

# Scientific practices and their social context

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# Acknowledgments

This dissertation is not just the product of the last three years — although it is certainly that. Initially, it was conceived as a synthesis of some of the work I had done in the preceding four or six years, during my coursework as a graduate student at Notre Dame and, before that, reading philosophy in my spare time while studying math at the University of Illinois, Chicago. But, as the project developed, I found myself recovering themes and worries from my intellectual past: the history of science and technology that I had studied as an undergraduate; the optimistic socialism of my late teens; the disappointment with a profoundly unjust world, apparently lifelong; and the sense of wonder and curiosity at the great diversity of our social and biological world.

So I cannot just thank those who have done so much as mentors and colleagues during the past few years — though Don Howard, Paul Weithman, Janet Kourany, and Peter Wicks all deserve special thanks, as does the HPS Reading Group taken collectively. I must also thank Lynn Hankinson-Nelson, Jim Evans, and Paul Loeb, whose classes pushed an undergraduate from mathematics into philosophy (and history) of science. Before these were so many other formative relationships with teachers and friends.

Through all of these three long decades have been my mother and father. They were the source of so many of the ethical and intellectual commitments that I have brought to this project. From my father I learned to love the natural world and the science that explores it. And from my mother I learned the love of justice and peace and the virtue of persistence. Without the last of these, I most certainly would not have seen this project to its completion.





# Preface

This PDF includes the complete text of my Ph.D. dissertation, completed in April of 2012 at the University of Notre Dame, with margins and layout more efficient and appropriate for reading than those required by Notre Dame's graduate school. Both presentations of the dissertation were typeset in L<sup>A</sup>T<sub>E</sub>X 2<sub>ε</sub>.



# Chapter 1

## Introduction

### 1.1 Three vignettes in science and values

A small group of feminist scientists are outspoken critics of evolutionary psychology. They argue that evolutionary psychology depends on dubious assumptions, such as the differential parental investment model, and sexist values. Clearly, they say, the “science” of evolutionary psychology has been deliberately designed to rationalize sexist beliefs and values: that men are naturally promiscuous and that women should be housewives, for example. Evolutionary psychology is biased, special-interest science, and therefore unacceptable.

A small group of scientists affiliated with the fossil fuels industry are outspoken critics of the anthropogenic explanation for climate change. They argue that the anthropogenic explanation depends on dubious assumptions, such as the legitimacy of interpolated historical temperature data, and environmentalist and anti-capitalist values. Clearly, they say, the “science” of climate change has been deliberately designed to rationalize socialist and environmentalist beliefs and values: that human industry is destroying the world and that a strong central government should carefully regulate the economy, for example. Anthropogenic explanations for climate change are biased, special-interest science, and therefore unacceptable.

A small group of environmentalist scientists are outspoken critics of microbiological research on transgenic crops. They argue that this research depends on dubious assumptions, such as reductionist accounts of cell biology and ecology, and the values of globalized commerce. Clearly, they say, the “science” of transgenics has been deliberately designed to rationalize the beliefs and values of globalization: that biotechnology, developed and controlled by multinational corporations, is the best way to feed the world and that sustainable traditions and close-knit community should be sacrificed for the sake of efficiency and a global market, for example. Transgenics are biased, special-interest science, and therefore unacceptable.

How are we – whether “we” are active participants or outside observers – to sort through the claims, counterclaims, and counter-counterclaims in each of these three

vignettes? I'm inclined to say that the feminist scientists are correct, the climate change skeptics are wrong, and the environmentalist scientists, while they have a point, are a bit hyperbolic. But what can I do to support these inclinations? And what's to stop those with different views from arguing that feminist scientists, climatologists who support the anthropogenic explanation, and environmentalist scientists are illegitimately designing their "science" to rationalize their own beliefs and values?

These three are not, of course, the only seemingly-intractable public disputes over scientific research. Controversy over creationism and evolution by natural selection seems to have died down a bit since the 2005 case of *Kitzmiller v. Dover Area School District*, but no doubt it will spring to life again within a decade or two. At least once a year, some finding that some socially significant trait – often something to do with intelligence – is statistically heritable is played up in that nebulous entity-space, "the media," usually in the grossly misleading form of a claim that the trait has been proven to be genetically determined. And, over the last few years, numerous controversies have erupted over the economics of various public policies – bailouts of large financial firms, economic stimulus through fiscal policy, large governmental debt, and, as I write these words in early March 2011, public employee unions.

Does philosophy have anything to contribute here? In particular, since we are talking about controversies that touch on both politics and science, can political philosophy and philosophy of science do anything to help?

Consider first philosophy of science. For the better part of fifty years,<sup>1</sup> philosophers of science have discussed the relationship between science and its social context – including its political context – in a subfield that, today, is often called *science and values*. Some of this work has had a great deal of influence on the public controversies. To simplify the picture a bit, both the political left and the political right rhetorically appeal to scientists and scientific research to support their policy proposals, and both rhetorically decry the "junk science," "bias," and "ideology" of the other side. As Daniel Sarewitz has pointed out, all of this presupposes a "naive and idealized" conception of science and the (il)legitimate influence of ethical and political values; and this conception dates back to certain disputes between philosophers of science in the early and mid-twentieth century.<sup>2</sup> While Sarewitz's point is a bit pessimistic – philosophers of science got us into this mess – it also gives us some room for optimism – philosophers of science, working with less naive and idealized conceptions, might be able to get us out.

For these less naive and idealized conceptions, we might try to turn to more recent work in science and values. From roughly the early 1980s to the late 1990s, most if not all of the science and values discussion took the form of contributions to the so-called "Science Wars," a massive intellectual dispute that was as dysfunctional as any of the three opening vignettes. Over the last fifteen years or so, as the parties to the Science Wars have either moved on to other issues or become exhausted, the

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<sup>1</sup> And perhaps longer; see §4.1.

<sup>2</sup> Daniel Sarewitz, "Curing Climate Backlash," *Nature* 464, no. 4 (4 March 2010): 28.

bitter and adversarial tone has been replaced with one of reasonable disagreement and détente. But, among contemporary philosophers of science, there are still two sharply distinguished and deeply-entrenched sides to the science and values debate.<sup>3</sup> Within this debate, there is little common ground and just as much conceptual and argumentative innovation. In short, the science and values debate, within philosophy of science, seems to be stalled. The notable exceptions to this pattern include an active body of literature on the relationship between science and public policy.<sup>4</sup> But the sort of cases we saw in the opening vignettes, while they may have public policy aspects, generally happen below the public policy level, at what we might call the level of civil society.

Perhaps political philosophers can help. During roughly the same period of time that philosophers of science were engaged in the Science Wars, political philosophers were engaged in a dispute between, on the one hand, liberal individualism and, on the other, a family of views incorrectly labelled “communitarianism.” However, unlike in the Science Wars – in which the exchanges often led the interlocutors to dig in their heels even further – liberalism seems to have responded to some of the communitarian critiques, with prominent liberal political philosophers abandoning or significantly modifying their commitment to individualism.<sup>5</sup> Thus there is nothing like the deep, lingering animosity that the Science Wars left in philosophy of science. Further, political philosophy never retreated to the “icy slopes of logic,” as George Reisch (following Carnap) described post-Cold War philosophy of science;<sup>6</sup> many political philosophers over the past fifty years have been public intellectuals, moved to address urgent issues of social justice and improve the functioning of our public political culture. But, for their part, political philosophers have said very little about science. There are occasional worries about whether the specialization or expertise of scientists is in some tension with democracy, but even this is usually understood in terms of public ignorance of accepted or established scientific knowledge. Further, this work often deals with either highly abstract issues of public discourse or the formation and implementation of public policy. Here again, there is almost no attention to the “lower” organizations that compose civil society.

I suggest that a better approach would draw on, and combine, work in both areas of philosophy. The science and values debate requires a more sophisticated account

<sup>3</sup> We will discuss these two sides in considerable detail in chapters 3, 4, and 6.

<sup>4</sup> See, especially, Kristin Shrader-Frechette, *Taking Action, Saving Lives* (Oxford and New York: Oxford University Press, 2007), ISBN: 9780195325461; Heather Douglas, *Science, Policy, and the Value-Free Ideal* (Pittsburgh: University of Pittsburgh Press, 2009); Kevin Elliott, *Is a Little Pollution Good for You?: Incorporating Societal Values in Environmental Research* (Oxford and New York: Oxford University Press, 2011), ISBN: 9780199755622.

<sup>5</sup> We will discuss individualism in §7.4.

<sup>6</sup> George Reisch, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (Cambridge University Press, 2005).

of goal-oriented social activities, practical reason, systems of political organization, and ethical values — in short, better social and political philosophy. And social and political philosophers require a more sophisticated conception of scientific research and a more concrete and empiricist methodology — in other words, a richer philosophy of science.

So, in this dissertation, I aim to develop and utilize just this synthesis of philosophy of science and political philosophy. My Swiss army knife in this project is a conception of complex, collaborative, goal-oriented activities that I call “practices.” In chapter 2, I develop this conception; I draw explicitly on the work of Alasdair MacIntyre, but read him through John Dewey and contemporary philosophy of science. At the bottom of this conception is a distinction between the “internal goods” attached to practices — such as the knowledge and technology produced by scientific inquiry and the justice-related aims of such ethical and political practices as feminism, anti-racism, and movement socialism — and “external goods” — especially wealth, power, status, and happiness in the subjective sense. Where practices aim at producing their various internal goods, the social organizations that I call “institutions” aim at provisioning external goods for the use of practices. While practices require institutions for their continued maintenance and survival, the pursuit of external goods on the part of institutions often threatens to overwhelm the pursuit of internal goods in their corresponding practices. I call these situations or states “institutional conflict” and “institutional domination.”

We can also consider the relation between two (or more) different practices. After discussing a problem with a few of MacIntyre’s claims about such relations, I offer a conception of “joint practice,” which involves individuals pursuing the internal goods of several practices in tandem. Joint practice, I suggest, is essential for producing mutual understanding among the members of two different practices and stimulating certain kinds of progress within practices; when joint practitioners are marginalized, it may seem that other practices are actually institutions.

In chapter 3, I apply this general conception of practice to scientific inquiry, developing two distinct conceptions of scientific practice. The basic difference between these two conceptions is the aims or goals that they attribute to scientific practice: what, ultimately, scientists are trying to *do* when they do science. On the “narrow view,” the sole primary aim of scientific inquiry is the production of excellent representational knowledge. By contrast, on the “broad view,” the aims of scientific inquiry include practical knowledge (or know-how) and technology; achieving these aims is interdependent with producing representational knowledge, and all three aims are equally important or valuable. Since, I take it, the narrow view is widely held among philosophers of science, I devote considerable space in this chapter to making the broad view a plausible alternative to the narrow view.

Over the course of chapters 4, 5, and 6, through a series of claims that I call “connection hypotheses,” I show that the two conceptions of scientific practice are closely connected to the two sides of the science and values debate. Thus this debate

is not, as the literature would often suggest, (primarily) about epistemology or theory choice. The differences between the two sides go much deeper, down to the ultimate aims or goals of scientific practice. First, in chapter 4, I argue that the narrow view – when it is widely held – leads to the marginalization of joint practitioners (applied scientists and engineers). But then, in line with the account of joint practice given in chapter 2, this leads to the misperception that other practices – such as those practices whose aims are ethical and political values – are actually institutions – engaged in the pursuit of only external goods – and thus permitting them to influence scientific inquiry threatens the latter with institutional domination. So scientific inquiry should be isolated from the influence of ethical and political values. I call this position in the science and values debate “isolationism.” In light of the connection between this position and the narrow view, the whole complex of views can be called “narrow view-isolationism.” Developing the argument for this connection involves an extended examination of the relationship between “pure” and “applied” scientific inquiry.

In chapter 5, in preparation for the philosophical argument of chapter 6, I examine a series of important contemporary and historical cases in the influence of ethical and political values on scientific inquiry. Most of the case studies focus on the prospects that joint practice offers for progress in scientific inquiry. I also consider Nazi science, which is commonly presented by isolationists as a challenge to the “transactionist” view that ethical and political values may legitimately influence scientific inquiry.

Chapter 6 deals with the connection hypothesis for transactionism. The overall conclusion, similar to chapter 4, is that the broad view leads to transactionism. After developing the argument for this connection hypothesis, I examine the relationship between “pure” and “applied” scientific inquiry on transactionism, show how transactionists can respond to the threat of institutional domination, discuss the effects of the predominance of the narrow view on the reception of transactionism, and close with some advice for my fellow transactionists.

To be clear, my aim in these central chapters is not to argue for transactionism or against isolationism. While I take a position in this debate, I believe that it is necessary to develop a more adequate understanding of the basic disagreements between the two positions before the debate can move forward. That is, I believe the contemporary debate has stalled precisely because the two positions normally (and implicitly) appeal to quite different sets of assumptions, and thereby misunderstand each other in characteristic ways. So, my aim is merely to provide this more adequate understanding, in the form of the connection hypotheses.

In chapter 7, I turn to political philosophy in an attempt to develop the thought that institutional domination – as exemplified by the politicization and commercialization of science – is a form of injustice. After motivating this thought, I review the framework for political philosophy that John Rawls called “political liberalism,” and argue that the conception of practice — despite its roots in non-liberal political philosophy — can be embedded in a version of Rawls’s framework. As with the argument of the central chapters, my goal is less to give an argument for a thesis

and more to show how the conception of practice provides a rich understanding of socially-relevant issues.

Finally, in the conclusion, chapter 8, I return to the three opening vignettes. While I cannot hope to provide a complete solution to the problem they present, I do show how the tools that I have developed in the intervening pages can be applied to this problem.

## 1.2 Methodology and assumptions

Social scientists have long since acquired the admirable habit of stating explicitly their methods and assumptions. Analytic philosophers would do well to do the same.

### 1.2.1 Empiricism

Like most other philosophers of science, I consider myself an empiricist in some broad sense.<sup>7</sup> I have therefore made considerable effort to avoid superempirical speculation and appeals to intuition or other sorts of thoroughgoing *a priori* justification. At the same time, I'm completely convinced by arguments from Thomas Kuhn, Helen Longino, and others, that all human intellectual activities rely on unsupported and potentially quite controversial assumptions.<sup>8</sup> I have endeavored to identify these explicitly as my own assumptions throughout this dissertation, and to make them more-or-less amenable to empirical investigation, revision, confirmation, and rejection. For example, I believe the crucial distinction between internal and external goods that forms the starting point for almost everything I say here can and should be subject to empirical investigation in moral psychology, that is, by investigating whether or not people do in fact typically make and respect something like this distinction when engaging in practical deliberation.

I draw on three broad types of empirical data. First are loose generalizations from common human activities. For example, I take the distinction between internal and external goods to be supported in part by a generalization from my own practical deliberation and numerous but unsystematic observations of the practical deliberations of others. I do *not* consider these data incorrigible; by contrast, I consider them the weakest of the three types I draw on. Also, I do *not* consider these data

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<sup>7</sup> 56.3% of target faculty respondents to the PhilPapers Survey who listed General Philosophy of Science as one of their areas of specialization also identified themselves as empiricists. "Correlations with: AOS:General Philosophy of Science," The PhilPapers Surveys, 2010, [http://philpapers.org/surveys/linear\\_most\\_with.pl?A=profile:AOS:General%20Philosophy%20of%20Science](http://philpapers.org/surveys/linear_most_with.pl?A=profile:AOS:General%20Philosophy%20of%20Science) (accessed August 22, 2011)

<sup>8</sup> Pierre Duhem, *La théorie physique: Son objet et sa structure* (Paris: Chevalier et Rivière, 1906); Thomas Kuhn, *The Structure of Scientific Revolutions*, third edition (1962; Chicago: University of Chicago Press, 1996); Helen Longino, *Science as Social Knowledge* (Princeton: Princeton University Press, 1990).



to be *a priori* or the product of some rationalist or non-empirical faculty of intellectual intuition. They are, instead, the informal, inductive, empirical generalizations of everyday experience.

Second are primary texts, in something like the historian's sense. Much of chapter 4 deals with the motivations that led certain philosophers to take a certain position in the science and values debate. My method here is historical rather than, for example, psychoanalytic or cultural: I use the particular written words of these individuals to support claims about those individuals, rather than appealing to essential features of the human psyche or making claims about an abstract *Zeitgeist*.

Third are case studies, typically concerning the work and contributions of particular scientists or groups of scientists in particular times and places. These sorts of data are especially important in chapter 5. Historian of science Sarah Richardson has argued that analyses of case studies are a characteristic feature of feminist philosophy of science;<sup>9</sup> I believe they are a characteristic feature of philosophy of science more generally, though (since I am quite deeply immersed in the feminist philosophy of science literature) perhaps this reflects a sample bias on my part. In some cases these data are also primary texts, that is, the second kind of data: in §5.4, I consider the influence of movement socialism on the work of one of the members of the Vienna Circle by examining the pamphlets he wrote to explain his economic ideas to working-class members of the socialist movement. But in many other case studies I rely on secondary texts, again in the historian's sense. For example, I rely heavily on the accounts of others for my discussion of the work and impact of feminist primatologists and Nazi physicists.

### 1.2.2 Pragmatism

Over the last few years, I have become a fan of the work of John Dewey and what is often called the Left Vienna Circle – including the work of Otto Neurath and Philip Frank – and have rekindled my undergraduate affiliation for the Marxist tradition in political philosophy. I have also — as the following chapters will make clear — been deeply immersed in the work of Alasdair MacIntyre and in feminist philosophy. All these individuals and movements within philosophy – especially at their best and most profound – share a commitment that, for lack of a better term, I call *pragmatism*. Very roughly, this might be defined as *a socially-engaged naturalistic concern with human activity*. By *socially-engaged* I mean that all of these philosophers are (or were) deeply concerned with issues of justice and human flourishing “beyond” or “outside” academia. Many of them are active public intellectuals; even more are, in some way or another, active in social movements on a local or national scale. Consequently, there is a general sense that philosophy must be *operationalizable*, that is, able to

<sup>9</sup> Sarah Richardson, “The Left Vienna Circle, Part 2: The Left Vienna Circle, Disciplinary History, and Feminist Philosophy of Science,” *Studies in the History and Philosophy of Science Part A* 40, no. 2 (June 2009): 171, doi:[10.1016/j.shpsa.2009.03.010](https://doi.org/10.1016/j.shpsa.2009.03.010).

inform usefully action and practical deliberation, especially with respect to urgent issues of justice and human flourishing.

By *naturalistic* I mean that the work of these philosophers is deeply, but never uncritically, informed or influenced by scientific inquiry. They may hold supernatural metaphysical or ethical commitments, but in no case are they, for example, Cartesian dualists or Hegelian idealists, who believe that the most important features of human beings are supernatural. While not all of these philosophers endorse a conception of human nature, those that do all reject a strict separation of human minds from human bodies *au* Descartes. Also, many of these conceptions of human nature emphasize human interdependence and sociality: we are, on these views, essentially or naturally social beings.

By a *concern with human activity* I mean that these philosophers reject the view that Alison Jaggar calls *normative dualism* and that I call *rationalism*: the view that human cognitive abilities or activities are more valuable than our other abilities and activities, such as the activity of caring for others or various abilities to produce material goods.<sup>10</sup> When these philosophers discuss logic and reasoning, it is treated as a kind of human activity, often one that is deeply collaborative and dynamic, rather than a set of static relations between immaterial entities.

Science, for these pragmatists, is an activity undertaken by groups of material beings, often for the sake of goals other than truth or certainty. Much of this dissertation is an attempt to clarify and develop this claim.

