forth by the paradigm; however, he has failed to produce any plausible reason for thinking that external criteria – this is, *criteria according to which paradigms are themselves judged* – must be included in a paradigm. Paradigm debate is more properly construed as a second-order, or meta-, activity, and at this higher level of evaluation independent criteria are operative. The distinction between internal and external criteria thus renders gratuitous Kuhn's claim that paradigms are 'self-justifying' – *for in light of the internal/external distinction, it must be concluded that competing paradigms are judged according to external*

---

[43] The internal/external distinction is closely related to the discovery/justification distinction. Confusingly, however, in philosophy and history of science literature sometimes "internal" is associated with universal, logical justification while "external" is associated with historical, contextual factors, and other times, such as in these passages, it is the other way around.

> *criteria, hence not according to criteria which stem from and thus favour one of the paradigms being judged as against its rivals.* (Siegel 1980b p. 362)

What Scheffler, Siegel (and Kordig 1971 p. 105-6) are willing to grant for the sake of argument is that paradigms in fact serve the role of helping practitioners of science determine whether they consider their own claims to be justified. However, what these critics are not willing to grant is that such a role is itself justified. They do not care whether the scientists *think* their claims are justified, but rather whether the claims are *actually* justified. The former is part of the context of discovery, the latter the context of justification. In short, if different paradigms provide different guidelines for determining whether a claim is justified, then these critics still want to know which paradigm is *right*.

From the perspective of Kuhn, however, this last question makes no sense. It represents a complete misunderstanding of the role that he takes paradigms to play. (Recall the section on meta-meta-levels). While some paradigms might be better than others, there is not and cannot be a single right paradigm, and most certainly there is no way to do science or evaluate scientific claims from *outside* of a paradigm (such as a Nagelian "view from nowhere") (See, for instance, Fine 1998).

Hoyningen-Huene offers a similar defense of Kuhn against Siegel's charge:

> One could argue that Kuhn's account is not sufficient since what he proposes as good reasons is individual and group dependent, and therefore also time dependent, and *good* reasons should be completely group and individual independent. But Kuhn would object by insisting that scientific reasons that are group and individual independent, and are furthermore sufficient to determine theory choice *do not exist*. (Hoyningen-Huene 1987 p. 509, emphasis added)

And later Kuhn writes,

> First, the Archimedean platform outside of history, outside of time and space, is gone beyond recall. Second, in its absence, comparative evaluation is all there is. (Kuhn 1991 p. 115)

Here we see that underlying the seemingly simple context distinction is a further assumption, namely that there are universal, timeless, group-independent, logical standards of justification. Hoyningen-Huene suggests that philosophers who talk about the context distinction often fail to disambiguate between two different distinctions (Hoyningen-Huene 1993, p. 248-9): descriptive/normative and empirical/logical.

Hoyningen-Huene argues that the logical empiricist formulation of the context distinction actually has two aspects. The first is that justification for a scientific claim is distinct from that claim's historical development. The second is that justification is purely logical and that logic is objective and universal across time and culture. Kuhn's view of paradigms offers a formidable challenge to this view of logic as the final arbiter of knowledge claims. Indeed, Hoyningen-Huene argues that Kuhn's paradigm model effectively strips away the second meaning of the distinction (empirical/logical), while leaving intact the first meaning (which he identifies as descriptive/normative). Kuhn has tried to change our conception of what the normative standards are, from eternal logical standards to historically embedded and changing standards.

> The distinction simply amounts to the claim that either of the two perspectives (descriptive v. normative) may be adopted with respect to knowledge, and it leaves entirely open what, precisely, epistemic claims consist in or by what criteria they may be evaluated…. [Kuhn's] attack may thus not be *warded off* with the simple observation that it violated the context distinction, for Kuhn never attacks the seemingly philosophically neutral distinction between descriptive and normative. What's at issue is precisely the identification of this distinction with that between the empirical and the logical (Hoyningen-Huene 1993, p. 249-50)

Thus, Hoyningen-Huene argues that Kuhn does attack the context distinction when it is identified with the logical/empirical distinction. I agree with this, and it seems to fit very well with my arguments above that Kuhn's view on the context distinction depends on claims about *Weltanschauungen*. Hoyningen-Huene further argues that Kuhn does not challenge the context distinction when it is identified with the descriptive/normative distinction. This seems more or less consistent with Kuhn's claims, though I suggest that a full answer to this question would require looking at the descriptive/normative distinction as applied to different situations, just as we are doing with different version of the context distinction more generally. Hoyningen-Huene throws in several caveats to his revised context distinction (Hoyningen-Huene 2006) along these lines. One can discuss two different aspects of something (i.e., "make the distinction"). However, he acknowledges, that does not imply either: 1) that the two aspects can in actuality be separated *or* 2) that they do not affect one another. These clarifications make it much easier to identify Kuhn's views as consistent with the view that there are "descriptive perspectives" and "normative perspectives" from which to approach issues in philosophy of science (Hoyningen-Huene 2006).

Like Hoyningen-Huene, I set aside the question of whether Kuhn is right that there is no timeless, logical "Archimedean point" on which to rest our lever of objective justification. What I want to point out with this section is that, just as we saw in the discussion of meta-meta-levels, what appears to be a simple distinction between discovery/justification (here between the historical context of an idea and its justification), is actually entwined with a much larger philosophical debate about the nature of observation, as well as justification and objectivity (see Nagel, Harding, Hankinson-Nelson). As I argue elsewhere for a different situation, to use the context distinction to try to further that other debate is to beg the question at issue.

V.

a. At the object-level (science).

For a given scientific claim, H, is it justified?

To discover further nuances of Kuhn's view, we finally arrive at the object level, making use of specific examples. Kuhn assigns three types of tasks to the activities of normal science: Determining facts, matching facts with theory, and articulating theory (Kuhn 1962 p. 24, ET 232). The relevance of paradigms to the last two tasks is rather clear. If, as I am using it, "historical context" means the paradigm (disciplinary matrix and exemplar) under which the scientific activities are conducted, then Kuhn would clearly consider tasks involving theory to be relative to a given paradigm. However, the task of determining data is less clearly dependent on a paradigm. It seems that one could investigate facts from outside of or independently of any given paradigm. We will return to this third task in a moment. First, however, let us consider the other two, using Kuhn's own example of Copernicus.

Copernicus claims that the earth orbits the sun. This is a propositional claim that can be evaluated based on evidence.[44] Do we need an historical context (paradigm) to tell us whether these claims are justified? From our paradigm today, Copernicus's claim is well justified. Surprising, however, Kuhn argues that from Copernicus's paradigm, the claim was unjustified (Kuhn 1957 p. 73, 229-31). This is because Kuhn considers Copernicus's task to be an attempt to match facts (data observations of orbits) with *existing, Ptolemaic theory*. The orbits were presented as perfectly circular, complete with crystalline spheres and epicenters, and unfortunately the new calculations predicted orbits that were no more

[44] I am sorry to have to say this, knowing how much Kuhn would disapprove, but for our purposes it is irrelevant if this simple historiography is accurate, since what concerns us is Kuhn's own interpretation of events, not the events themselves.

accurate than those by the old calculations it was meant to replace. This is surprising if one considers Copernicus himself to be a member of the new paradigm associated with his name. Kuhn argues, however, that Copernicus himself was still operating mostly under the assumptions, norms, and exemplars provided by Ptolemaic systems. Kuhn's analysis aims to show how Copernicus's view presented an anomaly to the old paradigm, not just as providing the basis for a new paradigm. It is of course Kepler, Galileo, and Newton who pave the way for acceptance of the new paradigm, by providing the match between data and theory that Copernicus lacked (Kuhn 1957 p. 212, 263-5). They did this by using Copernicus's ideas to help articulate a *new* theory. This new theory contained an entirely new set of assumptions, such as inertia and gravity – in the absence of force, objects move at a constant velocity, and massive objects attract one another -- rather than the view that heavy objects *naturally* fall to the center of the universe/center of the earth. From the perspective of the new theory, complete with new assumptions and exemplars, Copernicus's claims were entirely justified.

Crucially, then, for Kuhn, one cannot ask whether a claim is justified *in general*, but whether it is justified from the perspective of a given paradigm. We have seen how this works for claims that attempt to match facts with theory, and that attempt to articulate theory. Yet what about the determining of simple facts? The notion of theory-laden observation can help us see that even for the task of determining facts, historical context (i.e., location within a paradigm) is relevant to "justification." Kuhn considers the example of measuring electrical attraction between various objects. When conducted outside of a paradigm, such experiments "remained *mere* facts, unrelated and unrelatable to the continuing process of electrical research" (Kuhn 1962 p. 35). Instead, Kuhn says, "Only in retrospect, possessed of a subsequent paradigm, can we see what characteristics of electrical phenomena they display" (Kuhn 1962 p. 35). Only when viewed from within Coulomb's paradigm do these facts become useful claims about the world that are related to each other and to laws, and that can be used to make predictions.

Yet one might argue that this misses the point of what "justification" means in this situation. Feigl, for instance, simply means that there are some basic observations that we can make and that we should take these "elementary" observations as the basis of justification (Feigl 1970 p. 9).

> It is simply not true that all empirical knowledge is 'contaminated' by theories. … The phenomenon of the Brownian motion can be *described* independently of the explanations given by Einstein and Smoluchowski in 1905… The laws of the propagation, reflection, and refraction of light (as in geometric optics) can be formulated quite independently of any theory regarding the nature of light (particles, waves, or 'wavicle'!). …. (Feigl 1970 p. 8).

Although Kuhn rejects this, there is still a sense in which his view is consistent with this separation. Kuhn admits that in the very simple sense observations can be made outside of a paradigm. However, one must conclude that for Kuhn these observations would be useless for scientific purposes. Given the onslaught of information available to us, no one would be motivated to offer a description of one's sensations in the absence of some organizing framework, and certainly no one would be able to make sense of them if they were so motivated.

At this level then, we have seen how Kuhn's *Historical context* Distinction plays itself out. Kuhn accepts that most historical factors are unrelated to the justification of a claim. However, Kuhn violates the Historical context Distinction when he makes one crucial exception: one relevant historical factor is which paradigm a claim is a part of. For instance, Kuhn claims that Copernicus's views were justified within later paradigms, but not within Copernicus's own paradigm. Orphan observations – Feigl's 'simple' theory-free observations such as Brownian motion – are possible. However, for Kuhn they are useless; any useful observation will be part of a paradigm.

At this point, we might recall Scheffler and Siegel's objection to Kuhn. They claim that philosophy should not be concerned with how individuals or communities evaluate claims, but rather with which claim is right or justified. We warded this objection off earlier by noting that for Kuhn, claims cannot be evaluated outside of a paradigm. One cannot ask: which paradigm is *right?* However, perhaps one can ask: which paradigm is better? This question asks one to compare between two or more frameworks, and thus it deals with revolutionary change rather than static normal science. We will turn our attention to this and similar questions in the next chapter.

VI. Conclusion

A variety of questions face philosophers of science; the context distinction manifests itself differently for each one. When reflecting on philosophy's own methods, one can ask whether *the way something is done* can serve as justification for *the way it should be done* (Is/Ought Distinction). For many, this has led to the conclusion that history cannot serve as evidence for the proper methodology of science (Historical Evidence Distinction). I have argued that it is possible for Kuhn to accept the Is/Ought Distinction while rejecting the Historical Evidence Distinction.

When trying to identify the proper scientific methodology for evaluating claims, one might argue that whatever it is, it must contain certain features. For instance, claims should be evaluated without regard to how they were discovered (Psychological Distinction), their historical context (Historical context Distinction), or the values of the people proposing or evaluating the claims (Values Distinction). I have shown how Kuhn accepts the Psychological Distinction, rejecting some versions of the Historical context Distinction. In particular, Kuhn accepts that one piece of historical information is crucial to the evaluation of a claim: what paradigm it is a part of. One must distinguish between two meanings of "paradigm" for Kuhn. If we identify the context of discovery with Kuhn's disciplinary matrix, and the context of justification with Kuhn's exemplars, we see that this manifestation of the context of justification is contained with the context of discovery, rather than being outside of and independent of it, which could explain some confusion surrounding Kuhn's views. I will address the Values Distinction in the next chapter. Finally, one can explore how these distinctions play out in specific examples.

Questions are not neutral. By choosing these three questions as central questions in philosophy of science, I set out to explore a particular approach to philosophy of science. To even set up the question this way buys into a hypothetical-deductive model of understanding science. It assumes that science is about examining single statements to determine whether we should write them into the book of science (i.e., accept them as true). Many of the conversations during and after Kuhn's time rejected this view of science, instead seeing science as a rich complex of activities and projects. The central questions of philosophy of science changed, correspondingly.[45] In this sense, some suggest that the more recent

[45] See, for example, (Hoyningen-Huene 1993) on how Kuhnian approaches to science raise different questions than previous approaches.

approaches to philosophy of science do not necessarily violate the context distinction, per se, but rather focus on different questions, such that the context distinction becomes irrelevant.

Yet Kuhn was dealing with questions similar to those of his predecessors, and simply offering substantially different answers. Perhaps Kuhn's lessons about the nature of scientific revolutions can be reflected back on the nature of philosophical revolutions as well. Kuhn explains that Copernicus's astronomy was deeply rooted in the assumptions of the Ptolemaic system. Although Copernicus broke away from his immediate predecessors in that he set the Earth in motion around the Sun, many of the rest of his assumptions remained the same: Copernicus employed crystalline spheres, complicated epicenters, and perfectly circular orbits. Kuhn, as well, can be seen with one foot in each tradition. While one might claim that Kuhn made the context distinction obsolete for future generations of philosophers, it would be a mistake to say (even as he himself does) that his own views embodied violations of the all versions of the context distinction.

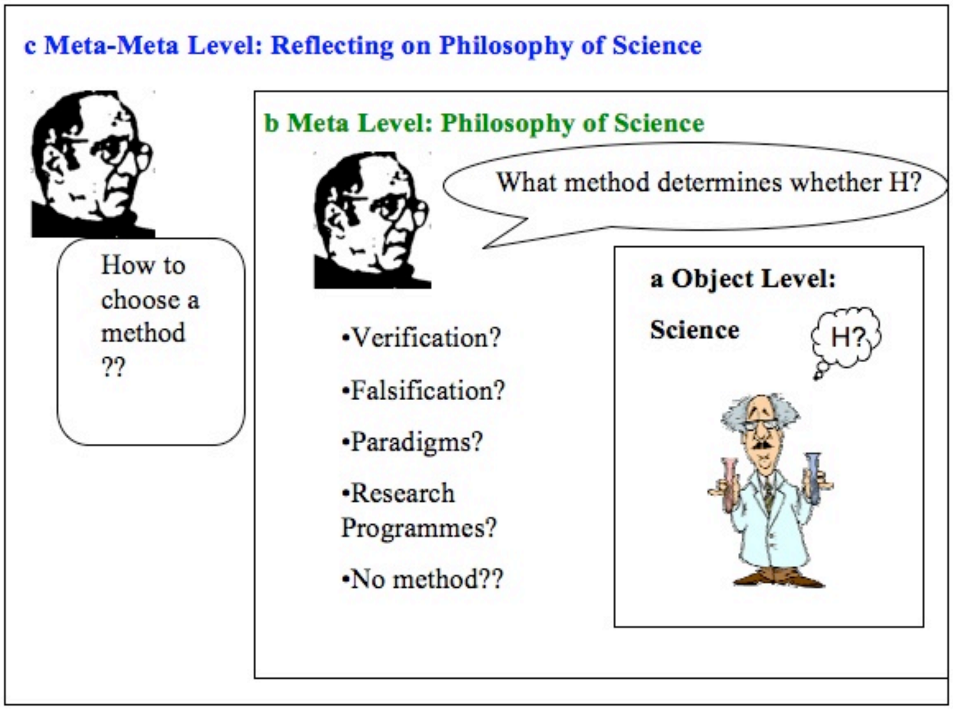

Figure 3 Three Levels in Philosophy of Science

Context of Discovery vs. Context of Justification

Traditional	How an idea occurs to a scientist	Whether the idea is justified	Kuhn
Is/ought	How something is done	How something should be done	X ✓
Historical Evidence	History of Science	Evidence for methodology	X
Psychological	Thought processes	Justification	✓
Historical context	Setting, identity of researcher	Justification	X ✓
Values	Political, moral claims	Justification	X ✓

…is *irrelevant to*…
OR …is *distinct from*…

Figure 4 Versions of the Context Distinction at Different Levels

4. The Structure of Philosophical Revolutions: Values, Rationality, and Theory Choice

I. Introduction

In this project, I have been developing a framework with which to think about the context distinction. I start with a specific philosophical aim that a philosopher has declared he or she is using the context distinction for. I use that aim to clarify their version of the context distinction. Then I evaluate whether the distinction was helpful in achieving that specific aim. This process results in a better understanding of the context distinction, its past uses, and its future promise. In the best cases, this process also results in a better understanding of the issues at stake in a given debate involving the context distinction.

In the last chapter we saw how the context distinction plays itself out for Kuhn during periods of normal science, where the key question at the meta-level is "By what criteria should one determine if a given claim is justified?". In this chapter, the debate focuses on periods of revolution, where the key question is "By what criteria should one choose between two competing claims?" For Kuhn, this translates into debates over *paradigm choice.* In this chapter I articulate Scheffler's argument that adhering to the Values Distinction is necessary in order for theory-choice to be rational and empirically adequate. The Values Distinction, as I called it, is the claim that *the justification for choosing one theory over another is independent of the values of the people evaluating that theory*. In contrast to Scheffler, I argue that the Values Distinction is not the right tool for ensuring science is rational and empirically adequate. Kuhn offers reasons to think that the Values Distinction is neither necessary nor sufficient for these goals; we can reach rationality and empirical adequacy by incorporating non-epistemic values into justification. That is, he argues that epistemic and non-epistemic values play a legitimate and important role in adjudicating between theories, and that this role facilitates rationality and does not impede empirical adequacy. Regardless of whether Kuhn's arguments are successful, we will see that the context distinction, this time in the form of the Values Distinction, is not a neutral tool but rather a controversial claim that requires argument and defense itself.

II. Scheffler's Goal and Reichenbach's Context Distinction

Scheffler is concerned with protecting the notion of science as an objective enterprise, including in the domain of theory-choice (Scheffler 1967 p. 1, 1972). In the face of growing concerns that objectivity is "impossibly unrealistic," Scheffler invokes the context distinction to defend objectivity and what he takes to be its crucial role in science. (Scheffler 1967, p. 67, 73).