### 1.2.3 Rigor

I am, in the terminology I introduce in chapter 4, a transactionist. Transactionism, for reasons I attempt to explain, has long been a marginalized view in mainstream analytic philosophy of science. I believe that, to some extent, this is due to matters of style rather than substance: Many of the most influential transactionists – especially feminist transactionists – are not philosophers, and have not been trained to write in the sociolect of mainstream analytic philosophy. Consequently, their ideas are easily misunderstood when encountered by mainstream analytic philosophers. For example, it is quite common for non-philosophers to run together (from a philosopher’s perspective) the philosophical distinction between truth and justification, and refer to both as “truth.” It is one thing to say that the *agent*-proposition relation “*S* is justified in believing *p*” is context-dependent, socially constructed, requires that *S* be embedded in a community with certain features, and so on. It is quite another to say that the *world*-proposition relation “*p* is true (of *W*)” is context-dependent, socially constructed, requires that something (*p*?) be embedded in a community with certain

<sup>10</sup> Alison Jaggar, *Feminist Politics and Human Nature* (Lanham, Maryland: Rowman / Littlefield, 1983), 40-8; Daniel Hicks, “Rawls’ Rationalist Conception of Personhood” (unpublished), <http://www.nd.edu/~dhicks1/writing/rationalism.pdf> (accessed May 17, 2011).

features, and so on. An enormous amount of time and paper has been wasted on exchanges in which a non-philosopher argues for relativism about justification and a philosopher argues against relativism about truth, both in terms of relativism about “truth.”

I wholly endorse a pluralistic approach to standards for excellent academic writing: scholars in literature and culture studies have a very different set of projects from scholars in analytic philosophy, and our different writing styles can and should reflect this. I do not think, for example, that Donna Haraway should write like G.A. Cohen. Nor do I think the work of non-philosophers should be dismissed as nonsense or incomprehensible when read by analytic philosophers and other scholars who have absolutely no training in how to read that style of writing.

But the pluralism of the last two paragraphs does not seem to be shared by many of my fellow analytic philosophers. I therefore believe that this shows a need for intellectuals who can present and argue for reasonable but marginalized ideas according to the standards of rigor of mainstream analytic philosophy. While some transactionists have done this,<sup>11</sup> I feel that there is still some room for improvement, and of course one more body on the pile can help achieve a critical mass. Hence, in this dissertation, I have endeavored to argue in the language of and according to the standards that I take to be widely accepted among mainstream analytic philosophers: clearly presented arguments that aspire to deductive validity and that use strictly defined terminology.

Despite the way we usually teach logic, I do not believe that all good arguments are deductively valid arguments. Inductive and abductive arguments (of various kinds) also provide good, though fallible, support for their conclusions. Another common kind of argument is what Gary Gutting calls *persuasive elaboration*.<sup>12</sup> Gutting introduces this term in his analysis of the work of several of the most influential twentieth-century analytic philosophers: Quine, Kripke, Gettier, Goldman, Lewis, Plantinga, and Rawls. Each, Gutting claims, did not present deductively valid arguments for precisely-stated claims so much as more-or-less vague appeals to “intuition” to muster support for a “picture” – a line of philosophical inquiry. Further, this line of inquiry is “a highly complex systematic enterprise, sketching a series of interacting and mutually supporting philosophical pictures on a striking range of topics.”<sup>13</sup> In Kuhnian terms, these pictures were taken up by philosophers because each was fruitful, had a broad scope, and either resolved or dissolved the anomalies of its predecessor.

When the claim is put in these Kuhnian terms, I take it to be at least highly plausible that fruitfulness and scope provided some initial support to the key claims of each research program. More generally, I assume, the persuasive elaboration of a

<sup>11</sup> See, especially, Helen Longino, *The Fate of Knowledge* (Princeton: Princeton University Press, 2002).

<sup>12</sup> Gary Gutting, *What Philosophers Know: Case Studies in Recent Analytic Philosophy* (Cambridge, UK and New York: Cambridge University Press, 2009), 77ff, et al.

<sup>13</sup> *Ibid.*, 77.

picture – done systematically and with care – provides a rigorous, though extremely fallible and therefore preliminary, argument for the key claims of that picture.<sup>14</sup>

Closely related to persuasive elaboration is what I call a *pragmatic argument* for a conceptual framework. In a pragmatic argument, one infers from a premise that the conceptual framework can be utilized in a (partial) solution of one or more important problems to the conclusion that the conceptual framework is true (or at least should be adopted for further research, etc.). Abductive arguments may be one species of pragmatic argument, though I am far from confident about this.

My overall arguments in this dissertation are a persuasive elaboration of and pragmatic arguments for the picture that I call the conception of practice and, especially in chapters 4-6, the more specific conception of science as practice. That is, I show how the loose set of concepts deployed by MacIntyre throughout his work can be systematically developed, refined, extended, and then utilized to take up some important problems. This provides some support to the conception of practice, but – since the support is extremely fallible – this dissertation can be, at best, just a starting point.

#### 1.2.4 Left liberal politics

This point is less a methodological point and more a point about the social location from which I am writing. It is a commonplace among many fields of contemporary social science that every researcher comes at his or her object of study from a particular social location — a gender, a sexual orientation, a class, an ethnicity, a religious background, a nationality, and so on — and that this influences his or her work in various ineliminable ways. Indeed, some of the most important elements of the conception of practice support a version of this insight, and the misunderstandings and miscommunications that this social locatedness produces will be a major theme of this dissertation.

One or two aspects of my own social location have had an especially noticeable influence on this dissertation, and so it seems appropriate to mention them here. First, I have deep commitments to and familiarity with various strains of leftist and egalitarian politics, especially radical feminism, ecocentric environmentalism, and democratic socialism. I find myself drawn to both relatively moderate egalitarian liberalism or social democracy — as exemplified by John Rawls — and relatively radical left-wing politics — as exemplified by Iris Marion Young. In chapter 7, I attempt to reconcile the tensions between these two strains; whether I’m successful or not, we might call my rather syncretic set of commitments left liberalism, to distinguish it from both not-necessarily-liberal democratic socialism and not-necessarily-radical egalitarian liberalism. Second, most of my working life has been spent in academia; I have

<sup>14</sup> Note that I’ve introduced a distinction between *rigorous* and *fallible* methods (arguments, etc.) here. Gutting notes that “persuasive elaboration cannot, of itself, establish the truth of a picture . . . at best [it] shows that there is some plausibility that this might be the case.” Gutting, *What Philosophers Know*, 78n5

had only one “normal” job in my life, and that was a stint at a pizza place the summer after my first year of college that lasted all of two months. With these two features combined, I have a deep-seated suspicion of and profound lack of familiarity with corporate workplaces and culture. This is why the overwhelming majority of my examples are taken from, for example, the work of feminists and socialists of various stripes, and the few representatives of corporations that do appear are, let’s say, not wearing the white hats.

I have tried to handle the unfamiliar kinds of cases with some care, but no doubt I have fallen short in several instances. I would ask my conservative and right-wing readers to take these instances as invitations to dialogue rather than challenge or dismissal: If you believe that a large multinational corporation really can be a practice, then let’s talk about what its internal goods might be. If you do not find my examples compelling, then perhaps there are examples from your own life that might take their place.

It is unavoidable that we come to inquiry from a particular social location. It is not inevitable that we remain confined to that location forever.

### 1.2.5 Non-ideal theory

When I began this project, I was under the impression that many philosophers of science were still Ayerean noncognitivists about moral judgment; the results of the PhilPapers survey of professional philosophers have happily proven me wrong about this.<sup>15</sup> But it is still quite common to find philosophers of science speaking of “social values” and “ethical values” in oversimplified ways that demonstrate a lack of contact with contemporary work in ethical theory, political philosophy, practical reason, and moral psychology – “ethics” for short.

After discussing this disconnect with a handful of other philosophers of science who work on science and values, I believe a significant part of the problem is the perception among philosophers of science that most contemporary ethics is all but irrelevant to the kinds of issues the philosophers of science are interested in. Longstanding esoteric debates between deontologists and utilitarians seem beside the point when one cares about the causes and effects of endless public controversies over climate change. Consequently, when philosophers of science do go to the trouble of applying ethics to scientific issues, the results either bear little resemblance to the original ethical theory, are still quite distant from the concrete issue at hand, or both.<sup>16</sup>

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<sup>15</sup> Only 27.5% of target faculty respondents who listed General Philosophy of Science among their areas of specialization also identified as noncognitivists about moral judgment. Less hearteningly, 49.4% of these respondents identified as moral antirealists.

<sup>16</sup> To be clear, I do not think that philosophers of science are individually culpable for the lack of familiarity with more relevant work in ethics. There is a deep-seated, even systemic, tendency towards the proliferation and isolation of sub-disciplines, in philosophy as well as science.

Philip Kitcher's *Science, Truth, and Democracy* is a prime example of “both.”<sup>17</sup> In chapter 10 of this book, Kitcher introduces an elaborate, highly idealized procedure for science policy that he calls “well-ordered science.” Kitcher claims that his procedure draws on John Rawls's conception of justice as fairness.<sup>18</sup> But it's difficult to find any even modestly specific similarities between Kitcher and Rawls: they both involve a hypothetical procedure of representative individuals making choices, and that's about it. Kitcher might just as well be drawing on Thomas Hobbes' version of the social contract or John Harsanyi's original position argument for average utilitarianism.<sup>19</sup> Further, he never suggests how this procedure might address any of the urgent issues he identifies elsewhere, such as (in chapter 8) whether research into sex-based differences in innate intelligence should be undertaken. Indeed, he seems to recognize that it is all but impossible to determine what the outcome of his procedure would be, much less actually carry it out.<sup>20</sup> By the end of the book, it's not at all clear how Kitcher's proposal might be operationalized.<sup>21</sup>

Underlying this problem of irrelevance and perceived irrelevance, I suggest, is a common emphasis on *ideal theory* in much of ethics, and the consequent belief by philosophers of science that ideal theory is more-or-less all that ethicists do. In this dissertation, by contrast, I focus on developing *non-ideal theory*.

The contemporary distinction between ideal and non-ideal theory is often attributed to John Rawls, though in certain respects it can be traced back to at least

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<sup>17</sup> Philip Kitcher, *Science, Truth, and Democracy* (Oxford and New York: Oxford University Press, 2001).

<sup>18</sup> *Ibid.*, 211.

<sup>19</sup> John Harsanyi, “Review: Can the Maximin Principle Serve as a Basis for Morality? A Critique of John Rawls' Theory,” *American Political Science Review* 69, no. 2 (June 1975): 594–606.

<sup>20</sup> Kitcher, *Science, Truth, and Democracy*, 123.

<sup>21</sup> In two recent books, Kitcher has elaborated this procedure and the general “ethical project” in which it is embedded, and seems to be sensitive to the critique that the earlier version was not operationalizable. This version of the account resembles, if anything, Jürgen Habermas's discourse ethics, but Kitcher does not give any indication of even having heard of Habermas. Philip Kitcher, *Science in a Democratic Society* (Amherst, NY: Prometheus Books, 2011), ISBN: 9781616144074; Philip Kitcher, *The Ethical Project* (Cambridge, MA and London: Harvard University Press, 2011), ISBN: 9780674061446.



Karl Marx and arguably even to Machiavelli.<sup>22</sup> As I will understand it here, ideal theory is a philosophical method in which the primary aim is an account of the *perfected state* of the subject matter in question. So, within political philosophy, ideal theory aims to produce an account that deals with a perfectly just (“well-ordered”) society. Much of twentieth-century philosophy of science also qualifies as ideal theory, insofar as it takes as its primary aim an account of epistemologically perfect representational knowledge. The *characteristic claim* of ideal theory is that such an account is necessary for achieving progress. For example, A. John Simmons has argued that we must first find out what a perfectly just society would look like before assessing the defects of our own society and working to improve things.<sup>23</sup>

Non-ideal theory, by contrast, is a philosophical method in which the primary aim is an account of the actual, *imperfect state* of the subject matter in question. Within political philosophy, non-ideal theory aims to produce a tentative account of the major injustices of our actual society. This is the case with Charles Mills’s critical race theory, for example. In *The Racial Contract*, he gives an account of the racial-economic-political injustices of the last five hundred years, arguing that these produced the racial-economic-political injustices of, for example, the contemporary United States.<sup>24</sup> The *characteristic claim* of non-ideal theory is that such an account is necessary for understanding why our actual society has these particular features (both positive and negative) and what must be done to remove or rectify the problems. In the terminology that I introduce in §2.4, ideal theory often assumes a telic conception of progress and non-ideal theory often assumes a melioric conception of progress.

Non-ideal theorists are often explicit that their accounts are intended to be only *partial* and *tentative*. Mills does not claim that racial injustices are the *only* injustices;

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<sup>22</sup> John Rawls, *A Theory of Justice*, revised edition (Oxford and New York: Oxford University Press, 1999), 7-8, ISBN: 9780674000780. *The German Ideology* can, in my terminology, be understood as a simultaneous pragmatic, non-ideal, and empiricist critique of mid-nineteenth century German philosophy. Marx and Engels write, for example, that

The premises from which we begin are not arbitrary ones, not dogmas, but real premises from which abstraction can only be made in the imagination. They are the real individuals, their activity and the material conditions under which they live, both those which they find already existing and those produced by their activity. These premises can thus be verified in a purely empirical way. Karl Marx and Friedrich Engels, *The German Ideology*, ed. C.J. Arthur (New York: International Publisher’s Co., 1970), 42

<sup>23</sup> A. John Simmons, “Ideal and Nonideal Theory,” *Philosophy and Public Affairs* 38, no. 1 (January 2010): 5–36.

<sup>24</sup> Charles Mills, *The Racial Contract* (Ithaca: Cornell University Press, 1997).

he recognizes, for example, gender and class injustice.<sup>25</sup> Hence he expects that, in time, his account will come to be replaced by one that includes these other forms of injustice. His account is tentative in that he expects it to be replaced; it is partial in that it leaves out certain things that it might otherwise be expected to include. By contrast, many ideal theorists seem to intend – or at least aspire – to give *total* and *complete* accounts: total in that they cover all aspects of the subject matter, and complete in that they do not require significant revision.<sup>26</sup> The characteristic claim of ideal theory seems to require this: if our account of a perfectly just society is (merely) partial and tentative, then why should we use it to evaluate our approach to perfect justice?

Common examples of non-ideal theory in political philosophy are negative, in the sense that they focus more-or-less exclusively on imperfections, injustices, and so on. After all, Mills’s basic criticism of ideal theory is not just that it fails to produce (or does not aim to produce) an account of “actual historic oppression and its legacy in the present, or current ongoing oppression,” but further that in doing so it effectively produces a rationalization or apology for this injustice.<sup>27</sup> Correspondingly, his non-ideal theory focuses on exactly these actual historic and ongoing injustices.<sup>28</sup> However, non-ideal theory need not be exclusively negative.<sup>29</sup> Non-ideal theory can give a frank assessment of both the good and bad aspects of the status quo: in the US we’ve made significant progress with respect to racial, gender, and sexual injustice, for example, though there are still some outstanding issues and new problems, and in such respects as economic equality and our treatment of non-human animals we may be doing worse.

I suggest that philosophy of science has largely – though far from completely – abandoned its own aspirations for ideal theory. For much of the twentieth century, many philosophers of science (especially those who were most prominent and influential after World War Two) aimed to give an account of perfected scientific reasoning, using the tools of mathematical logic, probability theory, and statistics. In the wake of the work of Thomas Kuhn and Paul Feyerabend,<sup>30</sup> however, the focus has shifted: highly technical work in philosophy of science is largely confined to the philosophy of specific sciences (especially physics and biology); and general philosophy of science is, for the most part, based on case studies of actual scientific practice and is

<sup>25</sup> Mills, *The Racial Contract*, 137-8, n.3.

<sup>26</sup> Rawls is a notable exception to this generalization: his work is explicitly limited to what he calls the basic structure of society, and is not total. And, to the extent that he only aspires to approach, not achieve, reflective equilibrium, his account does not aspire to be complete.

<sup>27</sup> Charles Mills, “‘Ideal Theory’ as Ideology,” *Hypatia* 20, no. 3 (Summer 2005): 168.

<sup>28</sup> Mills, *The Racial Contract*; Carole Pateman and Charles Mills, *Contract and Domination* (Cambridge, UK and Malden, MA: Polity Press, 2007).

<sup>29</sup> See, for example, Amartya Sen, *The Idea of Justice* (Cambridge, MA and London: Harvard University Press, 2009), ISBN: 9780674036130.

<sup>30</sup> I do not claim causation here; see §3.1.



concerned with, say, the theory-evidence relation that is at work in this and similar cases rather than a perfected and universal theory-evidence relation. For example, of the seventy-seven articles presented at the 2008 biennial meeting of the Philosophy of Science Association and subsequently published in *Philosophy of Science*,<sup>31</sup> I count sixty (approximately 78%) that either dealt with specific philosophy of science or adopt a case-study method. None of these sixty, as far as I have checked, purports to offer anything like a total and complete account of even the particular case that they are investigating, and many of the other seventeen appear to be similarly modest. In short, all appear to be engaged in non-ideal theory.

If it is right to say that non-ideal methods are in widespread use in philosophy of science, then I suggest further that this is best explained by a combination of (a) the widespread belief (among philosophers of science) that philosophy of science should be engaged with ongoing scientific practice and (b) the inadequacy of ideal theory for this sort of engagement. That is, in general, contemporary philosophers of science aim to understand (and, indeed, *assist*) ongoing scientific inquiry, and this cannot be done with, say, purely formal models of confirmation based on classical probability theory.

By contrast, non-ideal theory is still controversial in ethics and political philosophy. I can think of only three contemporary political philosophers who have written major works of non-ideal theory and who may be widely known among other philosophers: Peter Singer, Amartya Sen, and Martha Nussbaum. Singer's work has mostly been on specific issues of applied ethics (as opposed to providing a generally applicable framework) and Sen and Nussbaum's work – much of it on poverty, the status of women in impoverished communities, and religious conflict – is not obviously applicable to the sorts of problems with which I started this chapter. Consequently, when philosophers of science turn to ethics and political philosophy for help grappling with these issues, they expect and are unable to find non-ideal theory. And thus the perception mentioned above: philosophers of science think that contemporary ethics is all but practically irrelevant. I hope here to correct that misperception, by developing a non-ideal conception of scientific inquiry and the threat it may or may not face from other social organizations.

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<sup>31</sup> *Philosophy of Science* 76, no. 5 (December 2009); *Philosophy of Science* 77, no. 5 (December 2010).



# Chapter 2

## A conception of practice

### 2.1 Introduction

MacIntyre most famously develops his conception of practice in chapter 14 of *After Virtue*. He first presents it as follows:

By a “practice” I am going to mean any coherent and complex form of socially established cooperative human activity through which goods internal to that form of activity are realized in the course of trying to achieve those standards of excellence which are appropriate to, and partially definitive of, that form of activity, with the result that human powers to achieve excellence, and human conceptions of the ends and goods involved, are systematically extended.<sup>1</sup>

MacIntyre gives several examples of groups or families of practices: chess and other complex games; the arts; natural and social sciences. Since the purpose of the next chapter is to argue that scientific inquiry is a practice according to this conception, the bulk of my examples here will instead come from the arts.

Much of my discussion of this conception of practices, §§2.3-2.4, will deal with two of its crucial components: the set of “goods internal to” practices and the set of “standards of excellence” for those goods. I then turn, in §2.5, to the relation between practices and institutions. In the last section, §2.6, I consider the relations between practices and the basis for providing a justification of one practice to the members of another.

Throughout this chapter, my proximate goal is exegetical: I am trying to systematize and clarify MacIntyre’s conception of practice. My more ultimate goal, however, is a conception of practice that can be applied to address the kinds of problems discussed in the introduction. So, at a few points, I make friendly additions

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<sup>1</sup> Alasdair MacIntyre, *After Virtue*, second edition (Notre Dame: University of Notre Dame Press, 1984), 187, ISBN: 9780268006112.

and amendments or reject an assumption of MacIntyre's, all in order to make the conception more appropriate for my purposes. These should be obvious in each case.

## 2.2 Practitioners

First and foremost, MacIntyre says, practices are “form[s] of socially established cooperative human activity.”<sup>2</sup> While I would quibble with restricting the definition to humans — MacIntyre himself appears to think that dolphins, for example, engage in practices or something quite similar — the characteristically social nature of practices is fundamental.<sup>3</sup> This does not mean that the activities of a given practice cannot possibly be performed by an individual person, acting in isolation and without training, such as outsider or “naïve” artists. Instead, by calling practices *characteristically* social, I mean that they are *generally, for the most part, or typically* engaged in by either collective agents — a whole workshop of sculptors — or by individuals who have been more-or-less formally trained in the activities of the practice — painters who have attended an academy of art — or have regular contact with other practitioners — photographers who regularly share and discuss their recent work.

A practice thus characteristically has a set of *practitioners*, the individuals who participate in the practice by engaging in its activities. The practitioners of a given practice are connected through an intricate web of relations: teacher and student; commentator and critic; colleagues; professional rivals; and so on. These networks may be hierarchical or with strict demarcations among different kinds of practitioners — as in the Catholic church — or be thoroughly egalitarian — as in contemporary photography. But in either case practitioners will characteristically be associated with more-or-less well-defined social roles — teacher, student, artist, commentator, priest, parishioner, and so on — and the organized community of practitioners is characteristically best understood as a group or collective agent with some shared aims.

There are several rival accounts of collective agents, and the social roles that they comprise, in the philosophical and political theoretic literature;<sup>4</sup> my impression as a

<sup>2</sup> MacIntyre, *After Virtue*, 187.

<sup>3</sup> On dolphins, see Alasdair MacIntyre, *Dependent Rational Animals* (Chicago: Open Court Publishing Company, 2001), ch. 3, ISBN: 9780812694529.

<sup>4</sup> For overviews of this literature and numerous citations, see Abraham Sesshu Roth, “Shared Agency,” *The Stanford Encyclopedia of Philosophy* (Spring 2011 Edition), 2011, <http://plato.stanford.edu/archives/spr2011/entries/shared-agency/> (accessed September 30, 2011); Seumas Miller, “Social Institutions,” *The Stanford Encyclopedia of Philosophy* (Spring 2011 Edition), 2011, <http://plato.stanford.edu/archives/spr2011/entries/social-institutions/> (accessed September 30, 2011); two recent accounts include Christian List and Philip Pettit, *Group Agency: The Possibility, Design, and Status of Corporate Agents* (Oxford and New York: Oxford University Press, 2011), ISBN: 9780199591565; Tracy Isaacs, *Moral Responsibility*

non-expert in this area is that each account has some benefits and disadvantages vis-à-vis the others, that there is no clear best account, and that any of the accounts can support an account of social roles and their aims. While my views on individualism will incline me away from some of these, this inclination is not strong enough to lead me to prefer any one account for the purposes of this chapter.<sup>5</sup> So, at this point, it seems safe to suggest that the reader turn to her favorite account of collective agency to provide the missing account of social roles, with one caveat.

There may be strict distinctions between practitioners and non-practitioners — as in mystery cults — or the boundary may be thoroughly fraught with vagueness — as with many contemporary arts. I suggest that, typically, being a practitioner admits of degree, so that one can be a practitioner more or less. In some cases, we will want to consider practitioners in an extensive sense, including even those who have a very slight familiarity with the practice; but usually we will understand the term in a much more limited sense, so that only those who have some long-term familiarity with or commitment to the practice count as practitioners.

Strictly speaking, a practice is the activity of its practitioners. However, for simplicity, I will often refer to the set of organized practitioners itself, synecdochically, as the practice.<sup>6</sup>

## 2.3 Goods

Practices are *goal-oriented* activities; the activity is not random or willy-nilly, but instead designed to achieve some aim or produce some end. These ends, goals, or aims are the “goods” of MacIntyre’s definition. In this section and the following, I develop the account of the goods of a practice and the “standards of excellence” for those goods. This account includes several important distinctions. In this section, I lay out the primary distinctions. I leave the bulk of my discussion of one specific internal good, progress, for §2.4.

### 2.3.1 Internal and external goods

The first and most important distinction is between *internal goods* and *external goods*.<sup>7</sup> MacIntyre characterizes external goods explicitly by the following two features:

- (1) they are “externally and contingent attached to” the practice “by the accidents of social circumstances”; and

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*in Collective Contexts* (Oxford and New York: Oxford University Press, 2011), ISBN: 9780199782963.

<sup>5</sup> See §7.4.

<sup>6</sup> I thank Paul Weithman for pointing out this equivocation.

<sup>7</sup> My official definitions of these terms are given below, p. 22.

- (2) there are “always alternative ways for achieving such goods, and their achievement is never to be had *only* by engaging in some particular kind of practice.”<sup>8</sup>

MacIntyre gives the examples of “prestige, status and money,” and elsewhere characterizes both fame and pleasure in a Benthamite sense as external goods.<sup>9</sup> There are multiple, very different ways to acquire wealth or power; they are not tied to any specific form of activity. Marx makes a similar point:

Because money is the metamorphosed shape of all other commodities, the result of their general alienation, for this reason it is alienable itself without restriction or condition. It reads all prices backwards, and thus, so to say, depicts itself in the bodies of all other commodities, which offer to it the material for the realisation of its own use-value. At the same time the prices, wooing glances cast at money by commodities, define the limits of its convertibility, by pointing to its quantity. Since every commodity, upon becoming money, disappears as a commodity, it is impossible to tell from the money itself, how it got into the hands of its possessor, or what article has been changed into it. *Non olet*, from whatever source it may come.<sup>10</sup>

And as Nozick’s experience machine thought experiment shows, there are always alternative ways of experiencing Benthamite pleasure or subjectively satisfying one’s preferences.<sup>11</sup>

Internal goods, on the other hand, are characterized directly by the following three features:

- (1) they “cannot be had in any way but by [engaging in the practice] or some other [practice] of that specific kind”;
- (2) “we can only specify them in terms of [the practice] or some other [practice] of that specific kind and by means of examples from such [practices]”; and
- (3) “they can only be identified and recognized by the experience of participating in the practice in question.”<sup>12</sup>

It is clear that the second feature of external goods corresponds to the first feature of internal goods: internal goods are *attached* to their practices in the sense that

<sup>8</sup> MacIntyre, *After Virtue*, 188-9; his emphasis.

<sup>9</sup> *Ibid.*, 190, 198.

<sup>10</sup> Karl Marx, *Capital: A Critique of Political Economy*, reprint of first English edition, ed. Samuel Moore and Edward Aveling (Progress Publishers / Marxists.org, 2010), bk. 1, part 1, 1.3.2, pg. 74, <http://www.marxists.org/archive/marx/works/1867-c1/index.htm>.

<sup>11</sup> Robert Nozick, *Anarchy, State, and Utopia* (Basic Books, 1974), 42-5.

<sup>12</sup> MacIntyre, *After Virtue*, 188-9.

these internal goods can only be produced by these particular activities, while there are other ways to produce external goods. A painting can only be produced by the activity of painting; a photograph can only be produced by the activity of taking it. The second feature of internal goods also attaches internal goods to practice, in the distinct sense that these goods can only be defined by reference to the practice. A painting (the work of art) may be defined as pigment on a flat surface, but this definition fails to distinguish *Guernica* from a whitewashed factory wall; instead, a painting is better defined as the product of the activity of painting or of painters as such. Similarly, a purely mechanical definition of a photograph would fail to distinguish a mug shot from *Moon and Half Dome*.

When we classify an activity as a practice or not, we will do so by asking whether there are any goods that are attached to the activity — whether it has any internal goods. This may lead us to believe that just about any activity can qualify as a practice. Consider the activities of a corporate executive. It's entirely intelligible to say that such activities can be done well or poorly, to label some well-done activities with names (leadership, management, negotiating skill, and so on), and to claim that such things can be realized only by engaging in corporate executiveship. But these things are not the aims or ends of the activities of the executive. The aim of the activities of the corporate executive (as such) is the continued financial succession and expansion of the corporation — in a word, wealth — and this, I take it, is the sole aim of these activities. (Not knowing much about the culture of corporate executives, I grant that I may be wrong about this last point: Perhaps corporate executives are generally regarded as succeeding — achieving their aims as executives — if, for example, they are excellent leaders and negotiators but consistently fail to return a profit. I am rather doubtful about this, but I do acknowledge the possibility. If I am wrong, then it is a bad example: it would seem that corporate executiveship is a practice after all.) Similarly, consider a mid-level bureaucrat in some government agency. It's entirely intelligible to identify some specific activities or attributes that can only be realized by an excellent bureaucrat, such as a degree of generosity towards subordinates and efficiency in finishing paperwork. But these are not the aims of the activity of a bureaucrat, as such; rather, their aim is something like the management of the flow of power through their part of the hierarchy.

A more interesting case is that of an entrepreneur or small business owner. While the small business owner aims to make some profit — assuming all is going well, of course — it might be plausible to maintain that she aims at other things as well, such as thrift, hard work, providing certain goods and services to the rest of the community, and a certain kind of relationship with her employees and customers (many of whom will also be personal friends) and the community as a whole (for example, by sponsoring local charities) that we might call *petty bourgeois neighborliness*. It might also be plausible to maintain that these other aims can only be achieved by the sorts of activities that compose being a small business owner, and that an excellent small business owner will be one who is willing, at least sometimes, to sacrifice profit for

the sake of these other aims. For example, she will not exploit her employees or gouge or cheat her customers, even if doing so means reducing the bottom line. But then it seems plausible to maintain that entrepreneurship is a practice.

This last point seems to contrast sharply with MacIntyre's view on businesses. He clearly believes that the sole aim of a business is the external good of wealth and therefore a business cannot be a practice.<sup>13</sup> I suggest that he has in mind, primarily, the corporate executive rather than the entrepreneur. The corporate executive does, I think, more-or-less aim exclusively at wealth, and is characteristically willing to sacrifice anything for this aim. Her activities, therefore, cannot be a practice. By contrast — and assuming the characterization of the last paragraph is accurate — the entrepreneur aims at more-and-other than wealth, and is not willing to sacrifice these other things for his aim. His activities, therefore, could be considered a practice. But, of course, much of the wealth and power of our economy is in the hands of the corporate executives; very little is in the hands of the entrepreneur. Hence, with respect to institutional domination, business-as-done-by-the-corporate-executive is more important than business-as-done-by-the-entrepreneur.<sup>14</sup>

The discussion of the last few paragraphs suggests the following definitions: *Internal goods* are aims or ends of social roles that are attached to the those roles. *External goods* may also serve as aims or ends of social roles (though they need not do so), but are *not* attached to these roles in any case; the corporate executive aims at wealth, but so do other roles that have nothing at all to do with corporate executiveship. There may also be things (properties, and so on) attached to social roles that are not aims; these are neither internal nor external goods.

The third feature of internal goods is the most significant, but also the most ambiguous.<sup>15</sup> I shall call this feature the *recognition claim*, as on the first pass it is the claim that *internal goods can only be recognized by practitioners*. There are several ways we might try to understand “be recognized.” For my purposes, the most important such way is as *justification as good*. That is, the recognition claim is the claim that the claim that internal goods are good or valuable or worth pursuing can only be justified by practitioners or on the basis of participation in the practice. Or, more formally, using some terminology that I will define in §2.6:

**recognition:** An individual *a* can give or accept a justification of a practice *A* only if *a* is a participant of *A*.

<sup>13</sup> See Ron Beadle, “Why Business Cannot Be a Practice,” in *Revolutionary Aristotelianism: Ethics, Resistance and Utopia*, ed. Kelvin Knight and Paul Blackledge, vol. 30, 1 (2008), 229–41 and Russell Keat, “Ethics, Markets and MacIntyre,” in *Revolutionary Aristotelianism: Ethics, Resistance and Utopia*, ed. Kelvin Knight and Paul Blackledge, vol. 30, 1 (2008).

<sup>14</sup> See §2.5.

<sup>15</sup> It is also made, in terms of only recognition, in Alasdair MacIntyre, “Objectivity in Morality and Objectivity in Science,” in *Morals, Science and Sociality*, ed. H. Tristram Englehardt, Jr. and Daniel Callahan (Hastings-on-Hudson: Hastings Center, 1978), 28.



The recognition claim, clarified in this way, may seem absurd. Obviously non-musicians can explain why music is good and valuable, and non-painters can justify the continued activity of painting.<sup>16</sup> To some extent, I suggest, this is due to the familiarity of the example, especially for the highly educated Western readers of this dissertation. Most of us have had to take at least one college class in Western art history, and probably almost all of us have taken numerous trips to museums that featured (sometimes literally) examples of excellent portraiture. While our familiarity with the practice is limited, we do have some familiarity, and might be considered practitioners in a very attenuated sense.

The recognition claim can be motivated by considering an activity that is probably much less familiar than the examples given above: the performance of *aarti*, a Hindu ritual involving the burning of a oil lamp in front of a representation of a deity while singing songs of praise. As non-practitioners, complete outsiders to this activity, we may be able to give an accurate physicalist description of the ritual — the worshippers gathered in this location at this particular time, dressed in these clothes, prepared a lamp made of this metal with that fuel, sang these words, and so on. But we would not be able to describe the spiritual significance of the ritual, to identify which features are important for performing the ritual correctly, or to explain the importance or value of the ritual. Indeed, our naïve description may completely skip certain physicalist details — perhaps the way in which the fuel was prepared prior to the start of the ritual, or certain decorations on the representation, or the time of day — that are extremely important for correct performance. By contrast, if we abandoned our status as outsiders and became involved in the practice of Hinduism, we would be able to better identify some of those overlooked physicalist details of the rituals and use some of the terminology of the practice to describe the spiritual significance of performing the ritual well, even after a short amount of time. However, we can imagine that our use of this terminology is clumsy and inadequate, and we would still be unable to explain why the correct performance of this ritual is a good thing. The richest description and best justification of these internal goods would only be available to those who have spent the bulk of their lives immersed in the theology (in a sense appropriate for Hinduism) of the religious practice.

Even if this example makes the recognition claim much more plausible, it does not deal with the objection, that in many cases non-practitioners can recognize the internal goods of a given practice. The objection arises from considering how a practice “looks” from “outside,” that is, to someone who is not a practitioner. Dealing with it therefore requires terminology to describe the relationships between distinct practices. Since this terminology will be introduced in §2.6, we will set the objection to one side now and pick it up again later. My solution will introduce some extensions of MacIntyre’s original conception of practice — extensions that be quite useful in

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<sup>16</sup> Numerous commentators have made versions of this objection. Especially useful ones, for clarifying this discussion, have come from Don Howard, Paul Weithman, and Peter Wicks.

later chapters — and a weaker recognition claim.

MacIntyre never gives an explicit argument for any version of the recognition claim. In §2.6, I will discuss one of the major purposes that this claim has in his views on practical reason; here I will sketch an argument, then consider textual evidence for attributing it to MacIntyre.

This argument relies on Michael Polanyi’s conception of *tacit knowledge*; so we might as well call it the *argument from tacit knowledge*. Tacit knowledge is knowledge that an epistemic agent has but that she cannot articulate or explain to others. Polanyi’s first example is facial recognition: “We know a person’s face, and can recognize it among a thousand, indeed among a million. Yet we usually cannot tell [to someone else] how we recognize a face we know. So most of this knowledge cannot be put into words.”<sup>17</sup> To be still more concrete, consider recognizing two photographs of a child and an adult as the same person. When asked to explain how we recognized the same person, we might say that we saw “the same eyes” or “the same dimples.” But, of course, there was much, much more than this. Similarly, when we identify “cases of diseases and specimens of rocks, of plants and animals,”<sup>18</sup> we deploy criteria and standards that go well beyond what we can articulate linguistically.

I suggest that, generally and for the most part, applying sufficiently complex standards or criteria to a given case requires tacit knowledge. And recognizing something as an internal good of a given practice requires applying quite complex standards and criteria. Thus, generally and for the most part, recognizing internal goods requires tacit knowledge.

Now, because tacit knowledge cannot be articulated, it cannot be communicated linguistically, at least not completely adequately. If I am trying to teach you to distinguish flat, sharp, and on-pitch tones, I can describe various examples as “high” or “low,” and these descriptions will probably help you acquire the knowledge faster than otherwise. But you will not be able to identify flat tones just because I have told you that flat tones are low. Instead, communicating or teaching tacit knowledge requires more active student participation in the learning process, which Polanyi calls “interiorization” or “indwelling.” Paradigmatically, the student observes the teacher apply the criteria to several example cases; then the student tries to apply the criteria herself, with corrections from the teacher. For example, I might identify the intonation of several tones as flat or on-pitch, then have you do the same with several tones. Superficially more complex cases are actually similar: first I demonstrate a particular fingering technique while you observe; then you try to do the same; and I identify the places where you did and didn’t do it well. At least, this seems to be what Polanyi has in mind:

The performer co-ordinates his [sic] moves by dwelling in them as parts of his body while the watcher tries to correlate these moves by seeking

<sup>17</sup> Michael Polanyi, *The Tacit Dimension* (University of Chicago Press, 1966), 4.

<sup>18</sup> *Ibid.*, 5.

to dwell in them from outside. He dwells in these moves by interiorizing them. By such exploratory indwelling the pupil gets the feel of a master's skill and may learn to rival him.<sup>19</sup>

If this is right, then, paradigmatically, learning tacit knowledge relevant to an activity *A* requires actively participating in *A*. In light of the argument of the last two paragraphs, we conclude that learning to recognize the internal goods of a practice *A* requires engaging in *A*, that is, being a practitioner of *A*. And this last is the recognition claim.

MacIntyre himself never gives this argument, but there is some evidence that he was familiar with Polanyi's work in the philosophy of science, and possibly even the conception of tacit knowledge in particular. "Epistemological Crises, Dramatic Narrative and the Philosophy of Science" was published in 1977, about four years prior to the first edition of *After Virtue*, eleven years after Polanyi's *The Tacit Dimension*, and nearly twenty years after Polanyi's first development of the concept of tacit knowledge in *Personal Knowledge*. In "Epistemological Crises," MacIntyre argues that Thomas Kuhn's account of normal science is "largely indebted" to Polanyi:

What Polanyi had shown is that all justification takes place within a social tradition and that the pressures of such a tradition enforce often unrecognized rules by means of which discrepant pieces of evidence or difficult questions are often put on one side with the tacit assent of the scientific community. Polanyi is the Burke of the philosophy of science and I mean this analogy with political and moral philosophy to be taken with great seriousness . . . . Polanyi, like Burke, understands tradition as essentially conservative and essentially unitary . . . . It is because of this that anyone [namely, Kuhn] who took Polanyi's view would find it very difficult to explain how a transition might be made from one tradition to another or how a tradition which had lapsed into incoherence might be reconstructed.<sup>20</sup>

While MacIntyre does not cite any of Polanyi's works by name, his reading of Polanyi best resembles the "Burkeanism" of either *Personal Knowledge* or *The Tacit Dimension*. For example, late in *The Tacit Dimension*, Polanyi gives the following argument:

All I have said implied my repudiation of the grounds on which the absolute intellectual self-determination of man was proclaimed by the great philosophic movement engendered by the Enlightenment. For to acknowledge tacit thought as an indispensable element of all knowing . . . . is to

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<sup>19</sup> *Ibid.*, 30.

<sup>20</sup> Alasdair MacIntyre, "Epistemological Crises, Dramatic Narrative and the Philosophy of Science," *The Monist* 60 (1977): 16.

deny the possibility that each succeeding generation, let alone each member of it, should critically test all the teachings in which it is brought up . . . . [I]f we know a great deal that we *cannot* tell . . . then the idea of knowledge based on wholly identifiable grounds collapses, and we must conclude that the transmission of knowledge from one generation to the other must be predominantly tacit . . . .