Objectivity is a notoriously ambiguous term, not unlike the context distinction itself (Daston and Kitcher 2007, Douglas 2004). Scheffler's account of objectivity is nuanced and complex, so I will focus on two specific aspects of his account of objectivity that are particularly relevant to this discussion. While Scheffler views objectivity as having many features, I will focus on Scheffler's claims that objective science needs to allow rational deliberation and be empirically adequate.[46]

---

[46] For instance, Scheffler writes that, "Objectivity is primarily a matter of institutions which hold beliefs subject to public test by impartial criteria. To participate in the workings of such institutions is to acknowledge the critical relevance of impersonal considerations in the *assessment of one's claims*; it is to accept the responsibility of *rational dialogue with others*

For Scheffler, "objectivity requires the possibility of intelligible debate" (Scheffler 1972, p. 369), where intelligible debate involves "logical deliberation" and the offering of "relevant reasons" (Scheffler 1967, p. 2, 3). He writes that,

> The ideal of *objectivity* is, indeed, closely tied to the general notion of *rationality*, which is theoretically applicable to both the cognitive and the moral spheres. In both spheres, *we honor demands for relevant reasons* and acknowledge control by principle. In both, we suppose a commitment to general rules capable of running against one's wishes in any particular case. (Scheffler 1967, p. 2, emphasis added)

Regarding empirical adequacy, Scheffler often refers to "descriptive accuracy," "observational credibility," and "evidence" (Scheffler 1967, p. 68, 78). Scheffler is particularly concerned that for Kuhn,

> [a]doption of an alternative paradigm often requires *actual defiance of the evidence* and reliance on faith. We have here, it would seem, a *radical rejection of the distinction between discovery and justification*, in any sense at least that would preserve *objective controls* in the sphere of justification. (Scheffler 1967, p. 78, emphasis added)

We see here that Scheffler uses Reichenbach's distinction to create a place for objectivity in the notion of theory evaluation. Elsewhere, he does that even more explicitly:

> The distinction between theory genesis and theory evaluation, between the context of discovery and the context of justification, enables us to say with considerable plausibility that objectivity characterizes the evaluative or justification processes of science rather than the genesis of scientific ideas. (Scheffler 1967, p. 73)

Scheffler's concern here is to demonstrate how objectivity is both an achievable and desirable goal in science. In doing so, Scheffler has made two amendments to the context distinction. Reichenbach's original distinction did not emphasize objectivity *per se*, but

---

in the interest of truth, under the authority of observational credibility and logical cogency... The scientific game imposes the constraints of *descriptive accuracy,* theoretical coherence and *logical discussion*. (Scheffler 1967, p. 68, emphasis added)." He adds that, "Objectivity, in general, is a matter of test, control, and critique, … the evaluation a theory's *empirical adequacy*, its logical coherence, and its relative simplicity." (Scheffler 1967, p. 68-9, emphasis added) rather emphasized logical and critical assessment.[47] Scheffler's first addition to Reichenbach's distinction is to highlight "objectivity" as a crucial feature of such assessment, as he has done in this quote. Scheffler's second addition to Reichenbach's context distinction is to erase the distinction between "scientist" and "philosopher/epistemologist."

As discussed in earlier chapters, Reichenbach distinguishes between the three tasks of philosophy: the descriptive, critical, and advisory tasks. Scheffler's second addition is that he does not exclude scientists themselves from participating in evaluation (the epistemologist's critical task). He argues that there is "no sharp line between the concerns of science and the concerns of epistemology. Scientists themselves are continuously engaged in rational reconstruction, criticism, and evaluation of ideas" (Scheffler 1967, p. 72). Thus, according to Scheffler, even if the actual process of generating new ideas cannot be seen as objective, one could still attribute objectivity to the scientists' processes of evaluating those ideas. "To describe science … *simply* as a generative sequence of new theoretical ideas is … a mistake.… The process of critical appraisal is, then, integral to science" (Scheffler 1967, p. 72). That is, he argues that there is still a place for objectivity in *scientific practice.*

I argued earlier that Reichenbach uses the context distinction and the tool of Rational Reconstructions to focus philosophers on their own activities and away from the activities of scientists. Scheffler, however, sees the same distinction as focusing philosophers' attention back on scientists. This is why, unlike other critics such as Harvey Siegel and Dudley Shapere, Scheffler interprets Kuhn's account of science, with its heavy emphasis on practice,

---

[47] Objectivity does play a role in Reichenbach's contexts, though he does not go into details. Regarding the meaning of "objectivity" Reichenbach mentions "logical" and "not dependent on our choice" (Reichenbach 1938, p. 15).

as truly aimed at the process of the *justification* of theory choice, and not just at their discovery.

III. Scheffler's Critique of Kuhn on Theory Choice

The justification we are concerned with occurs during the fourth stage of science, as described in Kuhn's 1962 *Structure of Scientific Revolutions* (SSR). Pre-paradigm science progresses to normal science (stages one and two). After a while, anomalies accumulate that the paradigm cannot account for, leading to a crisis in the community (stage three). This crisis is resolved when a critical mass accepts the new paradigm, often when members of the old paradigm "convert" to a new paradigm, or when new young scientists get on board.

The questions at the center of Scheffler's criticism are: What, according to Kuhn, are the factors that cause a community to "convert" from one paradigm to another? Are these factors "rational"? Are they part of the context of discovery, or of justification, or does this distinction not apply? These questions are crucial for understanding scientific change and growth, since according to Kuhn it is this process of conversion that locates scientific change.

In SSR, Kuhn is often quoted as suggesting that the process of conversion is based on personal factors such as religion and reputation (153), faith (157-8), persuasion rather than proof (152), values (242) and aesthetics (242). It is vocabulary like this that prompted Scheffler and others to charge Kuhn with improperly placing factors of the context of discovery in the context of justification, and thereby making science look like an irrational enterprise.

Scheffler interprets Kuhn as claiming that the process of evaluation that occurs in the context of justification is not objective:

> It has been suggested that the justificatory processes of scientists themselves fail of objectivity, that personal factors in actuality permeate not only the genesis of theory but also its evaluation, and that psychology is therefore crucially relevant to the explanation of both. The fundamental Reichenbachian distinction between the context of discovery and the context of justification has accordingly been rejected. (Scheffler 1967, p. 73)

For Scheffler, objectivity requires "the possibility of intelligible debate over the comparative merits of rival paradigms" (Scheffler 1972 p. 369). Kuhn's view of paradigm shift, he argues, does not allow for this intelligible debate. He quotes Kuhn as saying those with different paradigms "will inevitably talk through each other when debating the relative merits of their respective paradigms" (Kuhn 1962 1st ed, p. 108).

Regarding empirical adequacy, he writes,

> We are now to take such convergence [of scientific belief] as a product of rhetorical persuasion, psychological conversion, the natural elimination of unreconciled dissidents, and the retraining of the young by the victorious faction. Instead of seeing reality's providing a constraint on scientific belief, reality is now to be seen as a projection of such belief, itself an outcome of non-rational influence. (Scheffler 1967, p. 73-4)

And he notes,

> … the striking way in which Kuhn's account applies psychological, political, and religious categories to the description of the scientific change. The older references to logical system, observation evidence, theoretical simplicity, and experimental test have given way, in his account, to mention of the gestalt switch, conversion, faith, decision, and death. Moreover, such mention is introduced to characterize not only the initial generation of theory but also its subsequent spread and adoption by the scientific community. (Scheffler, 1967, p. 78)

Scheffler reads Kuhn as promoting the view that theory choice is decided by irrational factors and persuasion, not rational, intelligible debate or evidence and tests. Thus, he sees Kuhn as trying to deny the objectivity of theory choice in science by introducing personal features such as faith, values, and aesthetics, in the wrong places.

This in itself would be problematic. However, Scheffler goes one step further, and argues that Kuhn's view is in fact inconsistent. He attributes to Kuhn "the main thesis… that paradigm change in science is not generally subject to deliberation and critical assessment" (Scheffler 1967, p. 89). Yet, he writes, "What compelling reasons have we then been offered for denying objectivity to the processes by which scientific theories are critically evaluated? We have, I believe, been offered none" (Scheffler 1967, p. 88). Indeed, there are aspects of Kuhn's view that run counter to the talk of conversion and faith. In keeping with what he perceives as proclamations of irrationality, Scheffler attributes to Kuhn the main claim that "paradigm change in science is not generally subject to deliberation and critical assessment" (Scheffler 1967, p. 89). Yet, he shows how several features of Kuhn's view preserve this rationality, for example the requirement of predictive accuracy, that anomalies are not ignored forever but do eventually lead to crisis, and the "preservation of previously acquired problem-solving abilities" (Scheffler 1967, p. 89).

Scheffler is charging Kuhn with *claiming*, on one hand, to be taking rational deliberation and empirical adequacy out of science, and yet on the other hand to be re-introducing these features when Kuhn offers the details of paradigm shifts. From these features, Scheffler concludes that Kuhn "seems to reinstate the very distinction between discovery and justification with which we started," since, in Scheffler's mind, this distinction is all that ensures these features of objectivity. Here Scheffler seem to have in mind the Values Distinction. And yet we know that Kuhn rejects the Values Distinction when he introduces personal features into theory-choice (as I will explain in more detail below). For Scheffler, this acceptance and rejection of objectivity and the context distinction marks the ultimate confusion and contradiction in Kuhn's account.

However, if we read Kuhn as claiming that deliberation and empirical adequacy are essential features of theory-choice *despite* his rejection of the Values Distinction, then that would explain the apparent inconsistencies. In fact, Kuhn sees personal factors as central to ensuring deliberation and empirical adequacy.

However, before we turn to Kuhn's argument, it is helpful to show how other philosophers have used the context distinction against Kuhn in the area of theory choice. As we have seen, Scheffler argues that Kuhn promotes bringing personal factors from the context of discovery into the context of justification, and that in doing so he sacrifices important features of objectivity and contradicts himself. In contrast, Harvey Siegel, another strong critic of Kuhn, argues that Kuhn never actually violates the context distinction because Kuhn never succeeds in showing how these personal factors are relevant to justification. To use more terminology developed in the previous chapter, Siegel argues that Kuhn never violates the Values Distinction (the claim that personal factors are irrelevant to justification), because he never violates what I call the Psychological Distinction (the claim that the thought process leading up to a scientific idea is irrelevant to that idea's justification).

Essentially, Siegel argues that Kuhn misses the point. Kuhn's view, he says, fails in its aim to give an account of what scientific change should be like. Siegel claims that at best Kuhn has given an account of how actual historical figures have decided between two paradigms, but that falls short of explaining how such choices *should* be made. For example, Siegel writes,

> Whatever factors led scientists to adopt the oxygen theory (for example), we still can ask questions about the justification of that decision. As noted already, the context of justification is distinct from the "context of *decision*." So at the most Kuhn has shown that factors relevant to the context of discovery are relevant to the context of *decision*. (Siegel 1980 pg. 312, emphasis added)

While Kuhn has attempted to show that personal factors are relevant to justification, Siegel argues that this fails since personal factors are relevant only to the actual decision process that goes into making a choice, and not to what the justified choice would be. He argues that one can ask what factors led a scientist to make a particular choice, but the philosopher is concerned with whether it was the *right* choice (Siegel 1980, p. 370).

Yet there is an ambiguity here between the Psychological Distinction and the Values Distinction. Siegel is assuming that if one accepts the Psychological Distinction, then one must also accept the Values Distinction. In the last chapter, I argue that Kuhn accepts the first distinction, and in the next section we will see how he rejects the latter. For Siegel, such a position is incoherent, but when we disambiguate the two, we see it is not obvious that the first distinction necessarily implies the second. Even if we grant that our concern is with what a *right* or justified choice would be (as in the context of justification), rather than what choice a given scientist actually made (in the Psychological context of discovery), this does not rule out the possibility that personal factors (from the Values context of discovery) will be relevant or even necessary to discern that right choice. That is because the two distinctions deal with different issues. Thus, Siegel's argument begs the question against Kuhn, since it assumes a fixed understanding of what constitutes justification.[48]

[48] This argument has many similarities with two arguments I discuss in Chapters 3 and 5. In Chapter 5, I address Noretta Koertge's argument against Feminist Epistemology approaches to philosophy of science. Koertge argues for blocking personal and political bias from the testing of ideas. In Chapter 3, I note how Siegel has a different argument that begs the question when considering exactly how history of science can inform philosophy of science (at the meta-meta level), and whether historical context can play a role in justification (at the meta level).

The contrast with Siegel's argument helps us clarify Scheffler's argument against Kuhn. Like Siegel, Scheffler does believe there are *some* instances where attacks on objectivity in science actually address the Psychological context of discovery, and so do not succeeding in making any new claims about justification. For instance, Scheffler notes that the study of history "reveals striking pervasiveness of personal and subjective factors" among scientists, countering the image of the detached and distant observer (Scheffler 1967, p. 67). Scheffler initially offers a straight-forward application of the Psychological Context Distinction, as Siegel does above:

> The historical fact that scientists differ in personal characteristics… does not in the least threaten objectivity as a feature of science…. The scientific game imposes the constraints of descriptive accuracy, theoretical coherence, and logical discussion; it imposes no general limitations on passion, imagination, or flair…Creation is free; discipline enters in the evaluation of a theory. (Scheffler 1967, p. 68-9).

Scheffler claims the ideal of objectivity does not require scientists to be "disembodied and passionless" intellects or "robots." A scientist is free to make very human and very personal decisions, to "choose," "revise," "guess," "extrapolate," and "invent." These non-objective procedures are acceptable in science, because they will all "later be subjected to critical assessment" and rational reconstruction (Scheffler 1967, p. 68).

However, as we have seen, Scheffler's defense of objectivity does not end here. While Scheffler agrees with Siegel that the Psychological Distinction can protect some aspects of science from subjective factors, he does not assume that it will automatically protect it from all factors. That is, Scheffler seems to recognize a separation between the Psychological Distinction and the Values Distinction, while still endorsing both. Thus, unlike Siegel, Scheffler thinks that Kuhn's view *does* (mistakenly) promote adding subjectivity in the form of values to the context of justification, not just the context of discovery.

IV. Kuhn's Response and Objection to Scheffler

The Values Distinction is unnecessary for making science objective. In fact, values in justification can help achieve objectivity.

Scheffler charges Kuhn with violating the Values Distinction and making science out to be non-objective. I have focused on two aspects of objectivity that concern Scheffler: rationality and empirical adequacy. Scheffler points out the many subjective factors that Kuhn endorses bringing into the justification of theory choice, and argues that if these subjective factors were indeed involved, then theory choice would be irrational and empirically inadequate. However, I argue that for the most part Kuhn sees these subjective factors as a useful and necessary part of justification; they do not conflict with empirical adequacy, and they do not preclude rational debate.

I say "for the most part" because Kuhn places a different interpretation on the role of values during the early stages of a paradigm formation. I will return to this point when considering objections to this interpretation of Kuhn. For now, I will focus on Kuhn's views for choosing between developed paradigms.

Kuhn's response is now relatively familiar, having been picked up and elaborated on by many after Kuhn (including, but not limited to, Hoyningen-Huene 1992, McMullin 1993, Longino 1990, and Gutting 1980).[49] My aim here is not to convince the reader of Kuhn's argument, but rather to use this more fine-grained version of the context distinction to show

---

[49] My understanding of Kuhn and thus the following account draws heavily on Feminist Epistemology discussions of Kuhn and values in science.

how this version does not help achieve Scheffler's goals because Kuhn can achieve Scheffler's goals without recourse to the Values Distinction. So to use the new terminology: Kuhn has the resources to show that the Values Distinction is not necessary to ensure that theory choice is rational and empirically adequate. Indeed, he seems to suggest (as others do after him), that the Values Distinction is pernicious to these goals.

Earlier I offered the characterization of the Values Distinction as the claim that *the justification for choosing one theory over another is independent of the values of the people evaluating that theory.* But here we must clarify what is meant by 'values.'

Kuhn notoriously proposes that theory choice is in part determined by a minimum of five values: accuracy, consistency, broad scope, simplicity, and fruitfulness (Kuhn 1973, p. 321-2, also 1969, p. 184-5). These values are now known as "epistemic values," (McMullin 1993, see also Longino 1990), and can be contrasted with non-epistemic values such as religious faith, aesthetics, and politics. Crucial features of these epistemic values are that they apply to all paradigms and they are shared by all members of the scientific community: "Together with other [values] much of the same sort, they provide *the* shared basis for theory choice" (Kuhn 1973, p. 322). They are meant to offer the sought after second-order criteria (Siegel 1976, Scheffler 1967, p. 85, Shapere 1964) by which paradigms can be judged.

If Kuhn were to end his story of paradigm choice here, then he would not be in danger of violating the Values Distinction, nor of being seen as saying science is irrational and non-objective. These so-called "values" were well-established tools for theory choice when Kuhn wrote them down, and they are seen as a crucial part of justification by Scheffler and others in the debate (Scheffler 1972, p. 369, 1967, p. 9). Kuhn actually saw himself as corresponding very closely with established views when describing these values as a crucial part of scientific justification. Because of this, he did not emphasize these values in SSR. Ten years after his influential book, Kuhn writes,

> It is past time for me to describe, at greater length and with greater precision, what has been on my mind… If I have been reluctant to do so in the past, that is largely because I have preferred to devote attention to areas in which my views diverge more sharply from those currently received than they do with respect to theory choice. (Kuhn 1973, p. 321)

If one is to look for novelty in his views, he suggests, this is not the place to look.

Yet these values are not the end of the story. The values themselves are not easy to apply. As Kuhn writes, "When scientists must choose between competing theories, two men fully committed to the same list of criteria [values] may nevertheless reach different conclusions" (Kuhn 1973, p. 324). This is because the values are imprecise ("Individuals may legitimately differ about their application") and often conflict with each other (Kuhn 1973, p. 322).

> Perhaps [scientists] interpret simplicity differently or have different convictions about the range of fields within which the consistency criterion must be met. Or perhaps they agree about these matters but differ about the relative weights to be accorded to these or to other criteria when several are deployed together. (Kuhn 1973, 324)

That is, there is considerable room for disagreement on how to apply the values.

On what basis, then, can scientists rest their judgment? Kuhn suggests that non-epistemic values and personal factors play the role of deciding when and how to apply the epistemic values. These could include where one has previously worked as a scientist, the prevailing ideas and politics of one's day, or even personality:

> Some scientists place more premium than others on originality and are correspondingly more willing to take risks; some scientists prefer comprehensive, unified theories to precise and detailed problems solutions. (Kuhn 1973, p. 325)

All of these non-epistemic values mix with the epistemic values to help one decide which paradigm to choose.

Some critics, such as Shapere, read about this mixing and interpret Kuhn as saying that "the decision of a scientific group to adopt a new paradigm cannot be based on good reasons of any kind, factual or otherwise" (1966, p. 67). Yet the “good reasons” Shapere and Scheffler are concerned with have already been accounted for. Empirical adequacy, consistency and simplicity are central to theory choice, according to Kuhn. What distinguishes Kuhn’s view is that he maintains that even after embracing these epistemic values, one must still decide *how* to apply them. Even during the rational deliberation that Scheffler and Shapere want, there still needs to be a basis for the decisions. Non-epistemic values supposedly provide that basis.

There have been several reactions to Kuhn’s argument on this point. Koertge, for instance, argues that in some cases it is true that empirical values are not sufficient for determining some theory choices, but not for all cases. In those unfortunate cases where they are not sufficient, the proper action is to remain uncommitted. Do not use anything to fill the gap, and refrain from making scientific claims one way or the other (Koertge 2003).