[T]he pupil must presume that a teaching which appears meaningless to start with has in fact a meaning which can be discovered by hitting on the same kind of indwelling as the teacher is practicing. Such an effort is based on accepting the teacher's authority.<sup>21</sup>

Indeed, “today the ideas of Tom Paine can be saved from self-destruction only by a conscious reaffirmation of traditional continuity. Paine's ideal . . . can be saved from destruction by revolution only by the kind of traditionalism taught by Paine's opponent, Edmund Burke.”<sup>22</sup>

Despite common misreadings of MacIntyre as a conservative,<sup>23</sup> it is clear from the quotations above that he rejects Polanyi's – and Burke's — outlandish, uncritical traditionalism. Still, this does not mean that he completely rejects the concept of tacit knowledge. I suggest that MacIntyre retained — if only subconsciously — an appreciation for the concept of tacit knowledge, and may have had something like the argument from tacit knowledge in mind when he stated the recognition claim in later writing.

Before concluding this subsection, let me point out another difficulty with the distinction between internal and external goods and give a brief argument for the legitimacy of the distinction. First the difficulty: Any claim that some object-type *O* is attached to activity-type *A* in the sense that internal goods are attached to practices is extremely sensitive to the description of *O* and *A*, and especially the level of generality or specificity used in their descriptions. Call this the *description problem*.

Recall the proposed definition of a painting as pigment applied to a flat surface. As I pointed out, by this definition both a whitewashed factory wall and *Guernica* qualify as paintings. But now define the activity of painting with a list of examples: the activities of Picasso, Monet, da Vinci, Rembrandt, and so on. Defined in these ways, paintings (the object-type) are not attached to painting (the activity-type). This in turn suggests that painting (the activity-type) is not a practice, even defined with a list of exemplary painters. On the other hand, consider the good of generating gross revenue of \$15,000 next month *by selling 10,000 widgets*. At this level of specificity, this good is attached to the business of making and selling widgets. It thus appears to be an internal good of the widget business; hence the widget business is a practice.

<sup>21</sup> Polanyi, *The Tacit Dimension*, 60-1.

<sup>22</sup> *Ibid.*, 62-3.

<sup>23</sup> See §2.4.

But this contrasts with the discussion of the corporate executive, as a paradigm of non-practice activity, a few pages above.

Second, I believe that something like the distinction between internal and external goods is a common feature of ordinary practical reason for most, if not all, people. It is a commonplace (among non-economists and non-utilitarians) that you “can’t put a price” on some things: family and friendships, basic freedoms, human life (in a medical context), great works of art, truth. That is, generally we seem to think that there is something at least incorrect, typically absurd, and often deeply offensive about attempts to measure the value of these things in terms of wealth or thinking that they can be freely exchanged for some quantity of wealth. A standard objection to certain versions of utilitarianism — namely, that it is deeply mistaken to think that the value of all goods can be measured in terms of happiness — is similar. In these cases, we are taking the various classes of goods to be *incommensurable*, that is, lacking a common standard of measurement and comparison.

Things like family and friendships, I suggest, are the internal goods of specific practices, and as such are not just incommensurable with the internal goods of other practices (“How many copies of *Guernica* is your child worth?” is as absurd a question as “How much money is your child worth?”) and external goods, but also generally more important than any external goods. Similarly, the value of my relationship with my family cannot be reduced to the happiness I feel when I spend time with them; nor is philosophy superior to sophistry as a more reliable means to accumulating professional and social status. And again, when we accuse a musician or other artist of “selling out,” we are accusing them of sacrificing the internal goods of artistic practices for the external goods of wealth and fame. When reasoning from all of these sorts of premises, we are comparing internal and external goods; in these common situations of practical deliberation, we employ exactly the distinction that I am trying to represent in this section.

These observations and suggestions can be put, more formally, as an abductive argument for the distinction between internal and external goods: the best explanation for the fact that humans reason in these ways is that humans reason with a legitimate (or perhaps even real) distinction between external goods and the internal goods of their various practices; that all these various goods are incommensurable; and that internal goods are, in general, more valuable than external goods. To be clear, my *explanandum* is not that *everyone* reasons in this way *at all times*; it is rather the more modest claim that *many* people reason in this way *often*. This form of practical reason, even if it is not the only way in which we reason, is sufficiently common so as to warrant explanation.

### 2.3.2 Virtues, goods, progress, and eudaimonia

MacIntyre makes a further distinction among several types of internal goods. As I read him, the distinction has four parts, which I call virtues, goods, progress, and

eudaimonia. Note that this terminology is mine, not MacIntyre's, and that there is a potential for confusion between *internal goods*, the genera, and *goods*, one species of internal goods. The goods of a practice are among its internal goods, but not all internal goods are goods in the specific sense. I find this potential for confusion regrettable, but I cannot think of a better alternative. Also, the terminology is not intended to suggest that the goods of any practice are *the* goods, the virtues of any practice are *the* virtues, the conception of eudaimonia for a practice is *the* conception of the good human life *as such*, and so on. While there are some connections between these highly localized, practice-specific internal goods and their ethical namesakes (especially Aristotelean ethics), I chose this terminology to connote these connections, not to denote identities.

Discussing the practice of portraiture, MacIntyre discusses "the excellence of the products, both the excellence in performance by the painters and that of each portrait itself."<sup>24</sup> The first of these are *virtues*, excellences of the actions and character of the painters. The second are *goods*, objects (in this case, material; in other practices abstract or perhaps spiritual) that are distinct from their creators and, in particular, have excellences independent of those of their creator.

Goods are not necessarily more important or central to a practice than virtues; nor vice versa. In *goods-oriented* practices, goods will take priority or be more central to the practice. For example, in the visual and culinary arts, the *primary aim* or *primary internal goods* of the practice are its goods. In *virtue-oriented* practices, by contrast, virtues will be the primary aim. This is the case, for example, in dance, theatre, music, and other performing arts.

MacIntyre is much more vague about the third and fourth types of internal goods:

But it is in participation in the attempts to sustain progress and to respond creatively to problems that the ... [third and fourth] kinds of goods internal to the [practice] is to be found. For what the [practitioner] discovers within the pursuit of excellence in [the internal goods] ... is the good of a certain kind of life. That life may not constitute the whole of life for someone who is a [practitioner] by a very long way or it may at least for a period ... absorb him or her at the expense of almost everything else. But it is the [practitioner's] living out of a greater or lesser part of his or her life *as a [practitioner]* that is the ... [fourth] kind of good internal to [the practice].<sup>25</sup>

The third type of internal good is *progress*, the creative response of the practitioner to "problems" within the practice. MacIntyre's conception of progress is influenced by, though not identical to, those of several others: Kuhn and Lakatos, Aristotle and Aquinas, Hegel and Marx.<sup>26</sup>

<sup>24</sup> MacIntyre, *After Virtue*, 189.

<sup>25</sup> *Ibid.*, 189-90, his emphasis.

<sup>26</sup> For Kuhn and Lakatos, see MacIntyre, "Epistemological Crises, Dramatic Narrative and

A practice is never perfect, and is always faced with internal problems including, for example, inconsistency in the specification of the internal goods and the inability to produce the internal goods. The resolution or dissolution of these problems amount to an internal good of progress. Progress will be discussed in more detail in §2.4.

Finally there is “the good of a certain kind of life.” MacIntyre gives us very little to work with here. I suggest it involves two components or aspects. First, as numerous philosophers have pointed out, human beings often value something for its own sake that was previously valued only for the sake of something else. In MacIntyre’s example of teaching a child chess, for example, the child initially values playing chess only as a means for attaining candy.<sup>27</sup> But the child will gradually learn to value chess for its own sake, independent of any candy reward she receives for winning. MacIntyre uses this example to illustrate the process by which we come to value the internal goods of a practice on their own, and thereby become practitioners. But, of course, these excellences could also be valued for their own sake, independent of the (other) goods they produce. This is the first aspect of this internal good of a certain kind of life.

This first aspect leads to a second. One of MacIntyre’s more recent critiques of contemporary society focuses on what he calls the “compartmentalization of roles,” that is, the “division of contemporary social life into distinct spheres, each with its own highly specific standards of success and failure.”<sup>28</sup> MacIntyre argues that compartmentalization leads to a sort of fragmentation of the individual’s life: one is, at different times and places, a parent, an office worker, a consumer, and a citizen, but never a “unified self” simultaneously enmeshed in all of these relations. Virtues (and here I mean both *the* virtues and the virtues of specific practices), in such a society and applied to such a fragmented self, are understood as merely skills, “more or less effective means to the achievement of those predetermined ends” of each distinct role. By contrast, the conception of practice is meant to support a conception of society and politics, which, when realized, cultivates a unified self — something like “the

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the Philosophy of Science”; Alasdair MacIntyre, *Selected Essays: The Task of Philosophy* (Cambridge: Cambridge University Press, 2006), vii-viii. For Aristotle and Aquinas, see among many others Alasdair MacIntyre, “First Principles, Final Ends, and Contemporary Philosophical Issues,” in *Selected Essays: The Task of Philosophy* (Cambridge: Cambridge University Press, 2006), especially 154ff. For Marx, see Alasdair MacIntyre, “Three Perspectives of Marxism: 1953, 1968, 1995,” in *Selected Essays: Ethics and Politics* (Cambridge: Cambridge University Press, 2006), especially 152, and note the “thesis-antithesis-synthesis” relationship between the major intellectual traditions of *Whose Justice?* and *Three Rival Versions*. In the latter, for example, nineteenth century encyclopedism is the thesis; Nietzschean postmodernism is the antithetical reaction to the failures of encyclopedism; and Thomism is the synthesis that can overcome the problems of both of its rivals.

<sup>27</sup> MacIntyre, *After Virtue*, 188.

<sup>28</sup> Alasdair MacIntyre, “Moral Philosophy and Contemporary Social Practice: What Holds Them Apart?” In *Selected Essays: The Task of Philosophy* (1992; Cambridge: Cambridge University Press, 2006), 117.



unity of a narrative embodied in a single life.”<sup>29</sup> This requires, among other things, that some virtues be transferable — that one and the same virtue can be exercised in multiple practices, and be exemplified by a single unified self in each of these practices. And this transfer, in turn, requires that the virtues in question be at least somewhat independent of the internal goods of each practice.

Next, if (and I acknowledge that this is a very tenuous interpolation) the internal good of a practice of “a certain kind of life” is the same thing as the good of a unified, non-compartmentalized self of the last paragraph, then the certain kind of life involves the transfer of virtues among all of the various practices in which I participate (at least, those virtues that are transferable). This “transfer of virtues” is thus something like a stable or unified character. In discovering and living the certain kind of life associated with my various practices, that is, my actions are consistent as I move from role to role (I exercise the same virtues in each of them), and my character is not compartmentalized. I am a single, concrete, unified person, not a ghost moving from one role to the next, and as a unified person living or doing well in all my various aspects. It then makes sense to call the internal good of “a certain kind of life” *eudaimonia*.<sup>30</sup>

However, this transfer of internal goods among practices may also be understood in a much weaker sense: as the application of the other internal goods of a given practice to some other kind of activity, especially another practice. In what is probably a gross misappropriation of terminology, I will also use the term “*eudaimonia*” to denote this much weaker version of the fourth internal good. Worse, this will be the primary way in which I will use this term throughout the remainder of this dissertation.

MacIntyre does not explain the connection between progress and *eudaimonia*, nor the connection between the first two and last two kinds of internal goods. However, when I introduce the concept of joint practice in §2.6, we will see that the use of the internal goods of one practice to produce the internal goods of another, and hence *eudaimonia* in my diluted sense, is essential for certain kinds of progress. MacIntyre does famously argue that certain virtues will be needed in any practice; this suggests that these virtues will appear in the conception of *eudaimonia* of any practice. I shall cover this argument below, in §2.5.

### 2.3.3 Standards

I turn now to the set of “standards of excellence” for internal goods. The distinction between the standards for internal goods and the internal goods themselves is not as sharp as the division into subsections might suggest. For example, consider portraiture and the property of resemblance — the property that a portrait has when it realistically resembles its subject. We might say — or might have said, pre-Impressionists

<sup>29</sup> MacIntyre, *After Virtue*, 223.

<sup>30</sup> Compare Aristotle, *Nicomachean Ethics*, in *The Complete Works of Aristotle*, ed. Johnathan Barnes (Princeton: Princeton University Press, 1984), 1098a18.



and pre-Cubists — that resemblance is required for a portrait to be excellent. But there are two formally different reasons we might give for saying this. First, we might say that resemblance is required for something to be a portrait at all, whether excellent or not; a “portrait” that does not realistically resemble its subject, whatever it might be, simply is not a portrait. In this case, the internal good is defined in part by resemblance. Second, we might say that resemblance is required “merely” for a portrait to be an excellent portrait; a portrait that does not realistically resemble its subject is still a portrait, albeit one that is deficient in at least one respect. In this case, the internal good is not *defined* by resemblance; rather, resemblance is one of the *standards* for the internal good. I take it that, in many cases, there will be no good reason for classifying a property as a defining property of the internal good rather than a standard for especially excellent internal goods, or vice versa, and so often this will be a distinction without a difference. Still, this section will have a different focus from the previous, and I needed some place to discuss these issues.

I see no reason not to assume that a given practice will have standards for all four types of internal goods. For goods, the standards will be standards of excellence in the most familiar sense: the criteria by which the product is judged and evaluated. Hence, in portraiture, where the goods are the portraits themselves, the standards for the goods will be the criteria by which a portrait is judged and evaluated. Goods that realize or exemplify these standards will be called excellent goods or simply *excellent*.

The standards for virtues will be the criteria by which the actions of practitioners are judged and evaluated. Hence, it makes sense to call actions that realize or exemplify these standards excellent. These standards may or may not be codified as rules — what MacIntyre calls a “rule of practice.”<sup>31</sup> In either case, he claims that the standards are justified — have authority over the practitioners — because they are necessary to produce excellent goods: “it is they alone which confer authority on the rules defining the practice, the rules without which the goods internal to the practice cannot be achieved.”<sup>32</sup> In portraiture, for example, an unwillingness to flatter one’s sitters by including wrinkles, acne, moles, and other “blemishes” may (in particular times and places) be considered a virtue of an excellent painter, because an honesty of this sort is considered an excellence of a portrait. On this view, that standards of excellent action supervene on standards of excellent goods. Should the standards of excellent goods change, the standards of excellent action will change as well. For example, in one time and place, the standards of excellent cooking — when they include, for example, certain standards of presentation — may require that an excellent cook have the ability (virtue) to dice an onion rapidly, finely, and evenly. In other times and places — when presentation is less important than the speed with which the dish is prepared — this virtue will not be required, and may even be considered a vice.

All this, however, assumes a goods-oriented view of practices. In a virtue-oriented

<sup>31</sup> MacIntyre, “Objectivity in Morality and Objectivity in Science,” 27.

<sup>32</sup> *Ibid.*, 28.

practice, the relationship will generally be reversed. Consider the practice of ethics in the Aristotelean tradition. The primary internal goods of this practice are eudaimonia and virtue — and here I mean eudaimonia in its standard sense and *the* virtues — and so this practice is virtue-oriented — in the more attenuated sense of “virtue.” While texts of ethical philosophy are also produced by Aristotelean ethicists as internal goods, these texts are generally either textbooks (for instructing young people in the virtues) or practically-oriented philosophical theory (for improving humans’ understanding of the nature of the virtues).<sup>33</sup> These goods are thus excellent insofar as they have the ability to promote the virtues (in the general sense) of the practice. That is, the standards of excellence for the *goods* supervene on the standards of excellence for the *virtues*.

Since MacIntyre is so vague about the internal good of eudaimonia, it is hard to develop an account of its standards of excellence. If it is to realize the kind of unified life that I discussed above, it must at least involve the transfer of some of the internal goods of the practice into one’s other activities.

## 2.4 Progress

I turn now to my detailed discussion of the internal good of progress. I show how this internal good plays a critical role, both in MacIntyre’s conception of practice and in his broader philosophical project.

### 2.4.1 The good of progress

Above, we saw that MacIntyre introduced this internal good in terms of the solution of problems, “the attempts to sustain progress and to respond creatively to problems.”<sup>34</sup> However, progress need not only come from solving existing problems. A development, internal or external to the practice, might make it possible for the practice to produce

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<sup>33</sup> “Theory” may not seem like the right term here, because of certain Hempelian connotations that are inappropriate for MacIntyre’s view of ethics. Throughout his body of work, MacIntyre describes the aims of ethical inquiry using such terms as “conception,” “account,” “understanding,” “deductive scheme,” “demonstrative argument,” and so on. I use “theory” simply because, as a philosopher of science, I am used to calling all such things theories. In chapter 3, I introduce the term “representation” as a general term that does much of the work of “theory” without the undesirable Hempelian connotations. However, in the context of ethics, “representation” may have some even more undesirable meta-ethical connotations. In any case, I do not believe my point here depends on any details about what a theory is or isn’t; nor do I expect it to founder on differences between a theory and one of these other things. The reader should feel free to substitute some other term in place of “theory” throughout. I thank Peter Wicks for raising this concern.

<sup>34</sup> MacIntyre, *After Virtue*, 190.

some new good or adopt some new virtue. Consider the development of the single-lens reflex (SLR) camera. Prior to the development of SLR, the tableau that the photographer viewed through the rangefinder or viewfinder was not the same as the tableau to which the film would be exposed. The development of SLR allowed the photographer to compose a photograph “in the viewfinder,” rather than taking a wider shot and cropping the image down in the darkroom. This in turn made it significantly easier for photographers to shoot on 35 mm film — previously, the need to crop a shot down required the use of large format film, at least 4 inches by 5 inches. Next, this meant that cameras for professional, artistic use could be smaller, lighter, cheaper, more portable, and greatly speeded up the process of taking each individual shot. And finally, this made candid photography, wildlife photography, and sports photography viable goods for photographic practice. While this did not solve any sort of outstanding problem, it was undoubtedly progress.

More generally, then, I suggest the following: *the standards for the internal good of progress are the standards for the revision or extension of the internal goods of the practice.* By “revision” I mean that the internal goods can be improved, for example, by solving a problem of incoherence in their definition. By “extension” I mean that the internal goods can be improved, for example, by including new cases or examples. Note that progress can be made with respect to any of the four types of internal goods, including progress itself. One extremely important way a practice can make progress is to improve its understanding of what counts as progress.

Whether revision or extension, there are two basic ways in which progress can be stimulated. Consider, first, the development of impressionism in late nineteenth century France. Impressionism was a reaction to the highly realistic style that prevailed in France at the time; to the extent that it constituted a revision in the standards of excellent painting, this case of progress was *purely internal* to the practice of painting.<sup>35</sup> But in the case of the development of SLR cameras, the practice of photography saw progress thanks to some external development. We shall call cases like this *partly external* progress.

### 2.4.2 Progress and conservation

Like excellent action, the standards for progress often refer to excellent goods. But whereas the standards for virtue assume that the standards for goods are fixed (and vice versa for virtue-oriented practices), the standards for progress describe the (good, excellent, and so on.) ways in which the standards for goods *change*, and hence must assume that these standards are not fixed. MacIntyre points this out explicitly: “Practices never have a goal or goals fixed for all time . . . but the goals themselves are

<sup>35</sup> Assuming, of course, that Impressionism constituted progress. I thank Paul Weithman for reminding me the early controversies surrounding Impressionism. Clearly, at the center of these controversies was a disagreement over what constitutes progress in painting.

transmuted by the history of the activity. It therefore turns out not to be accidental that every practice has its own history and a history which is more and other than that of the improvement of the relevant technical skills.”<sup>36</sup> Hence this history is not the accretion of a Burkean tradition; rather, it reflects the gradual though perhaps still radical revision and extension of the practice.