Kuhn’s novel claim, however, is to say that it’s not simply that empirical values *tend* not to be sufficient, but rather that they are never sufficient, and never could be. “What the tradition sees as eliminable imperfections in its rules of choice I take to be in part responses to the essential nature of science” (Kuhn 1973, p. 330). Those are not “unfortunate cases” that we should try to eliminate, but rather are central to what it means to try to understand the natural world. This means that one could refuse to bring in personal factors, but then one will never be able to apply the epistemic values.

Kuhn offers several arguments for this stronger claim. His other arguments, based on theory-laden observation, talks of vision, and gestalt switches, are discussed at length elsewhere (Kuhn 1977, 2000). Here I focus on the argument against rules. Kuhn maintains that epistemic values function as general guidelines to be evaluated and balanced, and not as fixed rules. Unlike rules, guidelines leave room for interpretation and cannot lead to a clear, unique conclusion. Thus, epistemic values can never be sufficient for determining theory choice. Thus we have our 'gap' between evidence and conclusion. From this, Kuhn concludes that non-epistemic features are the only thing left that can determine the final choice. "The choices scientists make between competing theories depend not only on shared criteria – those my critics call objective – but also on idiosyncratic factors dependent on individual biography and personality. The later are, in my critics' vocabulary, subjective" (Kuhn 1973, p. 329). These subjective factors do not threaten rationality or empirical adequacy, however, since those have already been accounted for.

What is interesting about this argument is that Scheffler completely agrees that the epistemic values guide theory choice, that they do not function as rules, and that they do not determine a unique result. Reasonable people can, he emphasizes, disagree. (Scheffler 1967, p. 86, Scheffler 1972, p. 368-9) He does not see this as grounds for violating the Values Distinction, however. Doing so and bringing non-epistemic values into the debates about theory justification will presumably lead to a back-slide – the worry seems to be that it will result in a loss of all the hard-earned rational discussions and a disregard for the empirical evidence on the table.

What can account for this disagreement? First, when Kuhn says that values are relevant to theory choice, this claim needs to be disambiguated. It is really two claims[50]:

1) Empirical values such as empirical adequacy, predictive accuracy, coherence, simplicity, "etc." are relevant to whether a claim is justified.

2) Non-empirical values such a personal factors are also relevant to whether a claim is justified.

The former claim does not violate the Values Distinction, since everyone agrees that these sorts of "values" are part of context of justification, not context of discovery.

The latter claim does violate the Values Distinction, since these are precisely the sorts of factors that people using this version of the context distinction are trying to exclude from the context of justification. Yet Kuhn thinks empirical adequacy and rational deliberation can be maintained, as long as the non-epistemic values play a secondary role to the epistemic values. In fact, he maintains that they cannot be applied at all without non-epistemic values. For Scheffler, the use of non-epistemic values in justification always creates a problem. Scientific debate breaks down, and empirical adequacy is set aside (Scheffler 1967, p. 73-4).[51]

Kuhn sees the issue differently. He also places epistemic values front and foremost, but rather than seeing a choice – either non-epistemic values are involved or they are not – Kuhn looks at exactly *how* non-epistemic values are involved. For him, the crucial questions are: Are epistemic values enough to do the job alone or not? If not, can non-epistemic values fill in the rest, while still keeping science empirically adequate and rational? Kuhn agrees that it would be an epistemic mistake or irrational to place non-epistemic values above

[50] McMullin would say it is really three claims, since McMullin distinguishes between two types of empirical values (McMullin 1993).
[51] We will return to this discussion in the next chapter.

epistemic values in a hierarchy (e.g., to ignore blatant empirical inadequacy). Kuhn writes, "No part of my argument … implies that scientists may choose any theory they like" and "nature cannot be forced into an arbitrary set of conceptual boxes" (Kuhn 1969c, p. 159). However, placing them below should not be a problem, he argues.[52] So, while Scheffler claims that non-epistemic values should not be present at all, Kuhn claims that epistemic values should be primary in justification, and non-epistemic values secondary.[53]

What are the implications for objectivity? Or put another way, what is there here for Scheffler to disagree with? Scheffler readily agrees that reasonable disagreement is possible, indeed necessarily present (Scheffler 1967, p. 369). Yet what would be the basis for this disagreement, if not the idiosyncratic personal factors that Kuhn refers to? A great deal of literature has more recently appeared about on the topic of reasonable disagreement, but this literature was not part of the discussion between Scheffler and Kuhn. At the very least, from the existence of this literature, we can conclude that Scheffler's easy dismissal of Kuhn for bringing subjective factors to adjudicate reasonable disagreements is perhaps too hasty. I am tempted to say that Scheffler and Kuhn agree on much more than it initially appears, a view that Kuhn has articulated for years, and that Scheffler at points seems to unwittingly agree with (Scheffler 1967, p. 88, Kuhn 1973, p. 321, Kuhn 1969c, p. 156-7).

---

[52] This reading is complicated by the fact that he often describes people who do just that (cf. Kuhn 1962, 152-3). Those people are outside of our current conversation, however. As I will discuss in the next section, we should consider such people to be pioneers.

[53] As Hoyningen-Huene puts it, the individual values can 'colour' the communal values, as long as they do not 'overpower' them (Hoyningen-Huene 1993, p. 495).

So if their positions are not entirely dissimilar, or, at least as I am claiming, their true disagreements lie in a different place,[54] then what led Scheffler to point his criticism in the direction that he did? What led Scheffler to view Kuhn as suggesting that, "Adoption of an alternative paradigm often requires actual defiance of the evidence and reliance on faith" (Scheffler 1967, p. 78)?

V. Objections to this Interpretation of Kuhn

Obj 1: Gestalt switches deny deliberation

One reason to think that Scheffler sees paradigm choice as irrational is Kuhn's frequent references to "gestalt switch" and conversion. To Scheffler and Siegel, this must imply that Kuhn sees paradigm choice as not subject to rational deliberation, but rather irrational impulsions. As Siegel writes,

> If we take Kuhn's denial of the irrationality thesis seriously, he may then be a proponent of objectivity in science – thus agreeing with his critics. However, it is difficult to take that denial seriously, in light of his reluctance to give up the incommensurability thesis, his hedging over the status of reasons, and his continued descriptions of theory choices as gestalt switches and conversion experiences. (Siegel 1980, p. 365)

In this objection, Siegel and Scheffler are in agreement. Regarding incommensurability, their concern is that two paradigms cannot be compared. If paradigms cannot be compared, they cannot be rationally debated. Kuhn later says that paradigms can indeed be compared – though not directly or point-by-point, as Siegel and others would wish (Siegel 1980 p. 367,

[54] This is a not uncommon outsider's view of the Scheffler/Kuhn debates. See for instance (Meiland 1974).

Kuhn1969c p. 155).[55] Rather, it seems, paradigms can be compared when considered as a whole, using the aforementioned values of simplicity, fruitfulness, and the like. But for someone to be able to make this back and forth comparison in their mind they must first learn to 'see' the world through the new paradigm. They must first be "converted."

Kuhn often suggests that switching from one paradigm to another is akin to conversion or 'gestalt-switch,' like looking at a visual illusion and first seeing at as a drawing of a duck, and then suddenly of a rabbit (Kuhn 1962, p. 111-117). Of this switching, Scheffler writes, "We are led to develop a view that emphasizes the *intuitive and spontaneous* shift of thought and leaves no room for deliberation or interpretation" (Scheffler 1972, p. 371) and as we saw before, "*Evaluative arguments* over the merits of alternative paradigms are vastly minimized, such arguments being circular, and the essential factor consisting anyway *not in deliberation* or interpretation but rather in the gestalt switch" (Scheffler 1967, p. 78, emphasis added).

However, I suggest that Kuhn's analogy of "conversion" has more to do with the *holist* feature of the shift that occurs when one adopts a new paradigm, rather than rejecting deliberation. Just as with a gestalt switch, one cannot shift a few pieces at a time. One must adopt the new disciplinary matrix all at once, with assumptions, norms, exemplars all together. In this way, the gestalt metaphor is apt. One cannot see the bill of a duck on the left side while seeing the ear of a rabbit on the right side. One sees either a duck or a rabbit at any given time, not bits of each. And so one must adopt a new paradigm as a whole set of

[55] For a defense of Kuhn's view that incommensurability does not imply incomparability, see especially (Hoyningen-Huene 1993, p. 218-222).

beliefs, not a few at a time. The word 'conversion' conveys this sense of sudden, holistic shifts.

This interpretation raises another concern. Kuhn documents how the canonical shift from a Ptolemaic to a Copernican system took centuries, and he considers it a primary example (Kuhn 1957, 1962). So what can he mean when he claims that paradigms shifts are sudden? I propose that Kuhn's frequent contrast between individuals and communities can help make sense of this. The individual who grasps the new idea experiences the suddenness; this is a psychological claim about an individual. In contrast, for enough people to accept the idea such that there is an entire community built around the new paradigm could take centuries.

Obj 2: Kuhn ***says*** *paradigm choice is faith-based, irrational, and defies evidence*

Imagine that the year is 1543 and you have just sat down to read a new copy of *De Revolutionibus Orbium Coelestium*. You read of new ellipticals that place the Earth along with the planets on crystalline spheres that circle the sun, which is at the center. The mathematical formulas are complex, but you are a trained astronomer and mathematician, so you read them through. When you are done, you meet with your colleagues to discuss the ideas you have read. Are you convinced that this new system is better than the Ptolemaic system you were taught? With your colleagues, you debate what it means to be a "better" theory. After many calculations, you realize that Copernicus's system offers predictions no better than your old system. That is, it is no more empirically adequate. Should you be convinced?

Many think that such an example perfectly captures what Kuhn means when he discusses theory change. I suggest this is inaccurate. As Hoyningen-Huene notes, Kuhn divides theory choice into several stages, not just one (Hoyningen-Huene 1993, p. 139). We can see that this example highlights an early stage: we have not been presented with a choice between two *paradigms,* but rather between 1) a paradigm and 2) a general idea in its pre-paradigm state. Kuhn suggests that to make a decision at this point would indeed be irrational:

> Crisis alone is not enough. There must also be a basis, though it need be neither rational nor ultimately correct, for faith in the particular candidate chosen. Something must make at least a few scientists feel that the new proposal is on the right track, and sometimes it is only personal and inarticulate aesthetic considerations that can do that. (Kuhn 1962, p. 158)

As we have seen, the new theory is less empirically adequate than the old paradigm, it is inconsistent with other well-established beliefs, with nothing to replace them with. Its gains in simplicity are debatable (given that Copernicus was still using circular orbits, many more epicycles were necessary to account for the observed locations of the planets). In short, there are no convincing reasons to reject the established theory for the new one.

Yet, Kuhn notes, people make that decision anyway. On what basis? This is where personal factors such as religious belief and the quest for prestige might play a role (Kuhn, 1962, p. 152-3). Kuhn does not condemn these choices, but neither does he see them as rational, nor as the basis for *justification* of a theory choice. Later, he writes,

> The choice of a theory…involves major risks, particularly in its early stages. Some scientists must, by virtue of a value system differing in its applicability from the average, choose it early, or it will not be developed to the point of general persuasiveness. The choices dictated by these atypical value systems are, however, generally wrong. If all members of the community applied values in the same high-risk way, the group's enterprise would cease. (Kuhn, 1969c, p. 158)

Let's skip ahead almost 80 years to the year 1621. Few, if any, of the initial readers of *De Revolutionibus Orbium Coelestium* are still alive. The ideas within it are no longer radical when you sit to read Kepler's *Epitome Astronomiae Copernicanae.* Now, the crystalline spheres are gone, replaced by giant platonic solids. The Earth orbits the sun, but along an ellipse, not a sphere. Epicycles are gone. When you begin to deliberate the merits of the revised theory, you find that the empirical adequacy is astounding and unprecedented. In many ways, the theory is now simpler, although it is still inconsistent with other beliefs. For instance, there is still no good understanding of how the Earth could be moving without sending its inhabitants flying off into space.

Now this is the point at which Kuhn claims that good, rational deliberation about theory choice can take place. With Copernicus, the new theory simply was not developed enough. To embrace it at that stage would have been premature, although many people did, including Kepler and his teacher Michael Mästlin. However, it takes a while for the ideas to be developed, and not until this development has taken place can there be enough evidence to rationally warrant choosing a new paradigm. As Hoyningen-Huene writes,

> The reasons that prompt the first supporters of a new theory to make their choice may, indeed, be of dubious rationality. But whether a new theory is capable of attracting further adherents, and eventually convincing the entire community, depends on the arguments these first adherents produce in fleshing out the theory. (Hoyningen-Huene 1993, p. 240)

Those earlier adherents are what I call *pioneers*. Kuhn introduces this idea in SSR, as we have seen, though it is not obvious. It is easy to see how the idea got missed by many readers of SSR. Here is one of the key passages introducing the notion of pioneers:

> The man who embraces a new paradigm at an *early stage* must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith

> that the new paradigm will succeed with the many large problems that confront it, knowing that the older paradigm has failed with a few. A decision *of that kind* can only be made on faith. (Kuhn 1962, p. 158, emphasis added).

Scheffler includes this passage as a reference when he interprets Kuhn as saying that paradigm choice is always based on faith -- not just at the stage of generating ideas, but also at the stage at which paradigms are more generally adopted by a community -- and that faith replaces evaluative arguments (Scheffler 1967, p. 78, 1972, p. 368). Yet the key words, as we saw in the 1969 quote, are *early stage* and *a decision of that kind.* This particular reference to faith applies to the specific circumstances of the pioneer. As Kuhn writes later on that page, "If a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the point where hardheaded argument can be produced" (Kuhn 1962, p. 158, see also p. 153). These irrational pioneers labor to gather evidence and develop inchoate theories so that later rational debate over the merits of the emerging paradigm will be possible

Early pioneers set out to explore new territory. It is empirically dangerous work, since one must proceed with little evidence and much unwarranted faith. For instance, Michael Faraday initially wrote of the "*electro-tonic* state of matter,"

> This peculiar condition shows no known electrical effects whilst it continues; nor have I yet been able to discover any peculiar powers exerted, or properties possessed, by matter whilst retained in this state …The substances experimented upon, if electrical conductors, *must have* acquired this state; and yet no evidence of attractive or repulsive powers has been observed (*Diary* 60-62, reproduced in Williams 1965, emphasis added).

Faraday's biographer, L. Pearce Williams, offers the following comments on this passage:

> This is a very peculiar position to find Faraday in. His objections to Ampère's theory had been based on a suspicion of hypothetical entities that had to be deduced from experiment. Yet, here was Faraday suggesting a hypothetical state into which all conducting substances were thrown when under magnetic influence. The state had

> the very embarrassing property of being totally undetectable! Faraday, nevertheless, clung to it and never gave up looking for evidence of its existence. He was alternately to reject it and then, almost immediately, try once more to detect it. (Williams 1965, p.198)

This example is taken from what would become a very successful paradigm. Similar persistence is shown by another pioneer, whose theory never advanced past the pre-paradigm stage. In 1894, Hanns Hörbiger had a flash of insight that led him to realize the moon, and other heavenly bodies, were made completely of ice. Void of any other evidence except the clarity of his ideas, Hörbiger dedicated his life to proving his theory of *Welteis* (Wessely, forthcoming). Many contemporaries and historians have charged Hörbiger's science as pseudo-science. This only strengthens my points that 1) great dedication can occur at the early stages of theory, that 2) this dedication can be seen as irrational and contrary to empirical predictions, since it occurs in advance of anything resembling a paradigm and offers insufficient evidence to convince someone else to abandon their own paradigm in favor of the inchoate theory.

Contrast these two examples with other ideas, all of which developed into full-fledged paradigms: phlogiston, the Ptolemaic system, Newtonian mechanics. These are all considered failed paradigms now, but the point is that the ideas were sufficiently developed and had enough evidence in their favor that they advanced to the paradigm stage. It is only at this stage that rational deliberation could be made about accepting or rejecting them.

For pre-paradigmatic science, Kuhn seems to be suggesting, no rational deliberation is possible, since there simply is not enough evidence to base it on. Some, such as Allan Franklin, have challenged the notion that *pre*-paradigmatic science is marked by irrationality (Franklin 1993). He carefully documents how researchers pursued the possibility of a Fifth force, which would have supplemented gravity, the weak and strong forces, and electromagnetism. However, we can see this case as an example of a failed hypothesis well within normal science puzzle-solving activity, not as the pursuit of a new paradigm. Although discovery of a new force would be incredible, it might not challenge existing assumptions and it would not require a paradigm shift to a new way of seeing the world. As one reader of Kuhn notes, finding aliens would not force a paradigm shift, since they are expected under current astronomy and biology paradigms (but not, say, under creationism). Additionally, nothing in Kuhn's notion of pioneers precludes the possibility of pioneers exploring cautiously, using the best of techniques. What it does claim is that if pioneers are *convinced* of the truth of their new fledgling ideas at this early stage, as Hörbiger was, then they might be making decisions based more on personal factors, not personal factors plus evidence and empirical values, and that they might be acting irrationally.

However, these cases do not violate the Values Distinction, despite the central role of personal factors, since Kuhn does not view them as part of the context of justification.[56] Thus, Scheffler is right when he notes that Kuhn sees people making decisions based on non-empirical factors, and these decisions are often irrational, but this is irrelevant. It does not bear on the question at hand: Is the justification of paradigm-choice rational and empirically adequate? That's because these people are early pioneers who are 1) not choosing between two fully developed paradigms and so 2) Kuhn does not see this as an instance of *justification*. The notion of pioneers could help explain why Scheffler would misread Kuhn as suggesting that paradigm or theory choice is always irrational and empirically inadequate.

---

[56] This is where Siegel's distinction between the context of *discovery* and the context of *decision* as mentioned earlier would be appropriate.

The quotes he selects from Kuhn are often directed at the choices of pioneers, not at those who choose to adopt more developed paradigms.

Not only can the notion of paradigms explain mis-readings of Kuhn, making him appear to attribute more irrationality to theory choice than he actually does, but the notion of pioneers might also explain why non-academic retrospective accounts of theory-change often describe early adherents as people with *intuitive* genius: only the successful pre-paradigms survive to the paradigm stage, and hence only the successful pioneers are talked about.

VI. Legacy

Kuhn is often charged with making science look like an irrational enterprise. Yet Kuhn suggests that his inspiration for the idea of paradigms comes from the new historicist project to view historical figures on their own terms (see especially Hoyningen-Huene 1993, Ch 1). Part of that project is to reconsider apparently irrational episodes in science, and to re-explain them using the actors' categories. Kuhn's paradigm shifts have been used to explain one sort of episode: the important historical figure, such as Aristotle or Kelvin, who holds bizarre beliefs. Of the new historicists, Kuhn writes,

> The more carefully they study, say, Aristotelian dynamics, phlogiston chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today (Kuhn 1962, p. 2).