Virtue has a conservative effect on the practice: younger or less experienced practitioners attempt to realize virtue by replicating the actions of older or more experienced practitioners. Excellent action is achieved by doing things in the same way as one’s predecessors. The virtues of a practice demand a “subordination of individual experience and thought . . . that supplies the crucial element of impersonality and objectivity to practices.”<sup>37</sup> Progress, on the other hand (and as the name suggests), has a progressive effect, driving the practice to change and move into the future. Progress is made by reforming or even rejecting the ways of one’s predecessors and finding new methods that work better. Hence, *it is strictly incorrect to say that the aim of the practice is the production of its internal goods as they are understood at any given moment*. The internal good of progress is an aim to improve, and hence change, the practice’s understanding of its goods, beyond that understanding at any given time. As MacIntyre puts it, “The sequences of development find their point and purpose in a progress *towards and beyond* a variety of types and modes of excellence.”<sup>38</sup>

It is crucial for MacIntyre’s central meta-ethical project — the critique of the Enlightenment project to provide universal rational foundations for morality — that practices include the internal good of progress. For, if some practices (say, that of Enlightenment moral foundationalism) do *not* include this among their internal goods, then he cannot make the general claims that, first, a practice that is not making progress is failing to produce its internal goods, and second, a practice that is failing to produce its internal goods ought to be abandoned. And, if he cannot make these general claims, I do not see how he can argue in any but an *ad hoc* way from the fact (granting that it is a fact) that Enlightenment moral foundationalism is failing to make progress to the conclusion that Enlightenment moral foundationalism ought to be abandoned.

It may be objected that MacIntyre’s criticism of Enlightenment moral foundationalism is based simply on its failure to produce (according to its own standards of success) the internal good of a true and complete moral theory — its failure with respect to its goods rather than a failure with respect to progress. This is certainly the central claim of the argument of chapter five of *After Virtue*, “Why the Enlightenment

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<sup>36</sup> MacIntyre, *After Virtue*, 193-4.

<sup>37</sup> MacIntyre, “Objectivity in Morality and Objectivity in Science,” 29.

<sup>38</sup> MacIntyre, *After Virtue*, 189, my emphasis. MacIntyre is explicit that this is his basic disagreement with communitarianism; see Alasdair MacIntyre, “Politics, Philosophy and the Common Good,” in *The MacIntyre Reader*, ed. Kelvin Knight (Notre Dame: University of Notre Dame Press, 1998), 235–52

Project of Justifying Morality had to Fail.”<sup>39</sup>

But MacIntyre’s own project of normative ethics — a synthesis of Aristotle, Aquinas, and Marx — would be vulnerable to this same argument.<sup>40</sup> As the discussion of the need for a conception of ethics as a “narrative quest” in chapter fifteen of *After Virtue* makes clear, the good of the practice of ethical inquiry, as MacIntyre sees it, is a true and complete moral theory according to the standards for truth and completeness of the Aristotelean-Thomistic tradition:

It is in looking for a conception of *the* good which will enable us to order other goods, for a conception of *the* good which will enable us to extend our understanding of the purpose and content of the virtues, for a conception of *the* good which will enable us to understand the place of integrity and constancy in life, that we initially define the kind of life which is a quest for the good.<sup>41</sup>

This good of a true and complete theory — again, as these standards are understood within the Aristotelean-Thomistic tradition — is also found in MacIntyre’s development of a general, Aristotelean-Thomistic account of inquiry as a practice: “So the *Posterior Analytics* in its account of scientific demonstrative explanations as the *telos/finis* of enquiry furnishes us with an account of what it is to understand, that is, of the distinctive human good to be achieved by enquiry as a distinctive type of activity.”<sup>42</sup> While the particular standards for truth and completeness here are very different from those of Enlightenment projects in epistemology and ethics, the good of inquiry is, again, true and complete theory.

Furthermore, it is manifest that this good not only *has not* been achieved, even by the Aristotelean-Thomistic tradition, but *must* be conceived strictly as a regulative ideal, achievable only in the limit of infinitely extended human ethical inquiry.<sup>43</sup> Indeed, MacIntyre argues as much near the end of *After Virtue*:

<sup>39</sup> MacIntyre, *After Virtue*, 50-61.

<sup>40</sup> On the influence of Marx see, especially, MacIntyre, “Three Perspectives of Marxism”; Alasdair MacIntyre, “The *Theses on Feuerbach*: A Road not Taken,” in *The MacIntyre Reader*, ed. Kelvin Knight (Notre Dame: University of Notre Dame Press, 1998), 223–34.

<sup>41</sup> MacIntyre, *After Virtue*, 219, his emphasis.

<sup>42</sup> MacIntyre, “First Principles, Final Ends, and Contemporary Philosophical Issues,” 156. Note that, in the preface to MacIntyre, *Selected Essays: The Task of Philosophy*, he describes this lecture as an attempt to articulate his own Thomistic Aristotelean commitments.

<sup>43</sup> It may be objected that Enlightenment moral foundationalism may not actually name a practice. But, like Aristotelean-Thomistic ethical inquiry, it’s a collaborative activity that aims at producing certain kinds of goods, indeed a good that (if it could be produced at all) could only be produced by engaging in that sort of activity. That is, it appears to have internal goods, albeit arguably specious ones. Perhaps the objector would think that if a good cannot be achieved then no practice can pursue it, on some

For our situation in respect to theories about what makes one theory rationally superior to another is no different from our situation in regard to scientific theories or to moralities-and-moral-philosophies. In the former as in the latter case what we have to aspire to is not a perfect theory, one necessarily to be assented to by any rational being, because invulnerable or almost invulnerable to objections, but rather the best theory to emerge so far in the history of this class of theories.<sup>44</sup>

Hence the vindication, by MacIntyre's lights, of the Aristotelean-Thomistic tradition against Enlightenment moral foundationalism cannot come from the success of the former and failure of the latter in providing a true and complete ethical theory. Rather, it comes from the achievement of *progress* by the former and the *lack of progress* of the latter.

This can also be seen in *Whose Justice? Which Rationality?*, where the argument is based explicitly on progress (and, notably, progress *away from error*, not *towards truth* as such) rather than the immediate production of the true and complete moral theory. Chapter eighteen, "The Rationality of Traditions," provides the account of rationality and warranted assertability from which the historical-meta-ethical balance of the book depends:

The identification of incoherence within established belief will always provide a reason for enquiring further, but not in itself a conclusive reason for rejecting established belief, until something more adequate because less incoherent has been discovered. At every stage beliefs and judgments will be justified by reference to the beliefs and judgments of the previous stage, and insofar as a tradition has constituted itself as a successful form of enquiry, the claims to truth made within that tradition will always be in some specifiable way less vulnerable to dialectical questioning and objection than were their predecessors.<sup>45</sup>

MacIntyre's argument is not that Thomistic Aristoteleanism provides a superior foundation for ethics than Enlightenment foundationalist proposals, whether this foundation is understood in Enlightenment terms or in terms of Aristotelean *arche*. Rather, he is rejecting foundationalist aims and methods themselves, in favor of dialectical aims and methods.

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sort of "ought implies can"-type grounds. But then one could argue — because its internal good is only a regulative ideal, not something that could ever actually be achieved — that Aristotelean-Thomistic ethics would not qualify as a practice either. I thank Peter Wicks for raising this objection.

<sup>44</sup> MacIntyre, *After Virtue*, 270.

<sup>45</sup> Alasdair MacIntyre, *Whose Justice? Which Rationality?* (Notre Dame: University of Notre Dame Press, 1988), 359.



Similarly, in *Three Rival Versions*,<sup>46</sup> his Gifford lectures, MacIntyre’s primary argument for Thomism is based on the prospect that approach to ethical inquiry has and continues to offer for progress. Over the course of the lectures, MacIntyre presents two pairs of incommensurable rival intellectual traditions. In order of presentation, the first is the contemporary dispute between Enlightenment foundationalism and its successors on the one hand — exemplified by the Ninth Edition of the *Encyclopaedia Britannica* — and the anti-Enlightenment or postmodern tradition founded by Nietzsche — especially in *On the Genealogy of Morals* — on the other. The second is the High Medieval dispute between Augustinian Christianity and Latin Averroist-Aristoteleanism. For all the emphasis that MacIntyre places on the traditionalism and deference to authority of Thomism and (especially) Augustinianism in the early lectures,<sup>47</sup> it is clear in the middle lectures that Aquinas’s great contribution to the Medieval dispute was to synthesize (the term is used deliberately) the two traditions, thereby replacing interminable disagreement with a progressive intellectual project.<sup>48</sup> Doing this, however, required Aquinas to *reject* certain outdated elements of both the Averroist-Aristotelean and the Augustinian traditions;<sup>49</sup> whence the Condemnation of 1277.

Returning to the contemporary dispute in the final lectures,<sup>50</sup> MacIntyre argues that Thomism is able — better than both the Enlightenment and the anti-Enlightenment — to recognize the incommensurability of the two positions and to diagnose the underlying problem in terms of the failure of both traditions to make progress in their basic projects. By contrast, the Thomist tradition, assimilating what is worthwhile in both of its “rivals,” offers new opportunities for progress. In short and once again, Thomism is to be preferred because it, unlike both modernism and postmodernism, is progressive.<sup>51</sup>

<sup>46</sup> Alasdair MacIntyre, *Three Rival Versions of Moral Enquiry: Encyclopaedia, Genealogy, and Tradition* (Notre Dame: University of Notre Dame Press, 1991), ISBN: 9780268018771.

<sup>47</sup> *Ibid.*, especially lecture IV.

<sup>48</sup> *Ibid.*, lectures V-VII.

<sup>49</sup> *Ibid.*, 131ff.

<sup>50</sup> *Ibid.*, lectures VIII-X.

<sup>51</sup> The connotations of that final term are deliberate; cf. Alasdair MacIntyre, “Natural Law as Subversive: The Case of Aquinas,” in *Selected Essays: Ethics and Politics* (1995; Cambridge: Cambridge University Press, 2006), 41–63. Because of the central role for progress in MacIntyre’s conception of practices and traditions — not to mention the scathing rebuke of Burke as “an agent of positive harm” (MacIntyre, *Whose Justice? Which Rationality?* 353) — I am deeply puzzled by both his reputation as a conservative or traditionalist political philosopher and the fondness many conservatives and traditionalists (though certainly not all) seem to have for his work. For example, writing from the egalitarian liberal side, Dwight Furrow associates MacIntyre with Burke, John Kekes, William Buckley, William Bennett and Gertrude Himmelfarb and, while allowing that MacIntyre “attempt[s] to explain how change occurs organically and per-

### 2.4.3 Telic vs. melioric progress

MacIntyre claims that the good of progress requires a certain sort of realism about excellent goods and their standards: there are real, actual, human-independent standards for excellent goods, and the practice makes progress over time by better approximating those independent standards. Call such a conception of progress a *telic* conception, for its commitment to a final, perfected end-state — a *telos* — of the practice. Note that a telic conception need not involve the claim that this *telos* can actually be achieved within the stretch of (mortal) human history. Even if it can only be approached asymptotically, the conception of progress is still telic, in that progress is measured by reference to the *telos*. MacIntyre’s conception of progress has this telic-yet-asymptotic character. In many classic examples of telic conceptions of progress — Popper, Hegel, Aquinas — the *telos* is defined in terms of truth. However, a telic conception need not involve truth. In van Fraassen’s constructive empiricism,<sup>52</sup> for example, the *telos* of scientific inquiry is defined in terms of maximal empirical adequacy, which is strictly weaker than truth. Still further from truth, in *Dynamics of Reason* Michael Friedman presents a telic conception of scientific progress in which the *telos* is defined in terms of a more-encompassing Habermasian communicative rationality.<sup>53</sup>

Several historians and philosophers of science of the twentieth century, by contrast, have used a radically different conception of progress, which I will call *melioric*. On a melioric conception, progress is not measured by reference to an end-state using absolute standards, but instead by reference to the preceding state (or some preceding states) using relative standards. Thomas Kuhn’s rejection of a telic conception

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mit[s] a good deal of pluralism,” seems to think that he deploys “traditional values as absolute, fixed guidelines, especially on domestic policy and the family” and believes that tradition has “absolute authority.” Dwight Furrow, *Reviving the Left: The Need to Restore Liberal Values in America* (Amherst, NY: Prometheus Books, 2009), 235 For an example of a conservative fan of MacIntyre, see Rod Dreher, “Becoming Barbarians,” *The American Conservative* (May 18, 2009). The widespread rejection of MacIntyre’s work by progressive and leftist philosophers is the flip side of this puzzling coin. Susan Moller Okin’s feminist critique of MacIntyre Susan Moller Okin, *Justice, Gender, and the Family* (New York: Basic Books, 1989), ch. 3, for example, only makes sense on the assumption that MacIntyre cannot consistently reject the sexism of Aristotle and Aquinas; but this assumption, in turn, only makes sense on the further assumption that philosophical practice in the Thomistic Aristotelean tradition does not or cannot progress. For an examination of the relationship between MacIntyre’s views and the Marxist tradition, see Paul Blackledge, “Alasdair MacIntyre: Social Practices, Marxism and Ethical Anti-capitalism,” *Political Studies* 57, no. 4 (December 2009): 866–84.

<sup>52</sup> Bas Van Fraassen, *The Scientific Image* (Oxford and New York: Oxford University Press, 1980).

<sup>53</sup> Michael Friedman, *Dynamics of Reason* (Stanford: Center for the Study of Language / Information, 2001).



of progress for the natural sciences is famous: “We may . . . have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth.”<sup>54</sup> However, just as importantly, he retains a melioric conception: from the perspective of the paradigm that survives an (attempted) revolution, progress has been made in the form of resolving the anomalies of the paradigm that was dominant prior to the (attempted) revolution. This has sometimes (that is to say, frequently) been thought to imply that the dramatic changes in scientific inquiry that Kuhn called revolutions are radically arational, a matter of survival by political power rather than survival by good reasons.<sup>55</sup>

Larry Laudan’s “reticulated model” of scientific progress is meant to refine Kuhn’s account and avoid the irrationalist implication.<sup>56</sup> On Laudan’s model, scientific inquiry is analysed in terms of theories, methods, and aims, and a change to any one is rationally justified by appeal to the other two. An established theory may be rejected in favor of another that better fits the accepted methods and aims, or some aim may be rejected when it is found to be incompatible with established theories and methods. The process is thoroughly piecemeal, rational on the scale of a single proposed change, and does not approach some *telos*, even asymptotically. “There is no single ‘right’ goal for inquiry.”<sup>57</sup> This leads Laudan to a relativism that is not, or at least attempts not to be, an anything-goes arationalism:

[A] particular bit of science may be progressive (with respect to one set of values) and regressive (with respect to another). There is simply no escape from the fact that determinations of progress must be relativized to a certain set of ends, and that there is no uniquely appropriate set of those ends.<sup>58</sup>

John Dewey’s conception of inquiry also involves a melioric conception of progress. In his *Logic*, “inquiry” is defined as follows:

Inquiry is the controlled or directed transformation of an indeterminate situation into one that is so determinate in its constituent distinctions and relations as to convert the elements of the original situation into a unified whole.<sup>59</sup>

<sup>54</sup> Kuhn, *The Structure of Scientific Revolutions*, 170.

<sup>55</sup> For a typical example, see Ernan McMullin, “The Shaping of Scientific Rationality: Construction and Constraint,” in *Construction and Constraint: The Shaping of Scientific Rationality*, ed. Ernan McMullin (Notre Dame: University of Notre Dame Press, 1988), 2-7.

<sup>56</sup> Larry Laudan, *Science and Values: The Aims of Science and Their Role in Scientific Debate* (Berkeley and Los Angeles: University of California Press, 1986), chs. 3 and 4.

<sup>57</sup> *Ibid.*, 63.

<sup>58</sup> *Ibid.*, 66, his parentheses, my brackets.

<sup>59</sup> John Dewey, *Logic, the Theory of Inquiry*, in *John Dewey: The Later Works, 1925-1953*, ed. Jo Ann Boydston, vol. 12: 1938 (1938; Carbondale and Edwardsville: Southern Illinois University Press, 1986), 108.

For my purposes here, further exegesis of this conception of inquiry is neither desirable nor necessary. It suffices to note that inquiry involves the “transformation” of one prior state (the “indeterminate situation”) into a later state (the “determinate” situation). Hence an inquiry is evaluated (as successful or not) by comparing the present state to a past state, rather than by comparing the present state to some ideal future state. Further, the situation is determinate to the extent that there is a definite course of action for the organism to take so as to realize its aims, and indeterminate to the extent that there is not such a definite course of action. Hence the standards of success are relative to the organism and its activities, and are not absolute or universal.<sup>60</sup>

Imre Lakatos’s “methodology of scientific research programmes” can be understood as an attempt to synthesize the telic conception of progress from Popper’s falsificationism with Kuhnian melioric progress.<sup>61</sup> On Lakatos’s view, the problem of “theory choice” is a misnomer; what we standardly evaluate and choose among are the diachronic projects of rival research programs, *series* of theories, not the theories in which these research programs are articulated synchronically. As a research program develops, it will gradually identify various internal and external problems; it is “progressive” or enjoying “growth” if and insofar as it is able to solve and remove these problems, and “degenerating” otherwise.<sup>62</sup> A progressive research program should be preferred to a degenerating one; though there is always the possibility that some brilliant innovation will bring a degenerating research program back to robust, progressive flourishing.<sup>63</sup>

On this gloss, Lakatos’s conception of progress is melioric, and developed from Kuhn in an obvious way: progress consists in successfully dealing with the problems of predecessors.<sup>64</sup> However, Lakatos also wants to incorporate Popperian telic progress towards truth, or at least empirical adequacy: his official definitions of progress and degeneration refer to “excess empirical content,” that is, “predict[ing] some novel, hitherto unexpected fact.”<sup>65</sup> And he lurches awkwardly between the two conceptions. Consider his assessment of Einstein and Newton:

Einstein’s theory is better than — that is, represents progress compared with — Newton’s theory *anno 1916* . . . *because* it explained everything that Newton’s theory had successfully explained, and it explained also *to some extent* some known anomalies and, in addition, forbade events

<sup>60</sup> See my further discussion of Dewey in §5.6.

<sup>61</sup> Imre Lakatos, *Falsification and the Methodology of Scientific Research Programs*, in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge, UK and New York: Cambridge University Press, 1970), ISBN: 0521096235.

<sup>62</sup> *Ibid.*, 118, but see my next paragraph.

<sup>63</sup> *Ibid.*, 173 and 179.

<sup>64</sup> Compare Thomas Kuhn, “Objectivity, Value Judgment, and Theory Choice,” in *The Essential Tension* (Chicago: University of Chicago Press, 1977), 320–339.

<sup>65</sup> Lakatos, *Falsification and the Methodology of Scientific Research Programs*, 118.

like transmission of light along straight lines near large masses about which Newton's theory had said nothing . . . ; moreover, *at least some* of the unexpected excess Einsteinian [empirical] content was in fact *corroborated* (for instance, by the eclipse experiments [by Eddington in 1919]).<sup>66</sup>

For most of this paragraph, Einstein's advantage over Newton is the former's melioric progress: Einstein solves the problems that had developed in the Newtonian research program. None of this shows up in Lakatos's official definition; the only thing that counts, strictly speaking, is the prediction of novel observations. And, of course, these predictions had not yet been corroborated in 1916. For all that Lakatos goes on about how "empiricalness . . . and theoretical progress are inseparably connected,"<sup>67</sup> it seems clear that he has not achieved the desired grand Popper-Kuhnian synthesis;<sup>68</sup> he is, in fact, simply equivocating.

In chapter 1, I identified and characterized two different methods in contemporary political philosophy, called "ideal theory" and "non-ideal theory."<sup>69</sup> Ideal theory is often, though not always, characterized by a telic conception of progress towards justice: progress is achieved insofar as our society better approximates the perfectly just or well-ordered society. For non-ideal theorists, by contrast, progress is characteristically melioric: we make progress insofar as we remove or remedy the injustices of the past and present. These conceptions of progress also carry over into political philosophy itself. For many ideal theorists, progress in philosophy comes from a more true and complete account of a perfectly just society. By contrast, for many non-ideal theorists, progress in philosophy comes from an adequate (for practical purposes) understanding of the nature and causes of the most pressing present injustices; since the most pressing present injustices will be expected to change radically over time (we no longer need to worry about the injustices of the hereditary aristocracy and chattel slavery as such), non-ideal theory need not develop continuously towards any true and complete account.

As mentioned above, MacIntyre claims that, either for all practices or for such practices as scientific and philosophical inquiry, a telic conception of progress is required. However, I have not found any real argument in his work in support of this claim. The closest is the following:

Wherein does the unity of the history [of the kinetic theory of gases] lie? In the continuous attempt to construct a realistic representation of the inner structure of gases which will enable us to diagram the relationship of the microproperties of gases to the nature of the molecules of which gases are composed.<sup>70</sup>

<sup>66</sup> *Ibid.*, 124, his emphasis and parentheses, my brackets.

<sup>67</sup> *Ibid.*, 123.

<sup>68</sup> *Ibid.*, 179.

<sup>69</sup> See §1.2.5.

<sup>70</sup> MacIntyre, "Epistemological Crises, Dramatic Narrative and the Philosophy of Sci-

As I read it — and I readily admit that this reading involves significant interpolations — MacIntyre is making a connection in this passage between truth (the “realistic representation”) and explanation (“diagram[ing] the relationship”). If physics is indeed a practice, and explanations are among its goods, and explanations require some kind of asymptotic approach to truth, then it would seem that the practice of physics requires some kind of asymptotic approach to truth. To my ear, this is reminiscent of the no-miracles argument for scientific realism: truth is necessary for successful explanation.

However, as Nancy Cartwright has argued, truth is not required for explanation. Indeed, as I read her argument, explanation generally requires oversimplification and other ways of deliberately ignoring complicating factors that make the key claims of explanations strictly speaking false. Thus, on her view, not only is truth not necessary for successful explanation; taken too far, truth actually conflicts with explanation:

Most scientific explanations use *ceteris paribus* laws. These laws, read literally as descriptive statements, are false, not only false but deemed false even in the context of use. This is no surprise: we want laws that unify; but what happens may well be varied and diverse. We are lucky that we can organize phenomena [that is, explain them using *ceteris paribus* generalizations] at all. There is no reason to think that the principles [that is, the *ceteris paribus* generalizations] that best organize [that is, explain] will be true, nor that the principles that are true will organize much.<sup>71</sup>

All together, then, I see no reason to require a practice to have a telic conception of progress. The internal good of progress may be either telic or melioric, and indeed the conception of progress may shift from telic to melioric and vice versa as the practice develops.<sup>72</sup>

## 2.5 Practices and institutions

Next we have the sister to MacIntyre’s conception of practice: the conception of an *institution*. MacIntyre introduces institutions in *After Virtue* as follows:

Practices must not be confused with institutions . . . . Institutions are characteristically and necessarily concerned with what I have called external goods. They are involved in acquiring money and other material

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ence,” 33; cf. MacIntyre, “[First Principles, Final Ends, and Contemporary Philosophical Issues](#)”

<sup>71</sup> Nancy Cartwright, *How the Laws of Physics Lie* (Oxford and New York: Oxford University Press, 1983), 52-3, my brackets, ISBN: 0198247044.

<sup>72</sup> This is perhaps one of my deepest and most important points of disagreement with MacIntyre.

goods; they are structured in terms of power and status, and they distribute money, power and status as rewards. Nor could they do otherwise if they are to sustain not only themselves, but also the practices of which they are the bearers. For no practices can survive for any length of time unsustained by institutions. Indeed so intimate is the relationship of practices to institutions — and consequently of the goods external to the goods internal to the practices in question — that institutions and practices characteristically form a single causal order in which the ideals and the creativity of the practice are always vulnerable to the acquisitiveness of the institution, in which the cooperative care for common goods of the practice is always vulnerable to the competitiveness of the institution. In this context the essential function of the virtues is clear. Without them, without justice, courage and truthfulness, practices could not resist the corrupting power of institutions.<sup>73</sup>

This passage has two distinct parts. MacIntyre first gives a characterization of the relationship between practices and institutions in terms of the distinction between internal and external goods. He then gives a highly abbreviated version of his argument for the importance of the cardinal virtues in institutions. I shall cover each of these in turn.

It is incorrect to think of an institution as an instance (token) of a practice (type). If this were the case, we could not distinguish either individuals or positions that are members of an institution from those that are practitioners of the practice; on this account of institutions, to be a practitioner of a given practice would be to be a member of at least one of its institutional instances. Instead, we should think of an institution as *a mechanism for the provision of external goods among the practitioners*, where both institution and practice are normally taken to be particular (tokens), and perhaps instances of ideals (types).

Practices, in order to produce their internal goods, require and utilize external goods in at least three ways. First, in capitalist societies, practices characteristically require wealth in order to acquire the materials that they will use to produce their internal goods and in order to feed and maintain the physical bodies of their practitioners.<sup>74</sup> Second, the community of practitioners characteristically requires such external goods as status and power to organize itself: *A* is nothing but an amateur, *B* is young but talented with challenging new ideas, *C* is the long-accomplished and conservative master of the art, and consequently disagreements among *A*, *B*, *C* over, say, what constitutes progress in the practice will be unavoidable and must be settled.

<sup>73</sup> MacIntyre, *After Virtue*, 194.

<sup>74</sup> It is tempting to put this point in terms of the means of production: wealth is required in order to acquire the means used to produce the internal goods. But this is to say that wealth is utilized to acquire the means of production, whereas, in classical political economy, wealth is *itself* one of the means of the production.

For the sake of clarity of presentation, I will be working here with an oversimplified model of institutions and practices, on which each practice is paired with exactly one institution, and vice versa. In actuality, a single practice (portraiture) can be “spread over” many institutions (several different academies and workshops), and one institution (a given school for the visual arts) can support several practices (portraiture, landscape painting, abstract painting, sculpture, pottery, photography, and so on).

Again, the development, maintenance, and pursuit of a practice requires external goods. Since external goods are only available to the practice through the mechanisms of institutions, the construction, maintenance, and pursuit of practice requires the construction, maintenance, and successful function of institutions. Without the institution the practice withers and dies, for exactly the same reason the blossoms of an apple tree wither and die should the roots be removed. However, the institution, in itself, does not share the aims of the practice. The aim (both ultimate and proximate) of the practice is to produce excellent internal goods; while one of the ultimate aims of the institution is to enable the successful pursuit of the practice, the institution also has an aim (both ultimate and proximate) of acquiring and provisioning external goods.

Instances of *institutional conflict* are thereby possible. In institutional conflict, the aims of the practice (viz., internal goods) conflict with the aims of the institution (in particular, the acquisition of external goods). Suppose painters began to develop standards for excellent portraits that the wealthy patrons of the practice found ugly and unflattering — say, a certain honesty and insistence on including wrinkles, moles, acne, and other “blemishes” in the portrait. Patrons would begin to withdraw their support from the practice; that is, the institution supporting it would have difficulty acquiring the external goods that both require. Here there is a tension between the internal goods of the practice — the portrait, and the emphasis on an honest representation — and the external goods of its institution — the funding provided by the wealthy patrons.

Any particular instance of institutional conflict will be resolved in one of two ways. First, the internal goods of the practice might prevail. In this case, the loss of external goods might (or might not) be seen as a regrettable sacrifice, but necessary to preserve the practice. Call this a case of *practical priority*. Second, the external goods might prevail. The need to secure the external goods required by the practice, at least for the moment, is more important than the internal goods. Call this a case of *institutional priority*.

So far I have been considering particular instances of institutional conflict. However, any given combination of a practice and an institution will see numerous such conflicts. Since the combination will have (one expects) established and regularized procedures for settling institutional conflicts, trends are likely to emerge. If the trend is in the direction of institutional priority — in other words, if institutional priority is recurrent and systematic, and especially if it poses a serious threat to the pursuit of internal goods — we will say that the practice is suffering *institutional domination*

I believe that institutional domination is the basis for MacIntyre’s first argument — what I will call the *practice argument* — for several of the classical virtues, in *After Virtue*: these virtues either are or are required to avoid institutional domination.<sup>75</sup> This is best illustrated, I think, by reconstructing the particular argument for the virtue of justice. First, justice is connected to ideas of merit and desert.<sup>76</sup> By this MacIntyre seems to mean the standards for the internal goods of the practice. That is, justice is the virtue of (correctly) evaluating a practitioner according to the practice’s standards for excellent action and (correctly) evaluating a good according to the practice’s standards for excellent goods. Injustice, in this sense, would include such things as evaluating a practitioner based on her power and fame, or evaluating a good based on the amount of wealth for which it can be exchanged. Hence, injustice is evaluating something in terms of or as an external good, rather than as an internal good.

Suppose some given practice is involved in institutional conflict over the standards of the goods to be produced. Practical priority leads to the production of goods according to the standards for excellent goods of the practice. That is, practical priority evaluates goods in terms of the standards of the practice, and hence exercises the virtue of justice. Institutional priority leads to the production of goods according to their expected market value, and hence evaluates goods in terms of external goods, and thus exercises the vice of injustice.<sup>77</sup> If the virtue of injustice becomes fixed in the procedures for resolving institutional conflict, then the practice suffers institutional domination and the combination of practice and institution is unjust.

Institutional domination is a threat to the practice. If the internal goods are nothing more than a means to accumulate wealth and power then the practice will come to produce only those internal goods that can be exchanged for wealth and power and no or very few internal goods that meet the standards of excellence for the practice; to borrow from MacIntyre in a slightly different context, institutional domination “renders the practice pointless except as a device for achieving external goods.”<sup>78</sup> This is not to say that institutional priority must always be avoided; sometimes, for the continued survival of the art form, concessions have to be made to the rich and powerful. Selling out isn’t ideal, but it’s preferable to abandoning the practice entirely. What is to be avoided is sustained commercialization.

<sup>75</sup> MacIntyre, *After Virtue*, 191-5.

<sup>76</sup> *Ibid.*, 192.

<sup>77</sup> Note that I do not require that virtues or vices, in the sense at hand, be exercised by an individual agent. If a single person is responsible for settling cases of institutional conflict, then it is that single person who will exercise justice or injustice. However, if several people — perhaps in some deliberative procedure — are responsible for settling the case, then they will, together, exercise justice or injustice. It may even be more accurate to say that the procedure is just or unjust, and not characterize any of the individuals involved as either just or unjust.

<sup>78</sup> MacIntyre, *After Virtue*, 191.



## 2.6 Relations among practices

MacIntyre introduces no terminology for talking about the relations among different practices. Since my aim, starting in chapter 4, is to consider the relationship between scientific inquiry and such practices as feminism, I introduce this terminology here using a series of examples. This will also provide an opportunity to reconsider the recognition claim.

Consider first the practice of football.<sup>79</sup> This practice can be divided into two parts or *subpractices*: the practice of the coach and the practice of the players. Both share the internal goods of the *superpractice* of football itself or in general — the virtues of a well-played game — but also each will have, in addition, its own specific internal goods — such as the virtue of “tough love” exemplified by the coach. Furthermore, these two practices are *complementary*, in the sense that each requires the internal goods of the other to achieve (a) its own internal goods and (b) the internal goods of football itself. A coach without players — or with vicious players — is useless; players without a coach — or with a vicious coach — are disorganized and directionless.

In other cases, several subpractices are complementary only in the sense (b) that each requires the internal goods of the others to realize the internal goods of the superpractice. Consider the practices of architecture, carpentry, masonry, wiring (the practice of an electrician), and plumbing. None of these practices requires any of the others — at least, not much — to realize its own internal goods. But all are required to realize the internal goods of home construction. And, in still other cases, several practices are complementary in the sense (a) that each requires the internal goods of the others to realize its own internal goods, but without any superpractice to provide an overarching goal. For example, the practice of agriculture provides grain, which is milled into flour, which is baked into bread, which in turn feeds the farmer.

Practice *A* may utilize — or even require — the internal goods of practice *B* without making any similar contribution to *B* as such; but *B* still has its own internal goods, distinct from those of *A*. For example, medicine utilizes the pharmaceuticals produced by the practice of pharmacology-pharmacy, but does not make any similar contribution to pharmacology-pharmacy in turn. (The physician may treat the pharmacist, but not *as* a pharmacist.) In these cases, we will say that *B* is *subordinate* to *A*, and correspondingly *A* is *superordinate* to *B*.

We can also consider practices in terms of genera and species. The generic practice of art can be specified, as visual art, plastic art, culinary art, dance, and music; and these can be further specified in turn, such as visual art into painting, drawing, photography, and so on.

It seems quite plausible to think that one practice may be a part of several distinct practices. For example, architecture is a part of both home construction and industrial or workplace construction. Similarly, it seems, teaching is its own practice, but also part of every academic discipline. Yet MacIntyre argues from this observation about

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<sup>79</sup> I thank Don Howard for suggesting this example.



teaching to the conclusion that teaching is not a practice.<sup>80</sup> On his view, teaching is not one practice, but instead several practices; or, better still, each practice has, as one of its parts, the activity of teaching the practice to novices/students/apprentices. The internal goods of teaching the practice are simply the internal goods of the practice itself; teaching, as such, does not have any internal goods.

Here I disagree with MacIntyre. Consider mathematics. The internal goods of mathematics are, let us say, mathematical knowledge and understanding, and especially the development (whether construction or discovery) of new mathematical knowledge and understanding. But the internal goods of teaching mathematics include the communication of established mathematical knowledge to students and the cultivation of mathematical understanding in these same students; in general, this involves the development of no new mathematical knowledge and understanding, except in an analogical sense. Furthermore, it is a commonplace (at least among mathematics teachers) that good mathematicians are often the worst teachers: good mathematicians generally have not had to struggle to achieve understanding abstract mathematical concepts; thus they generally have not reflected on how they came to understand these concepts; and thus they generally are unable to articulate their understanding, walk students through a similar process of coming to understanding, and are characteristically impatient with students who do not understand as easily and immediately as they did. Good mathematical teaching involves both articulating mathematics for all (or most) of one's students and patience with students who have difficulty understanding. And these last two virtues of good teachers are specifications of virtues of good teachers in general and as such, not just as mathematics teachers. Thus teaching seems to me to be its own practice, with its own distinct internal goods, though it is also various parts or specifications of many other practices. (There is even, of course, teaching the practice of teaching to student-teachers.)

Public discourse over the value — both relative and absolute — of practices and their internal goods is a major feature of MacIntyre's conception of politics. This will typically involve what I will call the *justification of a practice*: a set of reasons, acceptable to a given audience, for the claims that, first, at least some of the internal goods of the practice are valuable, and second, the practice should be supported and developed (whether indirectly, by providing it with the external and internal goods that it requires to produce its internal goods, or directly, by participation). Note that a set of reasons may be a justification for one audience, but not for another.

I take it that the justification of one practice, *A*, to an audience of practitioners of another, *B*, is straightforward when *A* and *B* are complementary: the internal goods of *A* are valuable because they make an essential contribution to either some shared project (the superpractice) or to the internal goods of *B*. The same sort of justification will work when *A* is subordinate to *B*. And when *A* and *B* share a genus — that is, they are similar — then *A* can be justified to *B* in terms of these similarities.

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<sup>80</sup> Quoted in Beadle, "Why Business Cannot Be a Practice," 236.

However, the recognition claim seems to block justification in all other cases. Recall that this was the claim that participation in a practice is required to justify the claim that the internal goods of that practice are valuable and worth pursuing. More briefly, participation is required for justification. Or again, more formally,

**recognition:** An individual  $a$  can give or accept a justification of a practice  $A$  only if  $a$  is a participant of  $A$ .

When practices are not related in any of the ways given above, the practitioners of  $B$  do not participate in  $A$ ; thus no justification of  $A$  can be given to the practitioners of  $B$ . Mark Murphy makes a similar point:

But rational communal deliberation about the place of the goods of the different practices within the life of the community is bound to be a chimera. For such deliberation would have to be founded on an adequate appreciation by participants in that deliberation of the goods of the practices to be ordered, and no political deliberator could have all of the knowledge required. No one can enter sufficiently into the multifarious practices that make up the life of a community to be able adequately to appreciate the goods internal to each of those distinct practices.<sup>81</sup>

I shall call this the *problem of justification*. Note that it is closely related to the original objection to the recognition claim, considered above: in many cases, it seems obvious that we can give a justification of practices of which we are not practitioners.

Recall the distinction between purely internal and partly external progress in §2.4.1. What exactly happens in cases of partly external progress? A practice is not an immaterial or ideal entity. It exists by and in the actions of its individual practitioners. So partly external progress involves some particular practitioners of two or more distinct practices taking up some internal good of one practice and modifying it or using it in such a way that a new internal good of another practice is created as a result. Granting for the moment that engineering is a practice, this is exactly what happened in the case of SLR cameras: some individuals, practitioners of both engineering and photography, took up certain internal goods of engineering and used them to create the SLR camera and the consequent novel internal goods for photography. Further, without photographers who were not also engineers, this instance of progress would not have been possible. I take this pattern to be general: partly external progress requires the interaction, by way of *joint practitioners* — individuals who are practitioners of two or more practices — between two practices. And, conversely, insofar as joint practitioners are inspired to apply the internal goods of one practice to another, the participation of joint practitioners will be a valuable source of partly external progress for a practice. Their work constitutes *joint practice*, the

<sup>81</sup> Mark Murphy, “MacIntyre’s Political Philosophy,” in *Alasdair MacIntyre*, ed. Mark Murphy (Cambridge: Cambridge University Press, 2003), 173-4.

production or achievement of the internal goods of one practice that simultaneously promotes the internal goods (most importantly progress) of another practice.

This connection between joint practitioners and partly external progress means that the practitioners — *in general* — of practice *B* can be given a good reason to value the work of participants of *A*, viz., the contributions that the participants of *A* have made in the past to the partly external progress of *B* and the reasonable expectation of similar contributions in the future. Hence the practitioners of *B* have a reason to value the internal goods of *A*, viz., the past and future contribution of the internal goods of *A* to the internal goods of *B*. That is, *A* has a justification in *B*.

The situation is similar to that of complementary practices: in both cases, *A* is justified because of its contributions to *B*. But, while the contribution of *A* is more “stable” and “regular” (in some sense) in the case of complementary practices, in cases of joint practice the contribution is contingent, unstable, and irregular. If this small number of individuals (the joint practitioners) are not influential or do not continue their work in both practices over time, the connection between the practices will weaken and disappear.

Joint practice can be relatively conservative, when it involves no change to the standards for the internal goods of either practice. Sadi Carnot’s *Reflections on the Motive-Power of Heat* was one of the first treatises in classical thermodynamics, and Carnot’s explicit goal was a theoretical understanding of the action and efficiency of steam engines.<sup>82</sup> The model he developed — called the Carnot cycle — was enormously theoretically fruitful, leading (in some sense) to the second law of thermodynamics, and was crucial for the work of Sir William Thomson (Lord Kelvin) about twenty years later.<sup>83</sup> Carnot himself was an engineer, and his work in *Reflections* arguably amounts to joint practice: the engineer’s interest in improving the efficiency of the engine stimulated progress in the physics of heat.<sup>84</sup> However, this progress did not involve any progress in the *standards* of either practice. Carnot assumed the then-prevailing caloric theory of heat, modeled the work done by the engine as produced by the flow or “fall” of caloric through the engine — an idea that seems

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<sup>82</sup> Sadi Carnot, *Reflections on the Motive Power of Heat*, second edition, ed. Robert Thurston (1824; John Wiley / Sons, 1897), [http://openlibrary.org/books/OL14037447M/Reflections\\_on\\_the\\_motive\\_power\\_of\\_heat](http://openlibrary.org/books/OL14037447M/Reflections_on_the_motive_power_of_heat) (accessed February 22, 2012).

<sup>83</sup> Martin Bailyn, *A Survey of Thermodynamics* (Woodbury, NY: American Institute of Physics Press, 1994), 75, ISBN: 0883187973.

<sup>84</sup> The story is rather more complicated than this, because there are two different ways in which scientific inquiry can be thought of as a practice, and these two disagree precisely on the relationship between pure and applied science, including pure physics and mechanical engineering. However, I use this example only to explicate and make plausible the first sentence of this paragraph: joint practice need not involve any change in standards.

to be inspired by the structure of Watt's engine — and his presentation is entirely numerical and algebraic. While Thomson's reconstruction of Carnot's work presents it in the notation of integral and differential calculus, this too is entirely conventional for the mid-nineteenth century. Carnot, for all his importance, was not Newton: his innovative work did not require the development of new branches of mathematics or new epistemological standards.