Seeing Aristotle's views on motion as part of a complex network of assumptions helps to explain his otherwise irrational looking choices.

The notion of pioneers introduced above can help explain another irrational behavior, though one that was not always recognized as needing explanation: the "lone genius." In non-academic histories, figures such as Copernicus, Newton, and Einstein are often attributed as having some deeper insight into nature, better intuitions, or that they could simply "sniff out the truth." Such claims make the actions of these early figures mystical and completely inexplicable. The notion of pioneers offers an alternative explanation.

> To give these decisions a "*historical character*" or to suggest that they are made only "*with hindsight*" deprives them of their function. (Kuhn, 1969c, p. 158)

Identifying pioneers of only successful paradigms, such as Kepler and Einstein, provides a distorted view of pioneer work, since it excludes the often very similar work done by pioneers who failed to develop paradigms because they were off the mark -- the world never supplied the necessary evidence, since the theories were inaccurate -- or those people who successfully developed paradigms that have since been rejected. One can imagine how early pioneers of unsuccessful pre-paradigms failed in their journeys; they were unable to provide more evidence for their claims, so their initial ideas were never developed into paradigms. No normal science was established, no research was conducted following a particular exemplar and thus both they and their novel ideas have faded to obscurity. The stories that do survive tend to favor theories that developed into full-fledged paradigms: phlogiston, the Ptolemaic system, Newtonian mechanics.

By focusing on the survivors, we get the false sense that they had some special knowledge, and hence the suggestion that they had an "intuition for truth." By including all pioneers, however, we can see them in context and see that pioneers had a variety of reasons for pursuing their paradigms. Some of these reasons were better than others; they were based on personal factors, religion, prestige, hunches, and even preliminary research. Not all of the reasons are rational, but they are at least explainable, unlike some mysterious access to truth.

Kuhn inspired entire schools of thought, most notably the Sociology of Scientific Knowledge and Feminist Epistemology. However, as is often noted, these schools are as different from Kuhn as they are from each other. What is it, then, that they have in common and what distinguishes them from each other? One feature is the attempt to look at seemingly irrational behavior and to see if it can be explained, in particular explained without recourse to *truth*, since access to truth is unavailable. SSK and Feminist Epistemology approaches to science share with Kuhn 1) the desire to explain seemingly "irrational" and rational behavior alike, 2) this rejection of truth as an explanation or even as a goal[57], and 3) the willingness to include these personal "subjective" factors in explanations of legitimate scientific practice and even justification. Where they disagree with each other (and even within each approach) is which irrational behaviors need explaining, and exactly what role these non-epistemic factors should play. If we return to the notion of a hierarchy introduced in Section IV, we see that, like Kuhn, the Feminist Epistemology approach places epistemic values above non-epistemic values in the hierarchy (Longino 1990), whereas the SSK approach seems to place them on par.[58]

[57] Scheffler and Siegel make a nice distinction on this point: "While truth is not a criterion of theory choice (that is, we do not pick one theory over another *because* one is 'more true'), the theory chosen on independent grounds of credibility has a stronger claim to represent the truth than a theory rejected on those grounds" (Siegel 1976, p. 446, see also Scheffler 1967, p. 123). While accepting the distinction, Kuhn rejects both choices (Kuhn 1991, p. 114).

[58] A number of critics of SSK claim it ignores epistemic values all together and looks only at non-epistemic values. However, I suspect that these readings are mixing rejection of "truth" talk with rejection of "evidence" talk. For example, see (Sturm and Gigerenzer 2003).

VII. Conclusion

I argue that the Values Distinction is not the right tool to ensure that theory choice involves rational deliberation and is empirically adequate. Scheffler holds that these are two key features of objectivity, and that the Values Distinction is necessary to ensure these features and make science objective. Scheffler charges Kuhn with violating the Values Distinction with the goal of making science appear non-objective.

Kuhn's response and objection to Scheffler is that the Values Distinction is not necessary to make science objective. In fact, values can help in this goal of objectivity. While empirical values do not violate the Values Distinction, non-empirical values do. However, this does not jeopardize empirical adequacy or rational deliberation, since empirical values ensure these features and are central to theory choice.

One might object, noting that Kuhn says gestalt switches are like conversions, which are irrational. However, they are like conversions in that for the individual experiencing them, they are sudden and involve accepting the whole paradigm all at once, not pieces of it at a time. The analogy is not supposed to suggest that these 'shifts' are based on no evidence whatsoever. One might also object, noting that Kuhn *says* paradigm choice is irrational and based on faith. However, that is his view on *pioneers*. That is a domain different from full paradigm choice.

We have clarified one more version of the context distinction, and shown how it has been used to defend objectivity in science. Although the Values Distinction does not succeed as a tool for this particular aim by itself, I did not rule out the possibility that it could potentially be a useful tool when paired with other arguments. Moreover, the Values

Distinction provided a useful contrast to the Psychological Distinction for clarifying how and when scientists show their individuality.

5. Recent Uses: Conflicts over Feminist Epistemology

I. Introduction

In this paper I examine the uses of the context distinction in debates over the legitimacy of feminist epistemology as a field.[59] As we shall see, the meanings and uses of the context distinction have shifted from Hans Reichenbach's initial characterization of it. As outline in Chapter 2, Reichenbach's version of the "context of discovery" refers to the psychological thought process of a scientist developing a hypothesis, and the "context of justification" refers to the rational reconstruction of the evidence for that hypothesis (Reichenbach 1938). For example, German chemist Friedrich Kekulé envisioned the hypothesis that benzene molecules are ring-shaped when he dreamt of a snake biting its own tail. The dream could be considered part of the context of discovery, and any empirical evidence that supported Kekulé's hypothesis would be part of the context of justification.

However, contemporary debates have turned to the normative relationships between political values and rationality or epistemic values.[60] For example, Noretta Koertge uses the context distinction to object to a feminist epistemology approach to philosophy of science on the grounds such an approach violates the context distinction (Koertge 2003). Although Nelson and others have attempted to defend feminist epistemology against this type of charge (Nelson 1995a, Anderson 2004), I believe their responses have not fully captured Koertge's

---

[59] Authors in the debate tend to use "philosophy of science" and "epistemology" interchangeably, although the context is usually within philosophy of science.
[60] See Helen Longino on constitutive and contextual values (1990, 4).

objection. I suggest that their conceptions of the context distinction differ in important respects from Koertge's, which has lead to an obscuring of the underlying issues at debate. Here I attempt to refocus the debate in a more fruitful direction.

Recently, many have turned away from the context distinction, contending that its usefulness has ended. However, it is not so easy to turn away from a philosophical tool that has gotten so much traction in the past. Adherence to some version of the distinctions remains in the minds of many who object to certain philosophical approaches incorporating scientific practice. Teasing out the exact nature of the objections is helpful for addressing them. Moreover, renewed interest in the distinction suggests that we will be hearing more about it once again (Schickore and Steinle 2006). If this is so, then I contend we must proceed with caution. My central thesis is that the context distinction separating discovery from justification has been used in debates about feminist epistemology and scientific practice as a surrogate for underlying disagreements about justification itself. Ambiguous uses of the context distinction mask disagreements about what *kind* of thing scientific justification is (i.e., whether it is a stipulated definition) and how philosophers should determinate that (e.g., through *a priori* means versus through observation of instances of scientific justification). To demonstrate this thesis, I show how Koertge's objections to feminist epistemology are more substantial than they might first appear and can be better appreciated when thought of as a charge of making a *category mistake*. In the end, however, I conclude that one cannot object to a view by saying that it violates the context distinction, since it is often unclear which version of the context distinction is being violated, and at debate is whether such a violation is itself a problem.

In this paper I will proceed as follows. I begin by reviewing Koertge's objection that feminist epistemology conflicts with the context distinction. To motivate her objection, I frame it in terms of the example of Lysenko science (an example she mentions briefly but does not discuss in detail). I then offer an analysis of the example before turning to Lynn Hankinson Nelson's response to the objection. Nelson is part of an effort of Quinean scholars, feminist scientists, and philosophers attempting to reform traditional philosophy of science. Nelson, along with others, especially Richmond Campbell (1998) and (2003), argue that the "core tenets" of traditional philosophy of science, including the context distinction, should be central targets of this reform (Nelson & Nelson 2003). So, Nelson maintains that while the distinction does conflict with her version of feminist epistemology, this is an asset rather than a problem. I then return to Koertge's original objection. Equipped with a more complex understanding Koertge's and Nelson's conceptions of the context distinction, we will now see that Koertge's objection actually lies in a different place than this response acknowledges. Nonetheless, as I will show, Koertge's argument ultimately does not block the feminist epistemology approach, although analysis of the argument leads us to fruitful questions. In particular, uses of "the context of justification" in this debate reveal commitments not only about *what constitutes justification* of scientific claims, but also about *who* should help decide what constitutes justification, and *how* they should do it. These are meta-questions about how we should do philosophy of science. Ultimately, as I will show, the debate is about when philosophers should look to scientific practice, and for what.

II. Koertge: The argument against feminist epistemologies

While many critics dismiss feminist epistemologies as tangential to the traditional focus of philosophy of science,[61] notable exceptions include Susan Haack, Cassandra Pinnick, Robert Almeder, and Noretta Koertge. These authors see the approaches of feminist epistemologists as direct challenges to important and hard-won traditional views on objectivity and bias. Through anthologies such as *Scrutinizing Feminist Epistemology* (2003) and *A House Built on Sand* (1998), these authors respond directly to the writings of feminist epistemologists such as Elizabeth Anderson and Lynn Hankinson Nelson.[62]

Noretta Koertge presents a particularly forceful critique of feminist epistemology. She tends to gather together the views of a variety of feminist epistemologists, and then argues against the approach of feminist epistemology as a whole on the grounds that this approach to science studies conflicts with core tenets of traditional philosophy of science (Koertge 1993, 1996, 2003a, 2003b). Koertge contends that adhering to these core tenets is essential for protecting the integrity of scientific research and guarding against personal and political biases. One of the core tenets Koertge discusses is the distinction between the context of discovery and the context of justification.

Koertge's conception of the "context of discovery" and the "context of justification" differ from Reichenbach's and, as we shall see, from Lynn Hankinson Nelson's conceptions. Koertge describes the context of discovery as the point at which research questions are chosen and hypotheses are formulated; she describes the context of justification as the process of "pursuing a solution to a research problem" (Koertge 1993, 132) or the stage of

[61] See especially Gross and Levitt (1994)
[62] Also Helen Longino (1990), Sandra Harding (1986), Elizabeth Potter (2001), etc.

research at which hypotheses are tested and evaluated.[63] To understand Koertge's use of the distinction, and what is at stake in discussions of the context distinction, I offer the example of Lysenkoism.

Koertge and other critics of feminist epistemology frequently cite the example of Lysenkoism to demonstrate the dangers of violating the context distinction. To reveal Koertge's concerns, I will go into more detail than is typical in these debates, first reviewing the mythical version of the so-called "Lysenko affair," and then later returning with a more nuanced interpretation of the case. A common story about Trofim Lysenko is that he was an under-educated Soviet farmer who allowed his Marxist idealism, rather than experimental evidence and scientific rigor, to rule his scientific theories.[64] Lysenkoism consists of a practical component and a theoretical underpinning. The practical component, which Lysenko dubbed "vernalization" (*iarovizatsiia*), is the claim that if one soaks and chills seeds, one can plant them at untraditional times to yield better harvests (Sheehan 1985, p. 220; Jarovsky 1970, p. 190-197). This led Lysenko to argue for the theoretical component; he claimed that environmental factors, and not self-reproducing genes, are responsible for changes between generations. Promoted by Stalin, Lysenko's vernalization was implemented on a large scale, but never yielded the promised bountiful harvests (despite claims to the contrary). Although it had been previously known that one could affect seeds

---

[63] Although this phrasing might make it sound as though the context of discovery always precedes the context of justification, Koertge acknowledges that in practice the two processes are not necessarily "temporally disjoint"; scientists might be continuously generating, testing, and altering hypotheses (Koertge 1993, 126).

[64] Marxist dialectic materialism emphasized that nature is made up of interrelated, ever changing processes (Sheehan 1985, 38); some see Lysenkoism as more consistent with dialectic materialism than rival views in which individual genes controlled heredity, since genes might be viewed as inappropriately "individualistic" and deterministic (Sheehan 1985, 224). For an insightful critique of this view, see (Jarovsky 1970, Ch. 8).

in various ways by soaking and chilling them, Lysenko's further claim that this leads to larger harvests has never been substantiated. In the end, the typical story goes, Lysenkoism was not only a failed scientific theory, but it was also dangerous. Many people died as a result of lost crops in attempts to follow vernalization; and, more directly, scientists who challenged Lysenko were politically persecuted and sentenced to death. The traditional lesson told along with this myth is that allowing idealism to influence one's science is harmful to society

Using Koertge's characterization of the context distinction, Lysenko's Marxism is part of the context of discovery.[65] Any experimental evidence in support of the theory would be part of the context of justification. By allowing political views to count as evidence for a scientific claim, Lysenko failed to respect the context distinction.[66] He allowed factors relevant only to the context of discovery to count as evidence in the context of justification. Under Koertge's characterization of the context distinction, this would be as if Kekulé cited his dream as evidence for the ring-shape of benzene.

So what does it mean to "respect" or "violate the context distinction"? The Lysenko example can help one understand both what Koertge's endorsement of the context distinction means and also why she sees the distinction as necessary. According to Koertge, historical, political, and personal factors might legitimately influence the context of discovery (where hypotheses are developed). However, different factors should come into play in the context

[65] Characterizing Marxism as a political ideology does not fully capture the role it was playing in Soviet science. It was also seen as a scientific claim with empirical content. I will return to this point below.

[66] The reader might notice here that I am interpreting Koertge as applying the distinction normatively to scientists. In contrast, I read Reichenbach as applying the distinction only to philosophers studying scientists, not to scientists themselves. For Scheffler's view on this connection, see Chapter 4, Section II.

of justification (where hypotheses are tested and arguments are evaluated). In endorsing the distinction, Koertge supports the normative idea that these factors should remain distinct; in particular, political and personal values should not influence questions of justification. Discussing Reichenbach, Koertge writes, "The primary job of his distinction was to *protect* the context of justification *from* extraneous elements" and to "keep epistemology free of historical contingencies." (Koertge 1993, 125-6, emphasis in original). She continues,

> It is rationally permissible to use a motley array of ideas, beliefs, prejudices, interests, and ideologies for heuristic purposes [when developing ideas] but illegitimate to let them enter into the context of justification or evaluation. (Koertge 1993, 126)

Elsewhere, Koertge endorses the context distinction in even stronger language:

> I have argued that there should be no compromise when it comes to the proposal that ideological factors be invited into the context of justification. We should make every attempt to keep politics and religion out of the laboratory. (Koertge 2003b p. 229)

If we recall that for Koertge, the context of justification is the laboratory activity of testing hypotheses, we see that again Koertge argues that ideology and politics cannot be a legitimate part of the context of justification. So to "violate the context distinction," as I call it, is to *allow certain factors from the context of discovery, such as ideology, to influence the context of justification*.[67] Thus, Koertge argues that we should not violate the context distinction.

There are two possible interpretations of Koertge's argument against violating the context distinction. The first interpretation is that when ideology influences justification, it *always and necessarily* leads to science based on "wishful thinking." Wishing thinking occurs when one begins with the desired result and accepts only that evidence which supports

[67] When put this way, we can see that an additional argument is needed to determine *which* factors to keep out of the context of justification. Many have attempted to determine this, including (Longino 1990, Koertge 1996, Kuhn 1977).

it. This is clearly problematic, since selective use of evidence may lead to accepting theories and claims that are empirically inadequate, and I think Koertge is correct when she writes, “No one wants … science devoid of empirical adequacy (remember Lysenko)” (Koertge 2003b, 229). Objections to this stronger argument would rest on finding counterexamples, namely instances where ideological influences in the context of justification lead to empirically adequate theories. Several such examples have been offered in the literature, in many cases based on the idea that such ideological influences are in fact unavoidable, though Koertge and others do not find them convincing (Haraway 1989, Keller 1985, Potter 2001, and Koertge 2003a, Soble 2003, etc.). Below I offer an example from Nelson in which she argues that ideology can even be beneficial.

Under a second, more defensible, interpretation of Koertge’s argument, one would concede that ideological factors do not *necessarily* lead to empirical inadequacy, but would argue instead that they easily could. Thus, the argument would continue, it is better to avoid the risk by excluding ideology altogether. Objections to this argument often rest on risk assessment; there might be situations in which the benefits of including ideology outweigh the risks. The challenge would be to show that there are such benefits and that values are playing an active positive role in evaluating evidence (as opposed to simply not getting in the way, as in the Lysenko example. For examples of such benefits, see Douglas 2000).

The objections are not unfamiliar. What is at issue here is to see how they engage with uses of the context distinction. So far we have used the Lysenko example heuristically to understand what “violating the context distinction” means under Koertge’s view. By examining the mythical account, we have arrived at a better understanding of Koertge’s worries and what is at stake. We see now that the earlier case of Kekulé’s dream is not what concerns Koertge. Kekulé did not cite his dream as evidence for his benzene hypothesis, but rather offered independent evidence. Thus, he was adhering to Koertge's version of the distinction between the context of discovery and the context of justification: he did not apply the a-rational factor used in developing his idea directly to the justification of it. In contrast, according to our story above, *Lysenko violated the context distinction*: his political commitments served as the main evidence for his hypothesis and covered up the lack of experimental rigor; thus his continued support for the hypothesis was unjustified. Koertge is concerned that violating the context distinction by allowing ideology into the context of justification is dangerous; it may lead to wishful thinking, empirically inadequate theories and false claims. At best, we are led away from the truth; at worst people die (crops fail, people starve, dissenters are persecuted). Ideology in the context of justification can be dangerous and we should avoid it.

This is an interesting and important argument. However, on second examination, Lysenkoism no longer seems the best example to make these points. True, the theories and claims are empirically inadequate, and, true, the use of them was dangerous and should have been avoided. However, the problem was not any violation of the context distinction; the problem was not that ideology obscured the lack of evidence (as we shall see, it did not). Rather, the problem was that Lysenko made no attempt to get evidence whatsoever, regardless of his ideology.