However, in at least some cases, joint practice is more radical, and involves such changes, to either or both practices. For example, Naomi Rosenblum argues that technical developments in photography in the second half of the nineteenth century — especially artificial lighting, photosensitive materials that required only a fraction of a second to expose an image, and inexpensive single-lens cameras — stimulated two major aesthetic developments. First, they made possible snapshots — photos taken by amateurs of scenes of everyday life — which in turn made possible the genre of social documentaries, such as Jacob Riis's *How the Other Half Lives*.<sup>85</sup> Second, the way in which photographs were composed and represented motion seems to have also influenced practitioners in other arts:

Besides a preference for high horizons and blurred figures [that is, as though caught in motion by a long photographic exposure], similar to that seen in numbers of stereographs of city streets and exemplified in Claude Monet's *Boulevard des Capucines* . . . the Impressionists broke with tradition in their preference for accidental-looking arrangements of figures that appear to be sliced through by the edges of the canvas in the manner of the photographic plate. Certain canvases by these painters also mimic the optical distortions of figure and space visible in stereographs.<sup>86</sup>

Lorraine Daston and Peter Galison have argued that the development of photography and its adoption by scientists had profound effects on the conception of objectivity: hand drawings that were “true-to-nature,” representing an ideal type, were replaced by photographs of particular individuals that provided “mechanical objectivity.”<sup>87</sup> Hans-Jörg Rheinberger's account of the development and adoption of the liquid scintillation counter [LSC] — an automated instrument for analyzing radiologically-tagged biological samples — is also an very interesting example.<sup>88</sup> The LSC was one

<sup>85</sup> Jacob Riis, *How the Other Half Lives: Studies among the Tenements of New York* (1890; New York: Charles Scribner's Sons, 1914), <http://books.google.com/books?id=3cFIAAAAYAAJ> (accessed February 22, 2012); Naomi Rosenblum, *A World History of Photography*, third edition (New York, London, and Paris: Abbeville Press, 1997), ch. 6, ISBN: 0789200387.

<sup>86</sup> Rosenblum, *A World History of Photography*, 259.

<sup>87</sup> Lorraine Daston and Peter Galison, *Objectivity* (Brooklyn: Zone Books, 2007), ISBN: 9781890951788.

<sup>88</sup> Hans-Jörg Rheinberger, *An Epistemology of the Concrete* (Durham and London: Duke, 2010), ch. 9, ISBN: 9780822345756.

of the first instruments that could automatically analyze a potentially unlimited set of samples without a human operator present, and in part for this reason it was rapidly and widely adopted by the burgeoning community of molecular biologists between about 1955 and 1970. This enabled dramatic changes in biological methodology and the social structure of biological labs: experiments could now involve taking thousands of samples, and human beings no longer had to perform certain kinds of exhaustive data collection. But the LSC could only be used to analyze samples that were radiologically tagged and chemically pure, and this contributed to the prominence of molecular biology during this time period.

In each of these cases, the engineering developments didn't just influence the content of the internal goods of the other practice; they actually changed the standards for those internal goods. Partly external progress, I take it, will often require this sort of relatively radical joint practice, and the influence of both practices on each other.<sup>89</sup>

We should expect influence to be controversial among practitioners of the practice being influenced. This is an instance of change, and as discussed in §2.4.2, we expect controversy over whether a given change actually constitutes progress. But I assume that, in many cases, joint practitioners will be able to make a case, according to the standards of practice *B*, that their work does indeed constitute progress in *B*. So they can justify their work as joint practitioners (their particular practice of joint practice) to other members of *B*, and in this sense the influence of practice *A* on *B* is *legitimate*. To use the photography example again: The photographer-engineers are able to make a case, according to the standards of photography, that the new cameras do indeed constitute progress in photography. So they can justify their work as photographer-engineers — their work combines the two separate practices into a sort of composite or overlapping practice — to other photographers, and in this sense the influence of engineering on photography is legitimate.

If the preceding discussion has been right, then joint practice is sufficient for the justification of a practice; and, when practices are not similar or complementary, joint practice is also necessary. However, this is inconsistent with the recognition claim. So let us consider that claim once more.

As best I can tell, the recognition claim's primary function in MacIntyre's body of work is to distinguish his own account of practical reason from both Humean and intuitionist accounts.<sup>90</sup> Neither Hume nor intuitionist meta-ethicists give an account of practices, much less give them a special place in practical reason. Furthermore,

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<sup>89</sup> The concept of influence here is unavoidably vague; a better understanding of influence — and many other aspects of joint practice — requires an empirical investigation that is simply beyond the scope of this dissertation. I hope to undertake at least some of this investigation in future work.

<sup>90</sup> See, for example, Alasdair MacIntyre, "Social Structures and Their Threats to Moral Agency," in *Selected Essays: Ethics and Politics* (Cambridge: Cambridge University Press, 2006), 186–204.

practical reason is not necessarily social on either of these accounts — successful practical reason does not necessarily require engaging in certain kinds of interactions with other practical reasoners (though Humeans might concede that it is very, very difficult to learn how to reason correctly or what one’s reasons are without doing so).

By contrast, in his reading of Wittgenstein’s paradox of the rule-follower, MacIntyre argues that “the fact that what counts as a good reason is determined by public standards,” and so “if I cannot make my reasoning intelligible as reasoning to others I shall be equally unable to make it so intelligible to myself.”<sup>91</sup> Given that the activity of practical reason is the activity of making my actions (whether past, present, or future) intelligible to myself, it follows that the exercise of practical reason requires that my reasons for my actions be intelligible to others. But “it is in the context of practices . . . that we are educated . . . so that certain kinds of reason become the causes of certain kinds of action”; this is because “within practices a shared recognition of goods and a shared acknowledgment of standards provides socially established forms of practical reasoning.”<sup>92</sup> That is, within practices, where certain internal goods are accepted as valuable and worth pursuing, we give reasons for action — give reasons for the claim that doing-such-and-such is worth doing — by reference to those internal goods. Within the practice of portraiture, for example, use of a particular painting technique can be justified by the fact that it enables the production of excellent portraits; and this justification of the technique can be used by a particular painter as reason for her use of that technique. In short, the justification of some of the internal goods of a practice in terms of other internal goods of the same practice. MacIntyre takes this to be a paradigmatic form of practical reasoning; by the recognition claim, it requires participating in at least one practice. Hence — and this conclusion is what MacIntyre is really after — a paradigmatic form of practical reasoning is necessarily social, unlike the Humean and intuitionist accounts.

The argument of the last paragraph does not require the full strength of the recognition claim as MacIntyre states it. Consider the following:

**weak recognition:** An individual  $a$  can give or accept a justification of practice  $A$  only if there is an individual  $b$  (not necessarily distinct from  $a$ ) such that

- (1) there is a practice  $B$  (not necessarily distinct from  $A$ ) of which both  $a$  and  $b$  are practitioners and
- (2)  $b$ ’s participation in both  $A$  and  $B$  provides some basis for the justification of  $A$  to  $a$ .

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<sup>91</sup> Alasdair MacIntyre, “Positivism, Sociology and Practical Reason: Notes on Durkheim’s *Suicide*,” in *Human Nature and Natural Knowledge: Essays Presented to Marjorie Grene on the Occasion of Her Seventy-Fifth Birthday*, ed. Alan Donagan, Anthony Perovich, Jr., and Michael Wedin (Dordrecht: D. Reidel, 1986), 94.

<sup>92</sup> *Ibid.*, 99.



Like recognition, weak recognition implies that the individual  $a$  must be a participant in *some* practice in order to engage in a sort of practical reasoning that MacIntyre considers paradigmatic. Hence the argument above still goes through. But unlike recognition, weak recognition is compatible with the solution to the problem of justification given above. Indeed, when both  $A$  and  $B$  and  $a$  and  $b$  are distinct, the consequent in weak recognition describes a situation in which  $b$  is a joint practitioner and  $b$ 's joint practice provides the basis for the justification of  $A$  to  $a$ .

Can the weak recognition claim be supported by a version of the argument from tacit knowledge, in anything like the way the strong recognition claim was supported by this argument in §2.3.1? Again, when the joint practitioner  $b$  gives a justification of  $A$  to  $a$ , the argument is that  $A$  can contribute to some other practice  $B$ . This requires, first, that  $a$  be able to recognize the internal goods of  $B$ . And the argument from tacit knowledge, exactly as give above, applies here. Next, the justification *does not* require that  $a$  be able to recognize the internal goods of  $A$ . But, paradigmatically, it *does* require that  $a$  be able recognize cases in which the internal goods of  $A$  are being utilized to realize the internal goods of  $B$ . And this seems to me to be another case of applying quite complex criteria. Hence the argument from tacit knowledge can be run here as well.

To see this last point, consider *how*  $b$  gives the justification to  $a$ . It is not enough for  $b$  to simply declare that  $A$  contributes to  $B$ . It seems to me that  $b$  will have to *show*  $a$  how  $A$  is utilized to contribute to  $B$ . For example, it is not enough for the engineer-photographer to simply declare that the SLR camera will revolutionize photography. She must show her fellow photographers how to use this new kind of camera, and encourage them to explore and familiarize themselves with the new capabilities that it creates. They cannot simply sit back and passively contemplate the new camera; they must be actively involved in learning how it can be used. Some guidance will presumably be required from the engineer-photographer here: these are the physical limitations of the new camera, here are some things that I've discovered with it. In this way, her participation in both engineering and photography provides the basis for the justification of engineering to the photographer. But this involves communicating — indeed, developing originally — tacit knowledge. And hence it seems that the argument from tacit knowledge can be applied to this aspect of the weak recognition claim as well.





# Chapter 3

## Science as practice

### 3.1 Introduction

In this chapter, I apply the conception of practice laid out in the previous chapter to *scientific inquiry*. That is, I show that scientific inquiry is a practice. Indeed, I develop two distinct conceptions or views of scientific inquiry as a practice.<sup>1</sup> The first — and, I think, the most widely-held — I call the “narrow view”; on this view, the primary internal good of scientific inquiry is “narrowly” construed as epistemic, or knowing-that. On the second, which I call the “broad view,” the primary internal goods are construed “broadly” as including not just knowing-that but also technology and the knowledge of how (knowing-how) to use it. In the next section, I present some points of agreement between these two views. In §3.3, I present the specifics of the narrow view. Then, in §3.4, I present the broad view. Starting in chapter 4, I argue that the disagreement over these two views is one of the basic disagreements in the science and values debate. Critical to this overall argument is the argument presented in §3.3.5, called the connection argument. Finally, in §3.5, I briefly characterize the disagreement between the broad and narrow views in the terminology of final ends.

### 3.2 Points of agreement

#### 3.2.1 Practitioners

The conception of practice is, fundamentally, an account of certain types of goal-oriented group activity. It is therefore a piece of social theory. So characterizing

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<sup>1</sup> Throughout this dissertation, I use “scientific inquiry” instead of the more familiar “science.” This is to stress that I am dealing primarily with the activities that we call “science” — science as a *practice* — and not just the products of those activities — including what I have elsewhere called “science as theory.” Daniel Hicks, “On the Ideal of Autonomous Science,” *Philosophy of Science* 78, no. 5 (December 2011): 1235–48, doi:[10.1086/662255](https://doi.org/10.1086/662255)

scientific inquiry as a practice is characterizing scientific inquiry as social. Thus, on both the narrow view and broad view strictly speaking, scientific inquiry is done generally and for the most part by groups, not isolated individuals. This does not imply that scientific inquiry can *never* be the activity of a single, isolated individual; rather, it implies that this sort of scientific inquiry is relatively rare and exceptional. In particular, without denying that such cases can exist, we do not need to take them into account when sketching a general account of scientific inquiry.

If scientific inquiry is indeed typically a social activity, then who are the members of the society? Who is in the “tribe of science?”<sup>2</sup> The most natural answer would be “scientists,” and indeed throughout the remainder of this dissertation I will use “scientists” to refer to all practitioners of scientific inquiry.

But just who are “scientists?” Consider a lab, with a single primary investigator (PI), a handful of subordinate researchers (in an academic context, typically post-doctoral research assistants and graduate students), an array of technicians, and such “support staff” as secretaries and janitors. There may also be administrators or research managers involved, for example, in assigning research problems to the PI or approving research proposed by the PI. To further complicate things, there may be “transient members” or “remote members,” individuals who have a great deal of influence on the laboratory but are not officially connected to it, such as close colleagues of the PI. Certainly some of these people — the PI, the post-doctoral research assistants, the graduate students — are among “the scientists,” while others — the secretaries and janitors, administrators with absolutely no scientific training whatsoever — are just as certainly not. But it is difficult, if not impossible, to make a neat division between scientists and non-scientists. Technicians may have a Bachelor’s degree or higher in the area of the lab’s research, but perform only “mechanical” tasks such as cleaning or measuring chemicals. Administrators or research managers may be former PIs themselves, or may be MBAs responsible for passing on orders from the heights of the corporate hierarchy.

Finer-grade and multi-dimensional distinctions may be useful here. We might, for example, try to formalize the divisions above in terms of education, specialization of tasks, and various sorts of authority or expertise. But this is not necessary for my purposes. Participation in a practice is not a simple, all-or-nothing matter; rather, individuals can move more-or-less continuously from completely outside the practice to deeply involved, and be involved in various respects while remaining uninvolved in others. Since participation is graded in this way and lacks clear and distinct boundaries, the set of practitioners will also be gradual and lack clear and distinct boundaries. In other words, my usage of “scientist” will basically agree with the common usage, including PIs, research assistants, and graduate students, but not administrators or janitors.

I will, however, make one major deviation from common usage, by including philosophers of science as scientists. As I will indicate below, the standards for the

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<sup>2</sup> This term is due to Janet Stemwedel.

internal goods of scientific inquiry include such things as normative epistemology. Insofar as philosophers of science are engaged in developing our understanding of normative epistemology, they are engaged in developing the standards for the internal goods of scientific inquiry. When successful, this is a paradigmatic kind of progress in scientific inquiry. And this progress is one of the internal goods of scientific inquiry, therefore etc. This inclusion of philosophers of science will be important for the argument of chapters 4 and 6.

### 3.2.2 Internal goods

In the last chapter, I identified four kinds of internal goods: goods, virtues, progress, and eudaimonia.

#### Goods

On any view of scientific inquiry as a practice the goods of scientific inquiry should be one of its primary aims. The goods of scientific inquiry are the *raisons de faire* of scientific inquiry. Further, in order to be truly *internal* goods, the goods of a practice should only be producible by engaging in that practice (or a very similar one). That is, they must be attached to the practice of scientific inquiry.

Both the narrow and broad views agree that *representational knowledge*, or simply *representation*,<sup>3</sup> is one of the goods of scientific inquiry. I use “representations” in an extremely broad way, covering everything from the sets of propositions or sentences structured by logical relations, as in Hempel’s work, to multiple layers of models, as in Suppes and van Fraassen “semantic” views of theories, to Mauricio Suárez’s pragmatic, inferential conception of representation.<sup>4</sup> In particular, when I speak of “representation” I will typically have in mind everything from systems of differential equations and ANOVA tables to photographs, maps, and chemical structural formulas.<sup>5</sup>

<sup>3</sup> I rarely treat “knowledge” or “representations” as propositional attitudes. A propositional attitude is a property or relation that a mind exemplifies or stands in, not a thing that the mind can be said to have. By contrast, books and diagrams and other physical representations can easily be transferred from scientist to scientist; hence they are distinct from the scientists themselves. Thus, propositional attitudes, unlike physical representations, cannot be goods of a practice as I defined them in the last chapter.

<sup>4</sup> See, for example, Carl Hempel, *Philosophy of Natural Science* (Englewood Cliffs, NJ: Prentice Hall, 1966); Patrick Suppes, “Models of Data,” in *Logic, Methodology, and Philosophy of Science: Proceedings of the 1960 International Congress*, ed. E. Nagel, P. Suppes, and A. Tarski (Stanford: Stanford University Press, 1962), 252–61; Bas Van Fraassen, *Scientific Representation* (Oxford and New York: Oxford University Press, 2008); Mauricio Suárez, “An Inferential Conception of Scientific Representation,” *Philosophy of Science* 71, no. 5 (December 2004): 767–79.

<sup>5</sup> For a discussion of the last of these, see William Goodwin, “Visual Representations in

Whatever the particular conception of representation attached to scientific inquiry, there will be accompanying standards for good representation. These will typically be articulated in epistemic or epistemological terms: this theory is empirically accurate, that dataset was produced by a reliable process, and so on. Just as I do not include any particular conception of representational knowledge in either view of science as practice (and, indeed, for exactly that reason), I likewise do not include or assume any particular epistemology.<sup>6</sup> Everything from falsificationism and Bayesianism to Kuhn's value epistemology and Longino's Habermasian process of localized critique and revision could be included in the standards for the good of representational knowledge.<sup>7</sup> At times, I will take this generality as a premise in arguments against other philosophers: those who also appear to assume no particular epistemology but still appeal to substantive epistemological assumptions.

Again, the key characteristic of internal goods is that they are attached to their practices, in at least two ways. First, they are attached in the sense that these internal goods can only be produced by the particular activities of the practice, and second, they are attached in the sense that these goods can only be defined by reference to the practice. Representational knowledge is attached to scientific inquiry in both senses.

In the first sense, representational knowledge in general can only be produced reliably by activities of investigation; hypothesis formulation, testing, and revision; and critical discourse; that is, by the activities characteristic of those practices we call *inquiry*. In particular, *empirical* representational knowledge can only be produced by those particular types of inquiry that involve making, recording, and analyzing observations of the world, often requiring deliberately manipulating the conditions under which these observations are made; that is, by the activities of field and laboratory scientific inquiry.

We must be clear about the degree of generality or specificity in the claim that representational knowledge can only be produced by scientific inquiry. For example, it would be clearly false to claim that representational knowledge in the most general sense can only be produced using the specific methods of contemporary scientific inquiry. But I take it to be clear that the specific representations produced by contemporary scientific inquiry — structural equation models and Feynman diagrams, for example — can only be produced by the specific methods of contemporary social and physical sciences. The specific ways in which we produce representational knowledge are, indeed, specific to us, and this is compatible with the fact that they are the same at some level of description as other representations produced in other times and places or by other activities.

As a solution to the traditional, conceptual demarcation problem — the problem

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Science," *Philosophy of Science* 76, no. 3 (July 2009): 372–390.

<sup>6</sup> In particular, I make no assumptions about the legitimacy of objectivity, in some sense, or the relationship between epistemic standards and ethical and political values.

<sup>7</sup> On Longino's debt to and differences from Habermas, see Longino, *Science as Social Knowledge*, 78 and 197ff.

of giving non-circular necessary and sufficient conditions for scientific inquiry — this is unsatisfying. But the second sense in which representation is attached to scientific inquiry suggests that the demarcation problem cannot be solved and I am not attempting to solve it here in any case. (Recall that, in this second sense, internal goods can only be defined by reference to the practice.) So long as I can, in principle, give an ostensive definition of practitioners and their activity — *here* are some chemists; *this thing they are doing* is chemistry — I can see nothing wrong with characterizing a practice in terms of its internal goods and simultaneously characterizing its internal goods as products of the practice. The circularity is not vicious.

Again, the second sense of attachment gives us the claim that representational knowledge can only be defined (at an appropriate level of specificity) by reference to scientific inquiry. Representational knowledge, as it is produced by contemporary scientific inquiry, represents the *natural, material, physical, immanent, or observable* parts of the world, as opposed to such *supernatural, immaterial, metaphysical, transcendent, or unobservable* parts of the world as might exist.<sup>8</sup> However, defining each of the emphasized terms in any remotely operationalizable way requires the use of the representational knowledge produced by scientific inquiry. As discussion of empiricist anti-realist views has repeatedly pointed out, any distinction between observables and unobservables must make reference to the sensory organs and cognitive capabilities of human beings in some way or another; hence, this distinction must be made with reference to the representational knowledge produced by physiology and cognitive science. Similarly, the only stable distinction between the physical parts of the world and the metaphysical parts over the last two hundred years has been built on the nominal definition that the physical parts are those parts studied by physicists.