To see this, we need to make two corrections to the traditional story. The first is that coherence with Marxism (specifically, dialectic materialism) was not a requirement for just Lysenko, but rather was a common requirement for Soviet biologists (Jarovsky 1970, 232-236; Graham 1993). Marxism operated as a framework assumption from which many theories emerged and were judged.[68] As a shared worldview, it was assumed by legitimate scientists as well, not just "cranks" like Lysenko and Michurin.[69] For example, Aleksandr Oparin, the prominent biochemist who studied the origins of life and the "primordial soup," incorporated Marxism into his later work. In particular, Oparin built on the Marxist view that "the evolution of life passes through several 'levels of being' that were necessary for its origin;" he studied these levels (from "no life" to "microorganisms" to "complex organisms"), viewing them as sequential and non-repeatable (Graham1993, 110).[70] This raises two problems for a Koertge style interpretation of the Lysenko affair. First, the use of Marxist ideology as a framework assumption does not distinguish the empirically adequate sciences from the empirically inadequate sciences, so it cannot be used to explain the differences between them.[71] Second, it is not clear that when ideology operates as a framework assumption it is still operating as evidence in the context of justification. The Lysenko case leads to philosophically interesting questions: "How should we adjudicate between different assumptions, especially ones shared by an entire epistemic community? Are some framework assumptions illegitimate, and how can we tell which ones?" These are important questions, and, though related, they are not clearly the same as "Should some kinds

[68] For more on framework assumptions, see (Putnam 1962a) and (Putnam 1962b). For a nice discussion of Putnam's views, see (Mueller and Fine 2004).
[69] Part of why Marxist ideology was able to be so versatile is that it was so vague. See (Jarovsky 1970, 234).
[70] Graham distinguishes between "authentic" and "calcified" dialectic materialism (1993, 119-121). I contend that ideology is problematic primarily when it is rigidly "calcified" and used by political entities to suppress and persecute those who work outside of a (narrow version) of the framework. Thus, the problem lies with political oppression, not the ideology itself.
[71] Graham makes this point himself, after receiving criticism for linking both *eminen*t and *disreputable* scientists with Marxism (1998, 7 & 26).

of evidence be excluded from the context of justification?" Thus we have moved away from connecting ideology with the *context distinction*.

So the first correction of the Lysenko myth is that Marxist ideology was *a framework assumption commonly shared* by the scientific community; it was not *evidence* used by *just* Lysenko and other "cranks." The second correction builds on the first: Marxism was not Lysenko's motivation for his scientific claims. Historian David Jarovsky proposes that although Lysenko was partly motivated to be consistent with Marxist dialectic materialism, Lysenko was motivated as much by the desire to produce immediate practical results. Improving crop yield was a top priority of the Soviet government (Jarovsky 1970, p. 60, 91). By promising results on the time scale of 3-5 years instead of the typical 10-15 years, Lysenko was able to create a name for himself. Such promises were based on an explicit rejection of the lengthy verification processes that other scientists "foolishly" required. For example, Lysenko is attributed with saying, "In order to obtain a certain result, you must want to obtain precisely that result .... I need only such people as will obtain the results I need." (Sheehen, 1985, p 223). By itself, the practical orientation might not be problematic, but coupled with a disdain for gathering evidence in the traditional sense, we have a situation in which Lysenko is selectively choosing the evidence that will help him reach his goal.

At first this might appear to be exactly what Koertge is concerned about. Wishful thinking leads to empirically inadequate and dangerous science. However, according to my interpretation of Koertge, her charge is that *ideology leads to wishful thinking*, which leads to inadequate science. Although ideology was a factor in the Lysenko case, it was not the mechanism by which the science was distorted.

Thus, from the perspective of some feminist epistemologists, both Kekulé and Lysenko represent extreme cases. Kekulé's dream is extreme for being so benign, whereas Lysenko's science was particularly harmful. Moreover, it is not clear that the *problems* with Lysenkoism come from his violation of the context distinction rather than from other features. Although allowing ideological factors to inform justification might sometimes lead to lack of empirical rigor and empirical adequacy, it is not clear that they will always do so and they might even lead to benefits.

Now we have arrived at the connection between Koertge's views on the context distinction and feminist epistemology.[72] Feminists such as Nelson draw our attention to those cases where violating the context distinction does not lead to these problems, but instead enhances empirical rigor. These tend to be the ambiguous middle cases, rather than the extremes. For example, Nelson (1995b) discusses research on hormones in rats and cognitive abilities in humans. As I will discuss in more detail below, assumptions about biological connections between gender and sex have influenced how *both researchers and critics* interpret the data. Thus, Nelson rejects the claim that certain factors in the context of discovery should be irrelevant to the context of justification.

This is what Koertge objects to. Koertge argues that feminist epistemologists mistakenly propose that ideology should indeed guide the context of justification. She quotes Helen Longino as writing, "I am suggesting that a feminist scientific practice admits political considerations as relevant constraints on reasoning" and "when faced with a conflict between

---

[72] As mentioned earlier, Koertge tends to treat all feminist epistemologies as the same. There are, however, significant differences between views. For simplicity, I will consider objections and responses to features of Lynn Hankinson Nelson's views.

these [political] commitments and a particular model of brain-behavior relationship we allow the political commitments to guide the choice" (Longino 1990, 191-3).

To Koertge, this sounds entirely too close to the myth of Lysenkoism and to rejecting the context distinction: "The later remark hints that the [feminist] might also wish to interject politically progressive values into the context of justification [and not just into the context of discovery]" (Koertge 2003b, 227).[73] Thus, Koertge concludes, since feminist views of science conflict with core traditional views of science, including the context distinction, feminist philosophy of science is misguided. The argument from Koertge could be formalized in the following way:

Argument against Feminist Epistemology (FE):
1. FE conflicts with the context distinction.
2. The context distinction is a core tenet of philosophy of science.
3. Views that conflict with core tenets of philosophy of science should be rejected.
C. Thus, we should reject FE.

Koertge is not alone in offering this objection to a feminist epistemology approach. Haack, for example, argues that traditional epistemologists have not properly engaged with the ideas coming out of feminist epistemology. She writes, "My colleagues in the epistemology mainstream mostly hope that, if they ignore it, feminist epistemology will go away" (Haack 1993, p.557). She recognizes that feminist epistemologists engage with central philosophical questions and that more traditional philosophers should be responding to these challenges. However, instead of seeing the feminist authors as making useful contributions to mainstream discussions, Haack finds the views to be problematic and

[73] Koertge is opposed to using ideology in the context of discovery, as well, if it will limit the kinds of questions scientists are allowed to pursue (Koertge 2003b p. 226, 227, 233), so it is not clear that she would find Anderson's response (below) convincing.

potentially dangerous. The feminist epistemology approach should be responded to and refuted, according to Haack, to prevent it from opening the door for biased and unwarranted scientific claims (Haack 1993, p. 564). Feminist epistemologists wanting to respond to Haack, then, would need to demonstrate that the views are not dangerous (i.e., do not promote "wishful thinking" and biased science) in the ways that critics like Haack contend. Moreover, they would need to show that the views are potentially useful and so are worth discussing in a more general setting. Indeed, these are precisely the arguments that feminist epistemologists have been making for years.

III. Responses to the Argument

There are at least three possible reactions a feminist epistemologist could have to Koertge and Haack's critique. One could argue that Premise 1 is false: feminist epistemology does not violate the context distinction and core tenets of philosophy of science; while it addresses central questions, feminist epistemology supplements, rather than challenges, traditional answers to those questions. This response would endorse the context distinction by supporting the claim that there are factors in the context of discovery that should have no bearing on questions of justification. Alternatively, one could accept Premise 1 and concede that feminist epistemology is a radical break from traditional philosophy of science. One would then reject Premise 3; the context distinction is misleading, for example, because there is no clear-cut distinction between this version of discovery and justification, so conflict with the distinction is no reason to reject feminist epistemology. A final strategy could be to agree with all of the premises, in particular with an endorsement of the context distinction and core tenets, and so agree to adjust one's version of feminist epistemology to make it more compatible with those commitments. I contend that Elizabeth Anderson employs this first strategy (1995a), and arguable Louise Antony (2002) follows the third strategy, though I will not be able to examine those accounts here.[74] As I will show below, Nelson employs the second strategy.

Strategy 2 (Nelson): Feminist Epistemology is new and important

In several places, Lynn Hankinson Nelson (1990, 1994) maintains that feminist epistemology in general ought to be taken more seriously by mainstream philosophers of science and epistemologists, contending that her views address precisely the issues that mainstream philosophers are concerned with. Whereas other strategies minimize the differences between mainstream and feminist epistemologies, sometimes by suggesting they are asking different question, I suggest reading Nelson as taking the opposite position. Nelson maintains that her view addresses the same questions about how we know things, and so she *emphasizes* the differences between the two general approaches with the aim of showing that many versions of feminist epistemology offer important new insights on those questions (Nelson 1990, 1994).

It is important to note that although Koertge's charge is meant to apply to many different versions of feminist epistemology at once, the different versions are so diverse that it is not clear a single defense would suffice. Thus, the rest of this chapter will necessarily focus on Nelson's own version of feminist epistemology.

Nelson embraces Koertge's claim that the context distinction contradicts and precludes the feminist epistemology approach. Nelson describes the distinction as that

[74] Although an impressive work of reconciliation, I think Antony's response ultimately fails to capture the full force of some of Anderson's and Nelson's worries. For a critique of Antony, see especially (Campbell 1998).

between "questions concerning evidence, justification and warrantablility" (context of justification) and "questions concerning the material, historical, and cultural circumstances of cognitive agents and their interests" (context of discovery) (Nelson1995a, 42). In Nelson's version of feminist epistemology, evaluating the justification of scientific claims requires taking into account the cultural and political circumstances in which those claims are made (Nelson 1995a, 42). According to Nelson's holism, if each claim is part of a complex web of beliefs, then no single claim can be evaluated in isolation. Auxiliary assumptions, many of which are value claims, must also be considered.

Let me explore that example in more detail here. Nelson (1995b) discusses the case of Diamond et al. (1981), who showed that in the brains of male rats, but not female rats, parts of the right cortex are slightly thicker than the corresponding areas of the left cortex. Geschwind and Behan (1982, 1984) hypothesized that these changes are due to differences of *in utero* testosterone levels, and that these changes could account for statistical disparities between men and women regarding immune systems responses, left- or right-handedness, and mathematical abilities.

The political climate of emphasis on gender equality that led researchers in the 1980s to investigate apparent sex-differences is part of the context of discovery. *Whether these hypotheses are supported by the evidence* would be a part of the context of justification, according to Koertge's description of the distinction. Yet, Nelson argues, determining what counts as evidence rests on auxiliary assumptions about male/female dichotomies and the connection between hormone levels and gender, the brains of rats and humans, and standardized tests and actual cognitive abilities (Nelson 1995b, 414). Feminists might criticize each of these assumptions. For example, implicit political commitments to gender dichotomies might lead researchers to categorize people primarily by gender rather than by the more specific traits being studied (e.g., testosterone levels), on the perhaps mistaken assumption that the two traits are reliably correlated. That is to say, when a person self-identifies as male on a standardized test, that is not necessarily an indication of that person's *in utero* testosterone levels relative to others who self-identify as female. This could be due to a variety of factors: transgender individuals' identities have shifted since their birth; there might be greater variations of testosterone levels among individual females and males than can be captured by grouping them together by gender; and so on. As Nelson writes,

> The evidence for these critiques, like that for the hypothesis with which we began, encompasses a broad body of experimental results, current hypotheses, and theory – elements of which, like the research feminists criticize, are substantively informed by sociopolitical context and values. (Nelson 1995 b, 414).

According to Nelson, both the original research and the critiques of it are embedded in correspondingly different systems of social values. For the researchers, in Nelson's view of the context distinction, the social values that contribute to the context of discovery (deciding which questions to pursue) are inextricably linked with the context of justification (determining what kind of data should count as evidence). For feminists who critique the researches, their values are also integrated into the context of justification: their social values are grounded in challenges to traditional views about gender and sex, which will lead them to count different kinds of data as evidence.

Interestingly, Nelson does not claim that one group conducted "bad science" and the other "good science" (Nelson 1995b, 411, 414, 417). Just as ideology can lead to both good and bad science (as we saw in the Lysenko example), Nelson contends that *problematic or sexist ideology* can even lead to *good* science. So in this case the ideology which concerns

Koertge is not a determining factor. For example, one implication of the hormone example is that experimental evidence and empirical data may support a hypothesis when placed against one set of culturally embedded assumptions, and offer less support for the same hypothesis when placed against a different set of assumptions. Nelson writes,

> Judged against then current research questions and traditions, accepted theories and methods, and experimental results in endocrinology and empirical psychology, the hypothesis that testosterone causes right-hemisphere dominance enjoyed *considerable* evidential warrant… Considered in the light of the critiques advanced by feminist scientists, the assumption of a hormonal basis for sex-differentiated lateralization and the specific hypothesis we have considered are revealed to be substantially *less warranted* than when judged without benefit of these more exacting critiques. (Nelson 1995b, 417, emphasis in original)

Two groups with different assumptions can look at the same evidence, draw different conclusions, and both be doing "good science."[75] Thus, Nelson views historical and political factors as relevant to whether a claim is warranted. Since no one can escape having a sociopolitical perspective, issues within the "context of discovery" are relevant to the "context of justification." This means that Nelson endorses a violation of this version of the context distinction.

Moreover, according to Nelson, accepting the distinction would force one to conclude that feminist work falls outside of epistemology. That is because endorsing the distinction in the way I have outlined above entails viewing the social contexts of knowers as outside of justification:

---

[75] To some readers, this move may appear to expose Nelson's view to the charge of relativism, a charge that Anderson's view can avoid. Nelson emphatically wishes to reject relativism, and argues that her emphasis on empirical adequacy and reliability keeps her holism from becoming relativistic (Nelson 1990, 40 and 295). Unfortunately, I cannot survey those arguments here. Suffice to say that this criticism of Nelson's feminist epistemology is distinct from the criticism considered in this paper.

> If we recognize such a distinction and assume that epistemology is concerned only with questions of the first group [justification], then arguably feminist analyses that raise questions of the second group [discovery] are outside the domain of epistemology. The question I am raising is whether feminists should grant this distinction. (Nelson 1995a, 42)

Thus, Nelson endorses Koertge's Premise 1 that feminist epistemology is in conflict with one of the core tenets of traditional epistemology and philosophy of science. Instead, Nelson argues against Premise 3: *Views that conflict with the core tenets of philosophy of science should be rejected.* Since Nelson contends that feminist work falls clearly *within* epistemology, she must find a way to reject the tenet of the context distinction. Nelson argues that to use the context distinction to exclude social circumstances from accounts of justification is to beg the question against the feminist epistemologists.

> A *naturalistic* philosophy of science must allow the details of individual episodes to indicate, what, if any, such factors were of import, in what ways, and to what degree – not adopt methodological principles that *prejudge* this issue. (Nelson 1995b, 409)

That is, critics of feminist epistemology assume an answer to the very question being debated: *What constitutes justification and should we determine that question theoretically or by looking at the circumstances of actual scientific practitioners?*

IV. Possible Objection: violating the context distinction commits a category mistake

This charge of begging the question is at the heart of Nelson's defense of feminist epistemology and at the crux of our rhetorical analysis of the context distinction. How could a critic like Koertge respond to this charge? We get a clue from Nelson, who writes,

> Some scientists and philosophers have known there is a conversation about "gender and science" going on, but their views about science, and specifically their understanding of empiricist tenets and of current empiricist accounts of science, have

> led them to think there is something akin to a category mistake at work. (Nelson 1990, 4)

Although Nelson quickly moves on to other reasons many feminist epistemologies have been marginalized, I think that talking about a *category mistake* is exactly the right way to think about Koertge's use of the context distinction. If Nelson had indeed made a category mistake in allowing factors of the context of discovery to affect the context of justification, then Koertge's argument against Nelson would not be begging the question after all, since Koertge would have an independent reason to support the context distinction. On the other hand, if Nelson does not make a category mistake, then this part of Koertge's argument loses force.

There are two ways of describing category mistakes (Meiland 2001). Under one description, the arguer conceptually places the entity in one category when it actually, and *necessarily,* belongs in another category. Gilbert Ryle offers the example of team spirit: a person observing cricket for the first time will learn that players in different positions (the bowler, the batsmen, the fielders, etc.) perform different tasks (Ryle 1949). The novice then might ask who performs the task of exhibiting team spirit. This question would be based on a misunderstanding of the game. Exhibiting team spirit is not a task assigned to one position. Rather, a player exhibits team spirit in *the way he performs* his task, e.g., with graciousness, or with attention to the skills and needs of fellow players. Thus, the novice mistakenly places "team-spirit" in the category of *being a position in a cricket team*, when in fact it belongs in a different category, namely *the manner in which one performs one's task.*

One could also describe category mistakes as mistakes about *properties*, not just about *categories*. It is a simple mistake to attribute a property to an entity that it *contingently* happens to not have. In contrast, to make a category mistake is to attribute a property to an entity that it *necessarily* cannot have (Meiland 2001). For example, arguments can be valid or invalid, but they cannot be true and false. It is appropriate to refer to a person's walk as swift or slow, but not to refer to it as "orange" or "yellow" (at least, not literally). This is not because the walk is a different color (mauve, perhaps), but because ways of walking are actions, and actions do not have color except in a metaphorical sense.

Using distinctions to highlight mistakes is common in philosophical debates about confusing or abstract topics. Amie Thomasson notes that Ryle, Russell and others have often used the idea of category mistake "as a way of exposing, avoiding, or dissolving various presumed philosophical mistakes, confusions, and paradoxes" (Thomasson 2008, sec. 2.1). Thus, Koertge's use of the context distinction against feminist epistemology can be seen as part of a larger tradition of using the distinction. Although Koertge does not use the language of category mistakes, she does suggest that justification has certain properties that Lynn Hankinson Nelson is not recognizing. Koertge writes,

> It is tempting to throw social values into the grab bag of desiderata that guide all aspects of scientific decision making. *But to do so is to jeopardize seriously the very features of science that make it so valuable in the first place.*
> We want scientific results that have withstood the highest levels of empirical criticism and theoretical scrutiny. When political considerations limit the questions that can be raised, the hypotheses that can be tested, or the alternative explanations that can be brought forward, that area of inquiry ceases to have scientific value, regardless of whether the political motivations are good or bad. Scientific norms are not negotiable, and scientific values are not fungible. (Koertge 2003b, 233, emphasis added)

Koertge claims that those who violate the context distinction are taken to have attributed properties (e.g., being able to legitimately influence judgments of justification) to things (e.g., non-cognitive values) that cannot have those properties. The alleged mistake is to discuss the political aspects of legitimate scientific justification when legitimate scientific justification is precisely the kind of thing that could not have political aspects, and conversely political values are simply not the *kind* of thing that creates better scientific inquiry – such values can only detract from, obscure, and limit knowledge production. Koertge worries that scientific results could be negatively "tainted with the ideological biases of [sexist] scientists" and she agrees with feminists that this is something we have to be on guard against. She is then incredulous when faced with the apparent call to "inject *more* ideology into science" (Koertge 2003b, 225).[76] To Koertge, this probably sounds as though feminists are inadvertently endorsing Lysenko-style science (Koertge 2003b, 226) in which a politically motivated government dictates which scientific results will be acceptable; and where wishful thinking, rather than empirical adequacy, guides which hypotheses we accept.[77]

We can now return to a Koertge quote we saw earlier:

> …it is rationally permissible to use a motley array of ideas, beliefs, prejudices, interests, and ideologies for heuristic purposes [when developing ideas] but illegitimate to let them enter into the context of justification or evaluation. (Koertge 1993, 126)

For Koertge, ideology, whether based in good or bad values, can only lead one astray from empirical adequacy.[78] I suggest that for Koertge the reason this "motley array of ideas" is

---

[76] Elsewhere, Koertge labels this view the *Hair of the Dog 'Cure.'* "I have several objections to … the idea that the best cure for the ideological hangover is 'the hair of the dog that bit you'" (Koertge 1993, 128).
[77] Pinnick expresses similar worries, making reference to "politically motivated science, such as Shockley's eugenic or Brigham's and Grant's aptitude- and intelligence-test design" (Pinnick 2003, 22).
[78] Koertge offers a particularly vivid example of the dangers here: "The assessment of the probability that the O-ring on the Challenger will fail must be independent of how personally, politically or financially undesirable the result of that assessment will be"

properly considered part of the context of discovery but not the context of justification is because it is not the kind of thing that can legitimately influence evaluations of scientific claims. Just as asking "What position in cricket is responsible for team-spirit?" is based on a misunderstanding of the nature of team-spirit, so would allowing ideology into justification be based on a misunderstanding of what justification consists of.

Thus it might seem that we have uncovered the heart of the disagreement. Allegedly, Nelson commits a category mistake: ideological factors do not have the properties of enhancing justification; they can only detract from it. Presumably no further debate is needed. I contend that this is a significant objection, and that it has been obscured by ambiguities of the context distinction terminology, as well as by the more recent tendency in philosophy to turn away from discussions of the context distinction. However, now that we have uncovered this objection, there remain two possible responses to it.

First, whether any given distinction correctly captures the nature of the categories is a matter of debate itself. Speaking generally about the philosophical concept of *category* as well as about individual episodes of category mistakes, Thomasson reminds us that those who charge others of making category mistakes must offer reasons for deciding what belongs in which category. Such authors,

> owe an account of the conditions under which we can legitimately claim that two entities, concepts, or terms are of different categories, so that we know when a category mistake is (and is not) being made. Otherwise, they would face the charge of arbitrariness or *ad hocery* in views about which categories there are or where category differences lie. Yet there is little more agreement about the proper criteria for distinguishing categories than there is about the categories themselves. (Thomasson 2008, 2.2)

(Koertge 1995, 134). I suspect that Nelson and Anderson would both agree that this kind of "wishful thinking" should be avoided.

Given the difficulties of developing an account of the categories, it seems that distinctions such as the context distinction cannot stand in for arguments. Distinctions can be useful heuristic devices when they point out inconsistencies with commonly shared beliefs. However, when the legitimacy of a distinction is itself at debate, the distinction cannot serve as an independent reason. If we do not agree about which entities can have certain properties, then pointing to an alleged category mistake will fail to be convincing. In this case, there is disagreement over whether a thing (such as a political value) can have a certain property (such as the ability to enhance evidential warrant as well as detract from it), with the feminist epistemologists arguing that it can. Thus, to dismiss feminist epistemology on the basis that it violates the context distinction is, as Nelson argues, begging the question. Other arguments might be used against feminist epistemology, most notably that Nelson's view is open to the charge of relativism,[79] but that it violates the context distinction cannot be a reason to reject the feminist approach.

Second, while Koertge's objection falls within a tradition of pointing to category mistakes to dissolve apparent puzzles, she nonetheless uses the distinction in a non-traditional way here. Ryle uses categories to open up room for more possible arguments. He demonstrates how the Cartesian approach assumes minds are analogous to material bodies and so minds must have corresponding (yet mysteriously non-material) structures and causal interactions (Ryle 2000, 18). Once we recognize this assumption, we can explore alternatives to it without abandoning it outright. In contrast, in arguments against feminist epistemology, employing the context distinction closes down conceptual space, curtailing the number and types of arguments in play. One must offer more reasons to justify closing down

[79] See footnote 17.

possibilities than for opening them up, since closing down conceptual space asserts a final word on claims that are still being debated. Ryle does not make any claims when suggesting that we explore alternatives to the Cartesian assumptions (the claims come later, when he tries to justify those alternatives). In contrast, by rejecting inquiries into ideological influences on justification from the start, Koertge does make a claim about the nature of justification. Thus, this use of the context distinction is susceptible to the charge of begging the question in a way that Ryle's use of distinctions is not; Koertge's use of category mistakes really is an instance of begging the question after all.

V. Implications for Philosophy of Science

The context distinction is entangled with many issues, including bias, "wishful thinking" science, values, and application. Debates about these issues are on-going within much of the feminist literature; here I have argued that the ambiguity of the context distinction obscures rather than reveals the connections between them. In this section I will point to just a few of the possible questions that our discussion of the context distinction has brought to the forefront.

The first questions, which I will discuss in more detail than the others, are about the nature of justification. *Is justification an a priori concept/stipulation? Or are the features of justification something that we discover while engaging in epistemic practices?* Koertge's use of the context distinction seems to be an extension of her Popperian commitment to universal accounts of justification. In the Popperian approach, one develops a general account of standards of practice (e.g., Popper's model of falsifiability) and then turns to historical episodes in science to see how well they satisfy those standards. Nelson has noted this feature, and referred to such accounts of justification as “extra-scientific” or *a priori*. (Nelson 1995b, 401). However, while it is true that in Popper’s account of justification, a normative methodology descends from the outside to evaluate any particular episode in science, the procedure he used to create his normative methodology was not entirely *a priori*. Popper began by identifying both positive and negative exemplars of scientific methodology (e.g., Freud’s psycho-analysis and Einstein’s relativity) and then extracted from them features of justification.[80] Thus we see that while Koertge’s Popperian approach has some elements of an extra-scientific, *a priori* account of justification, that account was itself derived in part from actual scientific practice.

Nelson incorporates scientific practice on a different level and in a different way from Popper and Koertge. Rebecca Kukla puts it this way:

> The best way for philosophers of science to understand how epistemic practices succeed or fail … is not to come up with a philosophical theory … and then measure practices against it, but rather to look at actual, concrete epistemic practices engaged in by natural beings at particular historical moments and *extract an understanding of the normative ideals … that these practices strive to embody*. (Kukla 2008, 289, emphasis added)

Such a philosopher of science would look to episodes of science to see justification practices in action. Although to “extract … normative ideals” seems similar to Popper’s construction of falsificationism described above, in fact this approach stops short of constructing an “extra-scientific” normative methodology out of these ideals. Rather, episodes of science are incorporated into evolving normativities. According to Kukla, “Standards of accountability

---

[80] Popper writes, “It was the summer of 1919 that I began to feel more and more dissatisfied with these three theories—the Marxist theory of history, psycho-analysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps first took the simple form, ‘What is wrong with Marxism, psycho-analysis, and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?’” (Popper 1963, 34).

to reality emerge bottom-up out of the micropractices of epistemic labour, rather than controlling such micro-practices top-down" (Kukla 2008, 289). Rather than developing one universal account of justification, such feminists argue that from different situations there might emerge changing accounts of justification and that these accounts cannot always be determined before hand.

Other questions revealed by our discussion of the context distinction include:

- To what extent should philosophers look to actual scientific practice to inform our conception of justification?
- Are some framework assumptions better than others? If so, what principles can we use to determine which ones?
- Can ideology (as well as non-epistemic, non-cognitive, or political values) enhance justification, or only detract from it? If the later, on what principled basis do we distinguish between, say, epistemic and non-epistemic values?

These questions are not new – they have been at the heart of the debates in feminist epistemology and of many debates in philosophy of science. I maintain that using the versions of the context distinction discussed here obscures these differences, making debate between groups with very different assumptions even more difficult.

VI. Conclusion

As we have seen, the distinction between the context of discovery and the context of justification has been used to object to an entire approach to doing philosophy of science. Koertge, Haack and others have objected to the proposal that we allow "political ideology" to influence questions of evidence and warrant on the grounds that doing so violates the distinction between discovery and justification, and opens the door for science based on wishful thinking and empirical inadequacy.

I argue that while this objection has not been fully appreciated, it nonetheless does not succeed. First, by looking more carefully at Lysenkoism and examples of hormone studies, we see that, according to one type of feminist epistemology, ideology does not always distinguish empirically inadequate science from empirically adequate science, and indeed ideology can sometimes lead to greater empirical adequacy.

Moreover, the concept of "context of discovery versus context of justification" is different for different players in the debate. This ambiguity about the context of discovery and the context of justification leads to confusion and allows for question begging. The context distinction is not being used simply to point out various stages in inquiry, as it first appears (i.e., in one stage, socially relevant questions are chosen and hypotheses are formulated; in another stage, they are tested). Rather, Koertge and others use the distinction as a way of pointing out an alleged category mistake: ideological factors are inadmissible as evidence when evaluating a hypothesis, since such factors are not *the kind of thing* that can enhance justification. Now that we see this as the central issue, we can see as well that this conception of justification is precisely what is being debated.

Chapter 6: Conclusion

Let us learn to dream, gentlemen, then perhaps we shall find the truth. But let us beware of publishing our dreams till they have been tested by the waking understanding.

- Friedrich Kekulé, 1890

In this project I have argued for four claims. First, I suggest that we do not approach the topic by looking for one single best version of the context distinction. Rather than singling out one of the many ambiguous uses as more true or accepted by more people, I look at how each version can be useful. Second, building off the first point, I have shown that if we consider versions of the context distinction as tools, then we can evaluate them with respect to the aims for which they are being used. For instance, is a given distinction used as evidence in an argument or to provide a useful illustration? We can then evaluate the usefulness of the context distinction by determining if it succeeds in these tasks, and whether it eases discussion or makes it more confusing for any given debate. Third, many versions of the context distinction are independent of one another. There are a few interesting exceptions, most notably the Empirical/Logical Distinction, but if you remove that version, then most of the remaining versions are independent. That is, someone can use one version without being committed to another. Finally, ambiguous uses of the distinction that shift from one version to another mask underlying disagreements. Often in philosophy, clarifying ambiguities can resolve disputes by revealing areas of underlying agreement; however, in the case of the context distinction, clarifying ambiguities often reveals areas of disagreement, instead. Thus, this work of clarifying does not resolve debates; however, it does allow stalled-out debates to continue in more fruitful directions.

I. Versions of the Distinction

The distinction between the context of discovery and the context of justification has been presented as a logical distinction (Reichenbach, Popper), as a claim with truth-value (Koertge, Pinnick), and as a perspective from which to ask questions (Hoyningen-Huene, Gigerenzer and Sturm). Finally, Kuhn has suggested that the distinction is a framework assumption of an outdated philosophical paradigm, and as such should be rejected.

Not only has the distinction been characterized as several different kinds of things, it has also taken on several meanings. In the Introduction, we saw eight versions of the context distinction, as identified by Paul Hoyningen-Huene. During the course of the project, I have identified at least five more. These versions include distinctions between:

Identified by Hoyningen-Huene:

a) Temporal Processes	Discovery happens first, and is followed by justification.
b) Factual/Normative Processes	Discovery follows some historical process which can be described, where as justification involves normative evaluation.
c) Empirical/Logical Factors	Analysis of discovery is empirical, whereas what counts as justification is solely a question of logical reasoning, which is independent of time.

d) The Fields of History, Psychology, Sociology / Philosophy	Historians etc. are concerned with discovery, while philosophers are concerned with justification.
e) Types of Questions	Questions such as “Can a statement be justified?” are of a different character than other types of questions such as “What happened during the discovery of a particular statement?”.
f) External/Internal Factors	The context distinction has some similarities with the distinction in history between factors that relate to the historical development of a theory, and factors that relate to its content.
g) Descriptive/Normative	The context of discovery is really a perspective from which to ask descriptive questions, while the context of justification is a perspective from which to ask normative questions. (Called the “core” or “Lean DJ”. Hoyningen-Huene rejects the other versions in favor of this one).

In addition, I have identified[81]*:*

i) Is/Ought:	Just because something has been done a certain way is not itself a reason to keep doing it that way. (Is/Ought Distinction)
j) History/Philosophy:	Philosophers should not use the History of Science as evidence for a scientific methodology. It should be used only for illustration or inspiration. (Historical Evidence Distinction)
k) Thought process/Justification:	The thought processes leading a scientist to an idea are irrelevant to the justification of that idea. (Psychological Distinction)
l) Historical setting/Justification:	The historical setting of a scientist is irrelevant to the justification of their ideas. (Historical context Distinction)

[81] Some of the uses I identify are more specific variations of the ones Hoyningen-Huene identifies. Providing more specific variations allows one to engage more critically than one can with a general or vague version. There are also countless minor variations still to be found in the literature.

m) Values/Justification: The political, personal values of a scientist are irrelevant to the justification of their ideas. (Values Distinction)

This dizzying array of uses is reminiscent of Margaret Masterman's identification of 21 uses of the word "paradigm" in Kuhn's *Structure of Scientific Revolutions* (Masterman 1966). There, Masterman was able to distill these uses of 'paradigm' down to a handful of key concepts. Many philosophers have tried a related technique here of identifying one version of the context distinction as the single best version. For instance, Hoyningen-Huene has suggested that the core of the distinction is between descriptive and normative perspectives from which to ask questions. Looking for a best version is the typical approach to debates about the context distinction. However, given the continuing confusion on the topic, I suggest that this technique has not been as successful as hoped. Although I do not rule out the possibility that a single best version of the context distinction might be found, I take a different approach. Rather than asking if each version of the distinction offers true or accurate claims, I suggest that we regard each as a tool and ask whether it has been helpful in achieving the goals to which it has been put. In this manner, we might find some versions to be better or more useful than others, but it makes less sense to ask which is the one "right" version.

II. Tools for use

For instance, Hans Reichenbach used the Psychological Distinction to argue for the Historical Evidence Distinction. In Chapter 2, I show how he distinguishes between the thought processes of a scientist thinking up a hypothesis and the Rational Reconstruction of the scientist's argument for that hypothesis. Reichenbach concludes that it is no objection to his methodology if a scientist did not actually follow it when pursuing a hypothesis – all that matters is that when rationally reconstructed, the argument for the hypothesis follows Reichenbach's methodology.

Israel Scheffler uses the Values Distinction to argue that Thomas Kuhn's proposed methodology makes science look non-objective because Kuhn allows non-epistemic values to play a role in theory choice. However, in Chapter 4, I argue that for two of the features of objectivity that Scheffler is concerned with, it is not clear that the Values Distinction is useful, since non-epistemic values can arguably facilitate empirical adequacy and rational deliberation.

The context distinction has also been used to block certain approaches to doing philosophy of science more generally. The Historical Evidence Distinction has been used to argue that philosophers of science should focus on logic and not scientific practice, and the Values Distinction has been used to argue that feminist approaches to philosophy of science have made a mistake by incorporating values into the analysis of science. In Chapter 5, I discuss one particular argument to block values in science, and I argue that it remains unconvincing to those who disagree, since it begs the question at issue. Noretta Koertge charges feminist philosophers of science such as Lynn Hankinson Nelson with violating the context distinction when Nelson argues that values are an important feature of justification. Nelson accepts the charge, but does not see this as a problem. I highlight key features of Koertge's argument to show that she has two concerns; one is practical and the other is logical. Like Siegel, Koertge is concerned that violating the context distinction erodes objectivity; in particular, she is concerned that it will lead to wishful thinking and unreliable scientific claims. Like Kuhn in Chapter 4, Nelson defends against this by arguing that violating the Values Distinction and explicitly incorporating values into justification actually makes the process more reliable, not less. I will return to the logical argument below.

III. Independence

We have seen that there are many different versions of the context distinction, and that they have been used for different purposes. If these versions were directly related to each other, either because they essentially meant the same thing, or because one logically entailed another, then having these different versions might not be a problem. However, I argue that they are at most indirectly related, and sometimes independent of each other. For instance, the Temporal Distinction and the Historical Evidence Distinction are not directly related: any attempt to divide scientific research into exploratory and justification stages is only indirectly related to the claim that history should not be used as evidence when specifying a methodology for science. Any connection would need to be argued for, and not simply asserted.

Kuhn is often credited with challenging the whole context distinction, while Hanson is credited with arguing that discovery has a logic to it and so is also worthy of philosophical attention. However, in Chapter 3 I show how Kuhn actually accepts the Psychological Distinction, and much of the Historical context Distinction, while rejecting the Values Distinction. That is, Kuhn accepts that for periods of normal science, when a scientist is working within a set paradigm to solve a puzzle, any attempt to evaluate the solution does not need to involve the steps she took to find the solution, what her thought processes were, or the historical or political setting of the generation and testing of the solution. All that is irrelevant to the evaluation of the solution itself. So far this looks very much like the view of Kuhn's critics. It is when it comes to paradigm shifts and choosing between paradigms or theories that Kuhn's view differs from his critics with regard to the context distinction. Here, Kuhn argues that epistemic values such as empirical adequacy, simplicity, coherence, and fruitfulness are insufficient by themselves for choosing between paradigms. To make a rational choice, one must invoke personal and political values to determine how to apply and evaluate these epistemic criteria. Thus, for periods of paradigm shift, Kuhn rejects the Values Distinction. I also show how Kuhn accepts the Is/Ought Distinction while possibly rejecting the Historical Evidence Distinction. All together, this demonstrates how Kuhn accepts some versions of the context distinction as useful for answering some questions, and he rejects other versions of the context distinction as unhelpful for answering other questions. Thus, in Kuhn's work, the versions of the context distinction are independent of each other.

In another example, Hanson is often identified as rejecting the Fields of Study Distinction, since he claims that philosophers *should* investigate discovery after all and that discovery has a logic to it. However, much depends on his conception of a "discovery." In Chapter 1, I review how Hanson agrees there is no logic to the initial generation of an idea, despite his insistence that there is a logic to "discovery." Hanson accepts that the field of psychology should deal with creative inspiration while philosophy should deal with logic – specifically, the logical reasons for considering a hypothesis to be worthy of further pursuit. That is, Hanson accepts the Temporal Processes Distinction and the Fields of Study Distinction, if 'discovery' is considered narrowly to mean just the generation of an idea. At the same time, he rejects other versions of the context distinction, such as the Historical Evidence Distinction, which claims that history cannot serve as evidence for philosophy.

The diversity of distinctions makes it very easy for someone to accidentally commit the fallacy of equivocation, whereby they mistakenly assume that conclusions about one version of the context distinction will transfer to other versions of the context distinction. Authors often do this without realizing that two versions are in operation, rather than just one.

For instance, in Chapter 4, I show how the Psychological Distinction and the Values Distinction are independent of each other, leading to the possibility of equivocation. Harvey Siegel assumes that the Psychological Distinction (thought processes of scientists are irrelevant to justification) implies the Values Distinction (values are irrelevant to justification). Siegel recognizes that Kuhn accepts the Psychological Distinction during periods of normal science, and concludes that Kuhn is also committed to accepting the Values Distinction during periods of revolution and theory choice. That is, they both agree that the particular path a scientist uses to reach the solution of a problem is irrelevant once the solution has been discovered and reformulated as a logical argument. From this, Siegel concludes that whatever values Kuhn identifies as playing a role in science can play a role *only* in the thoughts of the scientist, and not in the logical justification for a given claim. That is, without explicitly stating this, he assumes that the Psychological Distinction directly implies the Values Distinction, and/or is identical with it. This is a reasonable assumption to make if one believes that justification is strictly logical – however, Kuhn and many who have built on his ideas disagree with that conception of justification.[82]

Hoyningen-Huene and Kuhn have claimed that the context distinction is part of an old framework of philosophy of science that needs to be reevaluated. I agree, and argue that this impression is created because the assumptions of that framework were holding together these different versions of the context distinction. If someone approaches philosophy of science from within a given framework, then many of these versions of the context distinction *may be* more or less interchangeable with each other. So this equivocation would not have been a problem for early users of the context distinction in the 1940s. However, if one begins to question the underlying assumptions of the framework, then the connections between the versions are no longer given, but now must be argued for.

For instance, if justification is purely an issue of logic, than anything that is *not* logic is irrelevant. Making a list of all the things that are not logic is redundant. So the Psychological, Historical context, and Values Distinctions are more or less interchangeable, since they consist of lists of non-logical factors that are irrelevant to justification, and for the same reasons. In contrast, if one considers all observation to be theory-laden, and all experiences as permeated by one's worldview, then justification becomes not just fallible but actually *impossible* unless one rejects the Empirical/Logical Distinction. This is because these non-logical factors are affecting processes that are crucial to justification (observation, and collection and evaluation of evidence). Once one rejects the Empirical/Logical Distinction, however, justification can consist of many different factors. Each factor must be

---

[82] This point correlates with Hoyningen-Huene's comments on the Empirical/Logical Distinction.

separately specified, and separately accepted or denied. If you consider some factors irrelevant to justification -- psychology, values, etc. -- you must list them one by one and argue for them one by one, since you can no longer clump them together under the label "illogical" in order to exclude them from being relevant to justification. Here we see that though most of the versions of the context distinction are independent, one is not. As Hoyningen-Huene points out, the Empirical/Logical Distinction (justification is an issue of logic alone) is part of the old framework. I see the Empirical/Logical Distinction, then, as a lynchpin that held together many of the other distinctions.

This dissolving of the connections between versions of the distinction can help explain why there is so much confusion when the context distinction enters current conversations in philosophy of science. One participant might begin with the relatively uncontroversial Is/Ought Distinction, and then slip into an endorsement of the Historical context Distinction (that history and scientific practice cannot be used as evidence for philosophical claims), and then become confused when the other participants object. The underlying assumptions that, for several years, bridged these various versions of the distinction are no longer shared by all members of the philosophical community.

If the influences of Thomas Kuhn, N. R. Hanson, Paul Feyerabend and others have led to severe challenges to the context distinction and even its abandonment, it is in this way. Yes, they have challenged particular versions of the distinction, though neither in the ways nor as often as they are credited with. More importantly, these authors challenged the underlying assumptions that were holding together *different* versions of the context distinction and making them appear as a unified distinction.

IV. The Ambiguities Mask Underlying Issues that are Difficult to Resolve

We see that the context distinction is not one tool, but many tools, and these tools have been used in the context of arguments for a variety of claims. Many of the attempts have been unsuccessful at convincing rivals because the tool being used was ambiguous, and thus hid underlying assumptions. What happens when these assumptions are revealed?

Sometimes philosophical debates more generally are about linguistic confusions, and once these confusions are cleared up, it ends up that the members of the debate more or less agree with each other. In contrast, I maintain that the debates surrounding the context distinction among philosophers of science tend not to be simple linguistic confusions.[83] Rather, the distinction is entangled with underlying issues that are notoriously intractable. Thus, we should not be surprised that many of the debates have been difficult to resolve. For instance, in the 1960s the debates were about the nature of observation, and whether one could do science (make observations, predictions, and laws) outside of a conceptual scheme such as a paradigm or research programme. In the late 1990s and early 2000s, the debates have turned on the nature of justification, and whether values are the kind of thing that can enhance justification. The context distinction has not been useful in resolving these debates; on the contrary invoking it has often obscured the relevant issues.

---

[83] The main exception seems to be around debates about whether discovery has a logic or methodology. One's answer depends on one's definition of "discovery" and whether one uses it as a success term. Some argue that "to discover a claim" implies that one has articulated a *true* claim, in which case discovery involves justification. Others maintain that "discovery" can refer to the genesis of any idea, regardless of its merits. Arguably, this is a linguistic debate that could be resolved without great disagreement, although it does have important implications for where philosophers should direct their attention.

For instance, in Chapter 5, I note how Noretta Koertge argues that since Feminist Epistemology violates the context distinction, it should be rejected. I show how Koertge's underlying logical concern is that Nelson and others are making a category mistake: values are simply not the *kind* of thing that can enhance justification; they can only detract from it. Once the charge of making a category mistake is made explicit, it becomes easier to see how it is based on a different notion of justification, a notion that Nelson and others have argued should be rejected. Thus the charge of making a category mistake actually begs the question at hand.

Thus we see that rather than consisting of a series of neutral logical distinctions, versions of the context distinction often endorse a view on what "discovery" and "justification" consist of. Rather than clarifying the issues at stake, invoking the context distinction can actually obscure them, since it creates the illusion of agreement. If there are disagreements about what constitutes "justification," then two people might agree to the statement about justification, but have completely different things in mind by it. Thus, the rest of their arguments will be mutually unintelligible.

Does this mean we should abandon all versions of the context distinction entirely? Not necessarily. It can be a useful tool, if used with caution.

V. Using the Distinction: Suggestions on how to engage with it

There are two ways that philosophers of science might engage with the context distinction today: by looking backward at past work, or by engaging with the context distinction in current debates. As we saw in Chapter 3, the framework of distinguishing between object-level and meta-level discussions was helpful for understanding Kuhn, and can be helpful here. At the object level, we have instances of use (Reichenbach, Popper, Feigl, Kuhn, Koertge). At the meta-level, we have philosophers trying to organize these instances of use, evaluate them, and show their relations to each other (Hoyningen-Huene, Arabatzis, Kordig, myself).

For reflecting on previous uses, I suggest looking at the aims and goals of the author using the distinction, and to discover what questions he or she was trying to answer. Then evaluate to what extent the version of the context distinction s/he uses can help achieve these goals, and to what extent it begs the question. Chapter 3 on Kuhn is an illustration and demonstration of this approach.

For current and future uses of the distinction, there are two methods of engagement to address here. The first is as listeners: If we hear the context distinction being used in an argument, we should use the context and goals of the author to determine the meaning and applicability of the distinction. The key question is, Does this use of the distinction serve the purpose that the author intends it to serve?

Secondly, one must decide whether to use the context distinction as a speaker oneself. *One might think we should abandon the context distinction all together*. After exploring the uses of the distinction, I do not endorse this suggestion. Granted, given the myriad uses of the context distinction, it may be prudent to avoid using the words "context of discovery" and "context of justification" whenever possible. However, this alone is no reason to avoid using the versions of the distinction that have been specified in this project and elsewhere. Many of the versions of the context distinction are useful tools for reaching various goals (although, as I have argued, many others are not). The trouble comes from using the context distinction as a premise in an argument or as a background assumption, and from slipping from one version to another without flagging the transition. One must be careful to keep the versions clearly disambiguated, and either offer a version of the context distinction for *illustration only* or argue for it explicitly, rather than take it as a given.

VI. Understanding the distinction: topics for future research of the distinction itself

So where might the context distinction be a useful tool? One place to explore further is in discussions about ad hominem attacks. When is it legitimate to disregard a scientific claim because of who that claim originated with? Don Howard suggests that defending against ad hominem attacks was one of the primary motivations for developing the context distinction in the first place. Reichenbach, Popper, Carnap, and others were in Nazi Germany trying to defend Einstein, other Jewish scientists, and eventually themselves from attack. The Psychological Distinction encourages one to look at a neutral argument on a page, rather than the religion, race, or political affiliation of the person who wrote it down. Logically, the Psychological Distinction could be a very effective tool against these sorts of ad hominem attacks.[84]

Current debates involving ad hominem attack center on pharmaceutical companies and whether their own tests for their products are trustworthy. Pharmaceutical companies have argued that it makes no difference who funds the studies as long as the evidence is clear. Kevin Elliott offers several reasons to think this might not be true. One prominent reason for doubt is that pharmaceutical companies tend to rate their products as safer and more effective than independent researchers do. This in itself is not a reason to think that the

[84] Unfortunately, logic did not seem to help in this case.

context distinction fails here, since it does not tell us why this discrepancy exists. However, it does suggest that those tests are biased and unreliable in some way, and suggests that this is a fruitful area for further philosophical study (Elliott 2008).[85] Heather Douglas's work on cancer research is another valuable step in that direction (Douglas 2000).

VII. Summary

In this project I have argued for four claims. First, I suggest that we do not look for a single best version of the context distinction. Second, I have argued that we should characterize versions of the context distinction as tools and we should evaluate them with respect to the aims for which they are being used. Third, many versions of the context distinction are independent of one another. One can often use one version without being committed to another. Finally, these ambiguities often mask underlying disagreements. Once the ambiguities of the context distinction are made clear, this often pinpoints areas of disagreement, rather than resolving disputes by showing underlying agreement.

Some versions of the context distinction can continue to be valuable tools, so long as they are clearly defined and disambiguated from other versions, and are not presented as shared assumptions but as illustrations or claims to be argued for. As long as these steps are taken and a particular version is specified, along with the goal that it is meant to achieve, then such usage can be useful, even illuminating.

[85] See also (Elliott and McKaughan 2009) and (De Melo-Martín and Intemann 2009).

Identified by Hoyningen-Huene:

a) Temporal Processes	Discovery happens first, and is followed by justification.
b) Factual/Normative Processes	Discovery follows some historical process which can be described, where as justification involves normative evaluation.
c) Empirical/Logical Factors	Analysis of discovery is empirical, whereas what counts as justification is solely a question of logical reasoning, which is independent of time.
d) The Fields of History, Psychology, and Sociology / Philosophy	Historians etc. are concerned with discovery, while philosophers are concerned with justification.
e) Types of Questions	Questions such as "Can a statement be justified?" are of a different character than other types of questions such as "What happened during the discovery of a particular statement?".
f) External/Internal Factors	The context distinction has some similarities with the distinction in history between factors that relate to the historical development of a theory, and factors that relate to its content.
g) Descriptive/Normative*	The context of discovery is really a perspective from which to ask descriptive questions, while the context of justification is a perspective from which to ask normative questions.

* Called the "core" or "Lean DJ." Hoyningen-Huene rejects the other versions in favor of this one.

In addition, I identify:

i) Is/Ought:	Just because something has been done a certain way is not itself a reason to keep doing it that way. (Is/ought Distinction)
j) History/Philosophy:	Philosophers should not use the History of Science as evidence for a scientific methodology. It should be used only for illustration or inspiration. (Historical Evidence Distinction)
k) Thought process/Justification:	The thought processes leading a scientist to an idea are irrelevant to the justification of that idea. (Psychological Distinction)
l) Historical setting/Justification:	The historical setting of a scientist is irrelevant to the justification of their ideas. (Historical context Distinction)
m) Values/Justification:	The political, personal values of a scientist are irrelevant to the justification of their ideas. (Values Distinction)

Some of the uses I identify are more specific variations of the ones Hoyningen-Huene identifies. Providing more specific variations allows one to engage more critically than one can with a general or vague version. There are also countless major and minor variations still to be found in the literature.

Figure 5 Twelve Versions of the Context Distinction

REFERENCES

Anderson, Elizabeth. (2004a). "How not to Criticize Feminist Epistemology: Review of Pinnick, Koertge, and Almeder's Scrutinizing Feminist Epistemology" (self published on the Internet: http://www-personal.umich.edu/%7Eeandersn/hownotreview.html)

-------- (2004b). Uses of Value Judgments in Science: A General Argument, with Lessons from a Case Study of Feminist Research on Divorce. *Hypatia* 19 (1).

-------- (1995a). "Feminist Epistemology: An Interpretation and a Defense," *Hypatia* 10.3: 50-84.

-------- (1995b). "Knowledge, Human Interests, and Objectivity in Feminist Epistemology," *Philosophical Topics* 23: 27-59.

Antony, Louise. (2002). "Quine as Feminist: The Radical Import of Naturalized Epistemology," *A Mind of One's Own: Essays on Reason and Objectivity*, 2nd edition, p. 93-109. ed. Louse M. Antony and Charlotte Witt (Boulder: Westview Press).

Arabatzis, Theodore. (2006). "On the Inextricability of the Context of Discovery and the Context of Justification." In (Schickore and Steinle 2006).

Bacon, Francis. ([1620] 2000). *The New Organon*, ed. by Lisa Jardine and Michael Silverthorne Cambridge: Cambridge University Press.

Barnes, Barry. (1972). "Sociological Explanation and Natural Science: A Kuhnian Reappraisal," in *Archives Européennes de Sociologie* 13 (373-93).

Benfry, O. T. (1958). *Journal of Chemical Education*, 35, 21.

Brown, John Seely. (2002). "Growing Up Digital: How the Web Changes Work, Education, and the Ways People Learn." *United States Distance Learning Association Journal* 16: 2. February 2002.

Burian, Richard. (1980). "Why Philosophers Should Not Despair of Understanding Discovery." In (Nickles 1980).

------- (1977). "More Than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science." *Philosophy of Science* 44:1, 1-42.

Campbell, Richmond. (1998). *Illusions of Paradox: A Feminist Epistemology Naturalized.* (Oxford: Rowman & Littlefield Publishers, Inc.).

------- (2003) "Feminist Epistemology Naturalized" in Nelson and Nelson (2003).

Carnap, Rudolf. ([1928] 1969), *The Logical Structure of the World* (Berkeley: University of California Press).

Chmielecka, Ewa. (1982). "The Context of Discovery and the Context of Justification: A Reappraisal." *Polish Essays in the Philosophy of the Natural Sciences,* Boston Studies in the philosophy of science, v. 68. ed. Wladyslaw Krajewski. Dordrecht: Holland. D. Reidel Publishing Company.

Curd, Martin. (1980). "The Logic of Discovery: An Analysis of Three Approaches." In (Nickles 1980).

Daston, Lorraine and Peter Galison. (2007). *Objectivity.* (Cambridge: MIT Press).

De Melo-Martín, Inmaculada and Kristin Intemann. (2009) "How Do Conflict of Interest Policies Fail? Let Us Count the Ways," *Federation of American Societies for Experimental Biology Journal* 23:1638-1642.

Diamond, M. C., G. A. Dowling, and R. E. Johnson. (1981). "Morphologic Cerebral Cortical Asymmetry in Mate and Female Rats," *Experimental Neurology* 71, 261-68.

Douglas, Heather. (2004). "The Irreducible Complexity of Objectivity." *Synthese* 138:3, 453-473.
-------- (2000). "Inductive Risk and Values in Science," *Philosophy of Science* 67, 559-579.

Duhem, Pierre. (1954). The Aim and Structure of Physical Theory. (Princeton: Princeton University Press).

Einstein, Albert. ([1949] 1979), *Albert Einstein: Autobiographical Notes*, translated by Paul Arthur Schlipp, (Chicago: Open Court Publishing).

Elliott, Kevin. (2008). "Scientific Judgment and the Limits of Conflict-of-Interest Policies," *Accountability in Research: Policies and Quality Assurance* 15, 1-29.

Elliott, Kevin and Daniel McKaughan. (2009). "How Values in Discovery and Pursuit Alter Theory Appraisal," *Philosophy of Science* 76:5, 598–611.

Feigl, Herbert. (1974) "Empiricism At Bay? Revisions and a New Defense," in R. S. Cohen and M. W. Wartofsky (eds*) Methodological and Historical Essays in the Natural and Social Sciences. Boston Studies in the Philosophy of Science* 14 (Dordrecht: Reidel).
-------- (1970a). "The 'orthodox' View of Theories: Remarks in Defense as well as Critique," in M. Radner and S. Winokur (eds.), *Analysis of Theories and Methods of Physics and Psychology. Minnesota Studies in the Philosophy of Science.* Vol. 4, (Minneapolis: University of Minnesota Press).
-------- (1970b). "Beyond Peaceful Coexistence," in Stuewer 1970.

Feyerabend, Paul. ([1975] 1977), *Against Method* (3rd Edition) (London: New Left

Books).
-------- ([1969] 1970). "Consolations for the Specialist," in (Lakatos 1970).

Fine, Arthur. (1998), "The Viewpoint of No-One in Particular," in *Proceedings and Addresses of* The American Philosophical Association 72, pp. 9-20. Reprinted in William Egginton and Mike Sandbothe (eds.) *The Pragmatic Turn in Philosophy: Contemporary Engagements between Analytic and Continental Thought.* (Albany, NY: SUNY Press) 2004, pp. 115-129.

Finocchiaro, Maurice. "Scientific Discoveries as Growth of Understanding: The Case of Newton's Gravitation." In (Nickles 1980).

Franklin, Allan. (1993). *The Rise and Fall of the "Fifth Force": Discovery, Pursuit, and Justification in Modern Physics.* (New York: American Institute of Physics).

Friedman, Michael. ([1991] 1996), "The Re-evaluation of Logical Positivism," in *The Legacy of the Vienna Circle: Modern Reappraisals*, Vol. 6 ed. Sohatra Sarkar (New York: Garland Publishing).

Galison, Peter. (2003). *Einstein's Clocks, Poincaré's Maps: empires of time.* (New York: Norton).

Giere, Ronald. (1973). "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?" *British Society for the Philosophy of Science* 24:3, 282-297.

Gigerenzer, Gerd and Thomas Sturm. (2007). "Tools = Theories = Data? On Some Circular Dynamics in Cognitive Science." In *Psychologies Territories* (London: Lawrence Erlbaum Associates Publishers).
------- (2006). "How Can we Use the Distinction Between Discovery and Justification? On the Weakness of the Strong Programme in the Sociology of Science." In (Steinle and Schickore 2006).

Geschwind, Norman and Peter Behan. (1982). "Left-handedness: Association with Immune Disease, Migraine and Developmental Learning Disorder" *Proceedings of the National Academy of Science*, 79, 5097-5100.
-------- (1984) "Laterality, Hormones, and Immunity" in Norman Geschwind and M. Galaburda (eds). *Cerebral Dominance: The Biological Foundations*. (Cambridge, MA: Harvard University Press).

Glymour, Clark. 1980. Theory and Evidence (Princeton: Princeton University Press).

Goodman, Nelson. (1972). *Problems and Projects.* (New York: Bobbs-Merrill).

Graham, Loren. (1993). *Science in Russia and the Soviet Union.* (Cambridge: Cambridge

University Press).
-------- (1998) *What Have We Learned About Science and Technology from the Russian Experience?* (Stanford: Stanford University Press).

Gross, Paul and Norman Levitt. (1994). *Higher Superstition* (Baltimore: Johns Hopkins University Press).

Gutting, Gary (editor). (1980a). *Paradigms and Revolutions: Appraisals and Applications of Thomas Kuhn's Philosophy of Science* (Notre Dame: University of Notre Dame Press).
-------- (1980b). "Introduction," in (Gutting 1980a).
-------- (1980c). "The Logic of Invention." In (Nickles 1980).

Haack, Susan. (1993). "Knowledge and Propaganda: Reflections of an Old Feminist," *Partisan Review*: 556-564.

Hacking, Ian. (1999), *The Social Construction of What?* (Cambridge, Mass: Harvard University Press).

Hanson, N.R. (1971). "The Idea of a Logic of Discovery," in *What I Do Not Believe and Other Essays* (Dordrecht: Reidel).
-------- (1967). The Genetic Fallacy Revisited. *American Philosophical Quarterly*. 4:2 (101-113).
-------- (1960). "More on the Logic of Discovery." *The Journal of Philosophy* 57:6 (182-188).
-------- (1958a). *Patterns of Discovery; An Inquiry into the Conceptual Foundations of Science* (Cambridge, Eng.: University Press).
-------- (1958b). "The Logic of Discovery." *The Journal of Philosophy* 55:25 (1073-1089).

Harding, Sandra. (1986). *The Science Question in Feminism* (Ithaca: Cornell University Press).

Haraway, Donna. (1989). *Primate Visions* (New York: Routledge).

Howard, Don. (2006). "Lost Wanderers in the Forest of Knowledge," in Schickore and Steinle (2006).

Hoyningen-Huene, Paul. (2006). "Context of Discovery versus Context of Justification and Thomas Kuhn." In (Schickore and Steinle 2006).
-------- (1993). *Reconstructing Scientific Revolution: Thomas S. Kuhn's Philosophy of Science*. (trans. By Alexander T. Levine). (Chicago: University of Chicago Press).
-------- (1992). "The Interrelations between the Philosophy, History and Sociology of Science in Thomas Kuhn's Theory of Scientific Development." *The British Journal for the Philosophy of Science* 43:4 (487-501).

-------- (1987). “Context of Discovery and Context of Justification.” *Studies in the History and Philosophy of Science*18:4 (501-515).

Jarovsky, David. (1970). *The Lysenko Affair* (Cambridge, Massachusetts: Harvard University Press).

Kekulé, Friedrich. ([1890] 1958). In O. T. Benfry (1958) p. 21.

Keller, Evelyn Fox. (1983). *A Feeling for the Organism* (San Francisco: W.H. Freeman).

Kellert, Stephen. (2008), *Borrowed Knowledge: Chaos Theory and the Challenge of Learning across Disciplines.* (Chicago: University of Chicago Press).

Koertge, Noretta (1993). “Ideology, Heuristics and Rationality in the Context of Discovery”, *Correspondence, Invariance, and Heuristics: Essays in Honour of Heinz Post*, edited by Steven French and Harmke Kamminga (Dordrecht: Kluwer Academic Publishers) 125-136.
-------- (1996). "Feminist Epistemology: Beating an Un-dead Horse", *Flight from Science and Reason*, edited by Paul Gross, Norman Levitt, and Martin Lewis (New York Academy of Sciences).
-------- (1998). ed. *A House Built on Sand: Exposing Postmodernist Myths about Science.* (Oxford: Oxford University Press).
-------- (2003a). “Gender and the Genealogy of Scientific Discoveries,” in Pinnick et al.
-------- (2003b). “Feminist Values and the Value of Science,” in Pinnick et al. (2003).

Kordig, Carl. (1978). “Discovery and Justification.” *Philosophy of Science* 45: 110-117.

Kuhn, Thomas. (2000). *The Road Since Structure* (RSS). Ed by James F. Conant and John Haugeland. (Chicago: The University of Chicago Press).
-------- (1991). “The Trouble with the Historical Philosophy of Science” in RSS.
-------- (1983). “Reflections on Receiving the John Desmond Bernal Award” in *4S Review* 1: 4 (26-30).
-------- (1977). *Essential Tension: Selected Studies in Scientific Tradition and Change* (ET). (Chicago: University of Chicago Press).
-------- (1974). “Second Thoughts on Paradigms” in ET.
-------- (1973). “Objectivity, Value Judgment, and Theory Choice” in ET.
-------- (1970). “Logic of Discovery or Psychology of Research” in ET.
-------- (1969a). “Post-Script” in SSR.
-------- (1969b). “Comment on the Relations of Science and Art” in ET.
-------- (1969c). “Reflections on My Critics” in RSS.
-------- (1968). “The Relations between the History and the Philosophy of Science” in ET.
-------- ([1962] 1996). *The Structure of Scientific Revolutions* 3rd edition (SSR).

(Chicago: University of Chicago Press).
-------- (1957). *The Copernican Revolution. Planetary Astronomy in the Development of Western Thought.* (Cambridge: Harvard University Press).

Kukla, Rebecca (2008). "Naturalizing Objectivity," *Perspectives on Science*, vol. 16, no. 3.

Lakatos, Imre. (1970a), "History of Science and its Rational Reconstructions," PSA Proceedings, 91-136.
-------- (1970b), "Methodology of Scientific Research Programmes," in (Lakatos and Musgrave 1970).

-------- ([1969] 1970). "Falsification and the Methodology of Scientific Research Programmes," in (Lakatos 1970).

Lakatos, Imre and Alan Musgrave. (1970). *Criticism and the Growth of Knowledge.* (Cambridge: Cambridge University Press).

Law, John and Annemarie Mol, *Complexities: Social Studies of Knowledge Practices*

Lloyd, Elizabeth. (1995). "Objectivity and the Double Standard for Feminist Epistemologies, *Synthese* 104: 351-381.

Longino, Helen. (1990). *Science as Social Knowledge* (Princeton: Princeton University Press).

Masterman, Margaret. (1966). "The Nature of Paradigm," in (Lakatos 1970).

McLaughlin, Robert. (1982a). *What? Where? When? Why? Essays on Induction, Space, and Time, Explanation*. (Dordrecht: Reidel).
-------- (1982b). "Invention and Appraisal." In (McLaughlin 1982a).

McMullin, Ernan. (1993). "Rationality and Paradigm Change in Science" in World Changes: Thomas Kuhn and the Nature of Science (reprinted in Curd and Cover.)
-------- (1974). "History and Philosophy of Science: A Marriage of Convenience?" *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association.* 1974, 585-601.
-------- (1970). "The History and Philosophy of Science: A Taxonomy," in (Stuewer 1989).

Meiland, Jack. (2001). "Category Mistake," *Cambridge Dictionary of Philosophy*, 2nd edition, edited by Robert Audi. (Cambridge: Cambridge University Press).

Musgrave, Alan. (1980). "Kuhn's Second Thoughts," in (Gary Gutting 1980a).

Mueller, Axel and Arthur Fine. (2004). "Realism, Beyond Miracles," in *Contemporary Philosophy in Focus: Hilary Putman*, edited by Ben Manahim (Cambridge: Cambridge University Press).

Nickles, Thomas. (2002). *Thomas Kuhn*. (Cambridge: Cambridge University Press).
-------- (1985). "Beyond Divorce: Current Status of the Discovery Debate." *Philosophy of Science.* 52: 2, 177-206.
-------- (1980). Ed. *Scientific Discovery, Logic and Rationality*. *Boston Studies in the Philosophy of Science 60* (Dordrecht: Reidel).

Nelson, Lynn Hankinson. (1990). *Who Knows: From Quine to a Feminist Empiricism* (Temple University Press: Philadelphia).
------- (1995a) "The Very Idea of Feminist Epistemology," *Hypatia* vol.10, no. 3.
------- (1995b). "Feminist Naturalized Philosophy of Science," *Synthese* vol. 104.

Nelson, Jack and Lynn Hankinson Nelson. (1994). "No Rush to Judgment." *Monist*, 77: 4.
------- (2003). *Feminist Interpretations of W. V. Quine* (University Park, Pennsylvania: Pennsylvania State University Press).

Pickering, Andrew. (1995), *The Mangle of Practice: Time, Agency, and Science* (Chicago: University of Chicago Press).

Pinnick, Cassandra, Noretta Koertge, and Robert Almeder (editors) (2003). *Scrutinizing Feminist Epistemology: an Examination of Gender in Science* (New Brunswick: Rutgers University Press).

Pinnick, Cassandra L. (2003). "Feminist Epistemology: Implications for Philosophy of Science," in Pinnick et al. (2003).

Popper, Karl. ([1963] 2002). *Conjectures and Refutations 4th Edition*. (New York: Routledge).
-------- ([1963] 1969). *Conjectures and Refutations: The Growth of Scientific Knowledge* 3rd Edition. (London: Routledge).
-------- ([1934] 1969). *Logik Der Forschung* (Tübingen: J.C.B. Mohr).
-------- ([1959] 2002). *The Logic of Scientific Discovery* (New York: Basic Books).

Potter, Elizabeth (2001). *Gender and Boyle's Law of Gases.* (Bloomington: Indiana University Press).

Putnam, Hilary ([1962a] 1975b). "The Analytic and the Synthetic," *In Philosophical Papers II. Mind, Language, and Reality*. (Cambridge: Cambridge University Press).

Putnam, Hilary ([1962b] 1975a). "It Ain't Necessarily So," *In Philosophical Papers I. Mathematics, Matter, and Method*. (Cambridge: Cambridge University Press).

Reichenbach, Hans.([1928] 1958). *Space and Time*, translation of *Philosophie der Raum-Zeit-Lehre*, (New York: Dover Publications).
-------- (1947). *Elements of Symbolic Logic* (New York: MacMillan).
-------- (1938). *Experience and Prediction* (Chicago: University of Chicago Press).
-------- (1935). "Zur Induktion-Machine," in *Erkenntnis*, p 172-173.

Richardson, Alan. (1996). "From Epistemology to the Logic of Science: Carnap's Philosophy of Empirical Knowledge in the 1930s," *Origins of Logical Empiricism, Minnesota Studies in the Philosophy of Science* vol. 16, ed. by Ronald Giere and Alan Richardson, (Minneapolis: University of Minnesota Press).
-------- (2000). "How to Be a Good Non-Naturalist: Epistemology as Rational Reconstruction in Carnap and His Predecessors," *Rationalität, Realismus, Revision,* ed. Julian Nida-Rümelin. (Berlin: Walter de Gruyter).

Ryle, Gilbert. ([1949] 2000). *The Concept of Mind* (Chicago: University of Chicago Press).

Schaffer, Simon. (1994). "Making Up Discovery," in *Dimensions of Creativity*, ed. by Margaret A. Boden (Cambridge, MA: MIT Press).

Scheffler, Israel. ([1967] 1982). *Science and Subjectivity*. 2nd edition. (Indianapolis: Hackett Publishing Company).
-------- (1972). "Vision and Revolution: A Postscript on Kuhn," in *Philosophy of Science* 39:3 (366-374).

Schickore, Jutta and Friedrich Steinle (editors) (2006). *Revisiting Discovery and Justification: Historical and philosophical perspectives on the context distinction.* (Dordrecht: Springer).

Shapere, Dudley. (1966). "Meaning and Scientific Change." In *Mind and Cosmos*. ed. by R. G. Colodny. (Pittsburgh: Univ. of Pittsburgh Press).
-------- (1964). "The Structure of Scientific Revolutions." *The Philosophical Review*, 73:3, 383-394

Shapin, Steven and Simon Schaffer. (1985). *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press).

Sheehen, Helena. (1985). *Marxism and the Philosophy of Science: A Critical History* (Atlantic Highlands, NJ: Humanities Press).

Siegel, Harvey. (1980a). "Justification, Discovery, and the Naturalizing of

Epistemology." *Philosophy of Science* 47:2 (297-321).

-------- (1980b). "Objectivity, Rationality, Incommensurability, and More." *The British Journal for the Philosophy of Science* 31:4 (359-375).

Soble, Alan. (2003). "Keller on Gender, Science, and McClintock" in Pinnick et al. (2003).

Steinle, Friedrich and Jutta Schickore. (2006). *Discovery and Justification: Revisiting a Precarious Distinction.* (Dordrecht: Springer).

Sturm, Thomas and Gerd Gigerenzer. (2006). "How Can We Use the Distinction Between Discovery and Justification? On the Weaknesses of the Strong Programme in the Sociology of Science" in Schickore and Steinle (2006).

Stuewer, Roger. ([1970] 1989). *Historical and Philosophical Perspectives of Science.* (New York: Gordon and Breach. Reprinted from University of Minnesota Press).

Thomasson, Amie. (2008). "Categories", *The Stanford Encyclopedia of Philosophy* Edward N. Zalta (ed.), Fall edition, forthcoming URL = <http://plato.stanford.edu/archives/fall2008/entries/categories/>.

Uebel, Thomas. (2007). *Empiricism at the Crossroads: The Vienna Circle's Protocol-Sentence Debate.* (Chicago: Open Court).

Waller, J. (2002). *Einstein's Luck: The Truth Behind Some of the Greatest Scientific Discoveries*. (Oxford: Oxford University Press).

Watkins, John. ([1965] 1970). "Against 'Normal Science'," in (Lakatos 1970).

Wessely, Christina. (forthcoming). *Cosmic-Ice Theory: Science, Fiction, and the Public.*

Williams, L. Pearce. (1965). *Michael Faraday, A Biography*. (New York: Basic Books).

Zammito, John H. (2004). *A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour.* (Chicago: University of Chicago Press).

CURRICULUM VITAE

MONICA AUFRECHT

Department of Philosophy
University of Washington
361 Savery Hall, Box 353350
Seattle, WA 98195

Max Planck Institute for
the History of Science
Boltzmannstraße 22
14195 Berlin, Germany

Email: aufrecht@uw.edu
http://staff.washington.edu/aufrecht
Phone: 49-0152-227048480

Education

2010	Ph.D.	University of Washington, Philosophy (2005-2010)
2005	M.A.	University of Washington, Philosophy (2003-2005)
2000	B.A.	Wellesley College, History and Philosophy of Science, *cum laude*

Area of Specialization Philosophy of Science

Areas of Competence Applied Ethics, Environmental Ethics, Bioethics, Feminist Philosophy, Early Analytic Philosophy

Dissertation

"Values in Science: The Context of Discovery and the Context of Justification"

Karl Popper famously distinguished between 'how an idea occurs to a man' and 'whether the idea is justified.' Recent work has examined the roots of this "context distinction" as well as its significance in contemporary philosophy of science. My dissertation examines the different and sometimes contradictory uses of the context distinction. As I navigate these debates, I use the distinction as a lens for focusing on how values enter science.

Committee Arthur Fine (chair), Stephen Gardiner, Lynn Hankinson Nelson, Andrea Woody, Alison Wylie

Awards and Honors

2010	**Research Fellow** Max Planck Institute for the History of Science in Berlin, Germany (March – July) For outstanding students completing their dissertations.
2009	**Graduate Fellow**, Science Studies Network, University of Washington Competitively selected from faculty and graduate students working in science studies. Fellows organize interdisciplinary bi-weekly seminars on the theme Democracy and Science.

2005 & 2008	**Philosophy Department Teaching Award**, University of Washington Awarded to graduate students in recognition of excellent teaching.
2009	**Dissertation Fellowship**, Dept. of Philosophy, University of Washington
2007-2008	**Lead Teaching Assistant**, Dept. of Philosophy, University of Washington Awarded to a graduate student who displays exceptional teaching, leadership and mentoring skills. Responsibilities include mentoring incoming graduate students, leading weekly graduate colloquia on teaching methods, and overseeing orientation and department events.
2005	**Graduate Student Research Fellowship,** Honorable Mention National Science Foundation (NSF)

Teaching Experience

As Main Instructor

Environmental Ethics (two times)
Practical Reasoning and Critical Thinking (three times)
Seminar in Teaching Philosophy
History and Philosophy of Science Capstone Course (upper level)
(for undergraduate HPS majors, co-taught with Professor Bruce Hevly, History Department)

As Teaching Assistant	*with*
Philosophy of Science	Lynn Hankinson Nelson
Environmental Ethics	Andrew Light
Environmental Ethics	Andrea Woody
Philosophy of Law	Ron Moore
Introduction to Ethics	Angela Smith
Introduction to Philosophy	Ann Baker
Introduction to Philosophy	Michael Rosenthal
Symbolic Logic	S. Marc Cohen
Symbolic Logic	Cass Weller
Symbolic Logic	Arthur Fine
Global Health and Justice: “Diagnosing Injustice. Ethics, Power and Global Health” (Humanities Dept.)	Janelle Taylor & Sara Goering (Anthropology, Phil.)

Presentations

2009 “The Context Distinction: Debates over Feminist Philosophy of Science”

- Society for Philosophy of Science in Practice (SPSP), University of Minnesota
- Feminist Epistemologies, Methodologies, Metaphysics, and Science Studies (FEMMSS3), Topic: *The Politics of Knowledge,* University of South Carolina

2009 "Looking Ahead: 'Bionic' Contact Lenses and Technical Systems" with Alex Dezieck
- The Society of Nanoscience and Emerging Technologies, Nanoethics Graduate Education Symposia (S.NET), University of Washington

2009, 2008 "On Reichenbach's Context Distinction"
- Integrated History & Philosophy of Science Conference (&HPS), U. of Notre Dame
- Columbia History of Science Group, Marine Biological Laboratory at Friday Harbor

Graduate Student Presentations

2009 "'Facts' in Francis Bacon's *Novum Organum*: Reflections on Lorraine Daston's account"
- History & Philosophy of Science & Technology Graduate Student Conference (HAPSAT), Institute for the History and Philosophy of Science & Technology (IHPST), Topic *Evidence in Context*., University of Toronto.

2009 "Mitigating Climate Change: Moral Obligations at the Personal Level"
- Conference on Communicating the Environment, University of Washington

2005 "Galileo's Maps of the Moon"
- Graduate Student Workshop: *Maps, Pictures, Graph: Scientific Images and Science,* with Robert Brain and Simon Schaffer, University of British Columbia

Academic Service

2009	Manuscript Reviewer, *International Studies in the Philosophy of Science*
2003-2009	Committees (Colloquium, Graduate Admissions, Faculty Liaison), U. of Washington
2006	Consultant, Project 2061: The American Association for the Advancement of Science (AAAS)

Conferences and Panels organized

2008	"US Energy Policy; Priorities for the Next Administration," Panel Discussion organized by the Forum on Science, Ethics, and Policy (FOSEP), UW
2007	"Environmental Ethics," Fourth Biennial Graduate Student Conference in Philosophy, UW

References

Arthur Fine, Professor of Philosophy, UW	afine@uw.edu
Stephen Gardiner, Associate Professor of Philosophy, UW	smgard@uw.edu
Sara Goering, Assistant Professor of Philosophy, UW	sgoering@uw.edu
Lynn Hankinson Nelson, Professor of Philosophy, UW	lynnhank@uw.edu
Andrea Woody, Associate Professor of Philosophy, Assoc. Chair, UW	awoody@uw.edu
Alison Wylie, Professor of Philosophy, UW	aw26@uw.edu