Next, distinguish between *production-dependent* and *production-independent* standards for a given good of a given practice. As the name suggests, production-independent standards for a good do not depend (semantically) on the way the good was produced. For example, empirical adequacy is a production-independent standard for a representation, as the empirical adequacy of a representation does not refer to the way the representation was produced. On the other hand, production-dependent standards depend (semantically) on the way the good was produced. Longino's conception of objectivity is a production-dependent standard, because the content of the standard refers to the process of criticism-and-revision by which the representation was produced.

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<sup>8</sup> I use “metaphysical” here as a near-synonym with “supernatural” and so on, and in contrast with “physical.” In particular, I do not use it in the sense of “concerning the fundamental nature of reality.” To the extent that some physicists and philosophers of physics are or have engaged in physics as the investigation of the fundamental nature of reality, they are not engaged in metaphysics as I am using the term in question here. Contrast my usage with, for example, James Ladyman et al., *Every Thing Must Go: Metaphysics Naturalized* (Oxford and New York: Oxford University Press, 2007), ISBN: 9780199276196.

The standards for the goods of many goods-oriented practices are often production-independent. An evocative impressionist landscape, for example, might be evocative *because* of the particular paint preparation and brush techniques used by the painter — the virtues of the painter — but the standard itself — call it “evocativeness” — does not *semantically* depend on these techniques. Further, it is quite common for practitioners in these practices to have secret or private techniques for producing goods exemplifying these standards. A master painter, for example, might have developed a secret paint preparation and brush technique that produces just that evocativeness. Other practitioners — other painters — can recognize this evocativeness and praise the excellence of the evocative paintings without knowing how it was produced, even in a very general way. Similarly, a master baker may have a secret recipe for vegan carrot cake frosting that is widely recognized by other bakers as a great achievement of vegan baking (it’s quite hard to mimic the consistency of cream cheese using vegan ingredients) even though they do not know the secret. In both these examples, the standards are production-independent.

Are the standards for representational knowledge in scientific inquiry production-independent? As my earlier examples suggest, this depends on the specific normative epistemology. Note that the classic methodological distinction between the *context of discovery* and the *context of justification* can be made in these terms. In particular, the distinction can be recast as the claim that the standards for representational knowledge in scientific inquiry are production-independent. To deny this is to assert that features of the process by which the representation was produced — features of the context of discovery — are relevant to the standards for those representations — within the context of justification. Hence, if the standards for representational knowledge are production-*dependent*, the subject matter and methodology of history and sociology of science cannot be sharply distinguished from the subject matter and methodology of philosophy of science.

However, the discovery-justification distinction no longer seems to be widely accepted, even among philosophers of science who otherwise take the narrow view of scientific inquiry.<sup>9</sup> And many of the standards for excellent representational knowledge that are actually used are production-dependent. For example, in much of the social and biological sciences, that a dataset was produced using a randomized, controlled, double-blind experiment with a reasonably large cross-sectional set of test subjects — that it was produced in a certain way — is one of the most important standards for an excellent dataset.

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<sup>9</sup> Jutta Schickore and Friedrich Steinle, eds., *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction* (Dordrecht: Springer Verlag, 2006), ISBN: 1402042507.



### Virtues

Given that at least one good of scientific inquiry is representational knowledge, the virtues of scientific inquiry will include those excellences of scientists that are conducive to the production of representational knowledge. As a first pass, these virtues comprise excellent techniques and methodology for the provision and analysis of data. Here “excellent techniques and methodology” are determined, at least in part, by some normative epistemology. For example, suppose a given sequence of steps for preparing, growing, and analyzing a bacterial culture constitutes a reliable mechanism for producing representations concerning the presence or absence of particular types of bacteria. At least when presence of these bacteria is the subject of investigation, this sequence of steps will qualify as excellent methodology, and thus a virtue of this particular kind of scientific inquiry.

However, this conception of the virtues of scientific inquiry is inadequate, in at least two respects. First, MacIntyre explicitly requires that the virtues of a practice not all be purely technical skills.<sup>10</sup> If the “virtues” of scientific inquiry were exhausted by the conception of the last paragraph, scientific inquiry would not count as a practice. Fortunately, not all characteristically virtuous behaviors of scientists can be or must be understood in terms of their direct contribution to the production of representations; this is the second inadequacy. I will illustrate this second inadequacy with an interpretation of Robert Merton’s ethos of science.

Merton’s conception of the ethos of science is stated most directly in his “Science and Technology in a Democratic Order.”<sup>11</sup> Early in this piece, Merton defines the ethos of science as “that affectively toned complex of values and norms which is held to be binding on the man of science.”<sup>12</sup> The ethos therefore comprises at least some of the values and norms for the standards of excellent action of scientists — that is, virtues. However, Merton claims that they are not (or not only) techniques and methodology in at least two places. First, in the opening, Merton states that he “shall consider, not the methods of science, but the mores with which they are hedged about.”<sup>13</sup> Second, Merton claims that “the institutional imperatives (mores) derive from the goal and the methods” of “the extension of certified knowledge,” thereby giving the ethos “a methodologic rationale,” but are also “believed right and good” and hence “moral as well as technical prescriptions.”<sup>14</sup> If Merton is right in these claims, then the four imperatives he identifies as the ethos of science are good examples of virtues in the extended, not-purely-technical sense of the conception of practice.

<sup>10</sup> MacIntyre, *After Virtue*, 223.

<sup>11</sup> Anthologized as Robert Merton, “The Ethos of Science,” in *On Social Structure and Science*, ed. Piotr Sztompka (1942; Chicago: University of Chicago Press, 1996), 267–76.

<sup>12</sup> *Ibid.*, 267.

<sup>13</sup> *Ibid.*

<sup>14</sup> *Ibid.*, 268; my brackets, his parentheses.

The four imperatives of the ethos of science that Merton identifies are, famously, universalism, communism, disinterestedness, and organized skepticism. Briefly, universalism is the imperative that “truth-claims, whatever their source, [be] subjected to preestablished impersonal criteria” that do not depend on “race, nationality, religion, class, and [other] personal qualities.”<sup>15</sup> In my jargon, universalism can be recast as the virtue of evaluating the goods produced by scientific inquiry without reference to the “personal qualities” of the scientists who produced them. A scientist exemplifying this virtue treats other scientists as equals, or values them according to their production of representational knowledge, and without regard to their personal qualities.

Second, communism is the virtue of treating “the substantive findings of science . . . [as] a product of social collaboration” and “a common heritage in which the equity of the individual producer is severely limited.”<sup>16</sup> That is, the goods (or perhaps just the internal goods) of scientific inquiry are produced and held by scientists collectively. Scientists compete among each other for “recognition and esteem” — external goods — but, when things are working properly, these are “roughly commensurate”<sup>17</sup> with the production of knowledge — internal goods. Hence the external goods are treated as mere incentives and rewards that facilitate the “essentially cooperative”<sup>18</sup> production of the internal goods; more generally, then, this virtue presupposes a distinction of a kind with the distinction between internal and external goods.

Merton does little to characterize directly the imperative of disinterestedness; instead, he presents contrasting examples of fraud, quackery, chicanery, and propagandizing.<sup>19</sup> I suggest reading these contrasting examples as cases of institutional conflict and priority: the epistemic standards for representational knowledge have been sacrificed for the sake of such external goods as wealth, power, and status. The virtue of disinterestedness would then be the virtue of practical priority. That is, disinterestedness is the virtue of settling cases of institutional conflict in favor of the internal goods of scientific inquiry and at the expense of external goods.

Finally, organized skepticism is “the temporary suspension of judgment and the detached scrutiny of beliefs in terms of empirical and logical criteria,”<sup>20</sup> with a contrast of these criteria with those of organized religion. This virtue could also be called open-mindedness or, in an epistemological mood, fallibilism; a scientist who exemplifies this virtue is willing to subject any of her claims to critical scrutiny.<sup>21</sup>

<sup>15</sup> Merton, “The Ethos of Science,” 269.

<sup>16</sup> *Ibid.*, 271.

<sup>17</sup> *Ibid.*, 272.

<sup>18</sup> *Ibid.*, 273.

<sup>19</sup> *Ibid.*, 275-6.

<sup>20</sup> *Ibid.*, 276.

<sup>21</sup> Merton says nothing requiring this skepticism to be of a wholesale, foundationalist, Cartesian type — the scientist does not need to subject *all* of her claims to *complete* critical scrutiny *at once*. The Mertonian ethos thus seems to be compatible, in a weak sense, with a Kuhnian paradigm.



All four imperatives of Merton's ethos are standards or norms for excellent actions of scientists. They are not, however, excellences because they directly conduce to representational knowledge, especially in the way that, for example, a reliable method for provisioning and analyzing data does. Universalism and organized skepticism are necessary for the representational knowledge produced by the practice to be free of distortion and false assumptions, but they act more as filters — eliminating false and unjustified claims through critical scrutiny — and less as techniques for the original production of representations. Communism and disinterestedness, as I have recast them, are even further removed from the production of the good of representational knowledge, as they have to do with the relations between this good and external goods. While they are necessary for scientific inquiry to flourish and maintain itself over time, they are not directly involved in the production of representations. Hence they qualify as virtues for the practice as scientific inquiry, in the extended and non-technical sense.

Of course, Merton's ethos is not, for the most part, an accurate description of the way scientists actually behave. But this is part of its ethical function: Merton himself uses the ethos to criticize a number of aspects of scientific inquiry as it was practiced in 1942, including the abuse of the patent system for private commercial gain (incompatible with communism and disinterestedness) and tensions with nationalist movements and closed-minded religious traditions (borne of universalism and organized skepticism, respectively). While Merton's sociology of science has sometimes been read as adhering to a strict "positivist" dichotomy between factual description and ethical guidance and a corresponding disciplinary division between the tasks of sociology and philosophy,<sup>22</sup> this is blatantly incompatible with Merton's actual work. At the beginning of "Science and Technology in a Democratic Order," Merton identifies two distinct yet interdependent aims for his project. The first is the identification of the content of the ethos; the second is the identification of the socio-political context that provides "the fullest measure of development" for this ethos.<sup>23</sup> Both aims are simultaneously descriptive and ethical.

Merton's ethos of science is perhaps best thought of as analogous with the character of the virtuous person in Aristotelean ethical philosophy: comprising several somewhat idealized yet not completely unrealistic standards for excellent action against which actual individuals compare potential and actual actions when engaged in practical deliberation. It is no deep criticism of either Merton's ethos or the character of the virtuous person to say that it is only realized sporadically and imperfectly, as neither account is meant to be strictly descriptive.

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<sup>22</sup> John Zammito, *A Nice Derangement of Epistemes* (University of Chicago Press, 2004), 129ff.

<sup>23</sup> Merton, "The Ethos of Science," 268.

## Progress

In §2.4.3, I made a distinction between telic and melioric progress, progress relative to some fixed or given end and progress relative only to the past and present. As that discussion indicated, there is a longstanding debate among philosophers and historians of science over which type of progress is most appropriate for scientific inquiry. All or almost all of the views I discussed there dealt with representational knowledge; they merely differ on the standards by which improvement in that knowledge is judged. Similarly, different versions of both the narrow and broad views will be compatible with different views of the long-term aims of scientific inquiry, including whether or not there is such an aim. Hence, just as both views of science as practice are neutral between particular conceptions of representation and normative epistemology, they are also neutral between telic and melioric conceptions of progress.

### 3.2.3 Institutions

The concepts of an institution, institutional conflict, and institutional domination, applied to the internal good of representational knowledge, provide sufficient resources to characterize two kinds of institutional domination, *commercialization* and *politicization*.

Commercialization is a type of institutional domination involving a conflict between the internal goods of scientific inquiry — such as representational knowledge — and the external good of wealth. We can identify some clear examples of commercialization in recent history. James Robert Brown has compiled sources showing that commercially-sponsored pharmaceutical research is much more likely than independently-sponsored research to give favorable assessments of new drugs.<sup>24</sup> Justin Biddle has argued that Merck “consistently mischaracterized the current state of knowledge regarding the possible cardiovascular side effects of Vioxx” and that “there are serious questions about whether Merck honestly reported data that was unfavorable to its financial interests.”<sup>25</sup> The European Commission has reached somewhat similar conclusions with respect to European pharmaceutical research in its 2009 Pharmaceutical Sector Inquiry Report.<sup>26</sup>

Brown has gone further, arguing that patents should be eliminated in the domain of medical research and that public funding of medical research should be dramatically increased.<sup>27</sup> In light of my reconstruction of MacIntyre’s first argument for the

<sup>24</sup> James Robert Brown, “The Community of Science<sup>®</sup>: Science and Values Revisited,” in *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, ed. Martin Carrier, Don Howard, and Janet Kourany (Pittsburgh: University of Pittsburgh Press, 2008), 191.

<sup>25</sup> Justin Biddle, “Lessons from the Vioxx Debacle: What the Privatization of Science Can Teach Us about Social Epistemology,” *Social Epistemology* 21, no. 1 (2007): 27.

<sup>26</sup> *Pharmaceutical Sector Inquiry Final Report* (European Commission, July 8, 2009).

<sup>27</sup> Brown, “The Community of Science<sup>®</sup>,” 209.

cardinal virtues in §2.5, we can read calls like Brown’s as calls for a return to the cardinal virtues in scientific inquiry, in a way managed or facilitated by the state: to counteract the corrosive forces of rampant institutional domination, scientific practice must return to practical priority, possibly even of an extreme version.

Politicization is a type of institutional domination that occurs when the pursuit of the internal goods of some practice are systematically and persistently sacrificed for the sake of institutional acquisition of power in some political system.<sup>28</sup> On standard accounts, both Nazi racial medicine and Lysenkoism in the Soviet Union were cases of politicization. In a contemporary context, politicization is the particular form of institutional domination associated with science policy, that is, “the decision process through which individuals and institutions allocate and organize the intellectual and fiscal resources that enable the conduct of scientific research.”<sup>29</sup>

As in Brown’s criticisms of commercialization, the threat of politicization leads some holders of the narrow view to call for practical priority in science policy. For example, in the epilogue to his popular polemic against Republican politicization of science policy,<sup>30</sup> Chris Mooney gives eight distinct suggestions for addressing the problem. At least five of these call for practical priority in one form or another. In one case, Mooney asserts that

elected representatives have no business specifying, in minute detail, how federal agencies [such as the EPA] should evaluate scientific information. We staff these agencies with scientific experts for a reason. Let’s let them do their jobs.<sup>31</sup>

In another suggestion, he argues that members of *another* practice — journalism — must substitute the internal standards of scientific inquiry for their own:

Reporters need to understand better how science abusers exploit the journalistic norm of ‘balance’ — demanding equal treatment for fringe or widely discredited views — and adjust their writing accordingly. Let’s face it: Journalistic ‘balance’ has no corollary in the world of science. On the contrary, scientific theories and interpretations survive or perish based on the process of peer review . . . . When consensus builds, it is based on repeated testing and retesting of an idea.<sup>32</sup>

<sup>28</sup> Note that I am giving a particular definition for politicization here. There may be cases of “politicized science” that do not count as politicization according to this definition. Three rival accounts of politicization will be discussed in §7.2.

<sup>29</sup> Daniel Sarewitz et al., “Science Policy in Its Social Context,” *Philosophy Today Supplement* 3, no. 8 (2004): 67.

<sup>30</sup> Chris Mooney, *The Republican War on Science*, paperback edition (New York: Basic Books, 2005/2006).

<sup>31</sup> *Ibid.*, 266.

<sup>32</sup> *Ibid.*, 267.

Note that the “war” that, Mooney argues, is being waged by Republican operatives is a war on the epistemic quality of scientific inquiry: as he defines it, politicization is “any attempt to inappropriately undermine, alter, or otherwise interfere with the science processes, or scientific conclusions, for political or ideological reasons,”<sup>33</sup> where these processes produce “a fount of useful information.”<sup>34</sup>

All together, then, both conceptions of scientific inquiry as a practice agree that the practitioners are “scientists,” in some sense; that representational knowledge is among its primary internal goods; that at least some of its virtues, such as Merton’s ethos of science, go beyond merely reliable methodology; and that commercialization and politicization are forms of institutional domination with which scientific inquiry should be concerned.

### 3.3 The narrow view

In this section, I present the narrow view of scientific inquiry as a practice, in terms of its internal goods. Recall that the taxonomy includes four kinds of internal goods: goods, virtues, progress, and eudaimonia. After discussing each of these four, I argue that this view leads to concerns about institutional domination, which I call the connection hypothesis.

#### 3.3.1 Goods

The *narrow view is defined by two claims*. First, the sole good of scientific inquiry is representational knowledge. Second, the narrow view is goods-oriented; that is, on the narrow view, the goods of scientific inquiry are more important than its virtues. Or, to collapse these two into one: *The primary internal good of scientific inquiry is its unique good, representational knowledge*. Scientific inquiry, on the narrow view, is completely organized around the production of representational knowledge. Historians and social scientists of science within the narrow view will be concerned primarily with the processes by which representations are produced; philosophers of science will be concerned primarily with the ontological implications of various representations and the general standards for the justification of representation, that is, with the metaphysics and epistemology of scientific representation.

The prevalence of the narrow view is, I think, obvious, especially among philosophers. Debates over realism or such methodological notions as confirmation and explanation are debates over the best way to construe the representational knowledge produced by science and its standards. If representational knowledge is just one of the aims of scientific inquiry, no more important than others, then why has it enjoyed so much attention? Similarly, the debate over the legitimacy of ethical and

<sup>33</sup> Mooney, *The Republican War on Science*, 17.

<sup>34</sup> *Ibid.*, 15.

political values in scientific inquiry has been, almost exclusively, a debate over the legitimacy of ethical and political values in setting the standards for good representation. How can this exclusivity be explained except by the fact the interlocutors have taken representation to be the primary product of scientific inquiry?

It is more difficult to defend the claim that the narrow view is prevalent among historians and social scientists. On one hand, much social scientific research on the natural sciences has focused on the processes by which representations are produced. On the other hand, the social constructivist-relativist works of some historians and sociologists can be read as arguments against (mid-century versions of) the narrow view. For example, one could read (correctly or incorrectly) Shapin and Schaffer's *Leviathan and the Air-pump* as an argument that scientific inquiry in Restoration England was not, not importantly, or not primarily a practice for producing representational knowledge; it was, instead, just another site of political struggle.<sup>35</sup> And yet even this reading might still be compatible with the narrow view if it is understood as a charge of institutional domination.

### 3.3.2 Virtues

As a goods-oriented view, the narrow view takes the production of good representational knowledge to be more important, in general, than the realization of such virtues as those found in Merton's ethos. Or, virtues are valued for the sake of the goods. This does not imply that the virtues are valued only because of their direct contribution to the production of representations or, in a consequentialist spirit, that these virtues are to be cast aside whenever they interfere with the production of representational knowledge. Instead, they will be valued for being conducive — both directly and indirectly — to the production of excellent representations. For example, the virtues in Merton's ethos will be valued for the way they maintain the trust, respect, and free flow of information that is crucial for the coordination of the work of all biologists across the world.

This relationship may be difficult to untangle in the case of production-dependent standards for goods, especially standards for excellent representations that depend on virtues of the scientists that produced them.<sup>36</sup> Consider the virtue of refusing to plagiarize, fudge, or simply make up one's data. One excellence of a dataset is that it was produced by a scientist who exemplifies such a virtue; this is a production-dependent standard. Indeed, the exemplification of the virtue seems to be both necessary and sufficient for the dataset to be excellent in this respect. And so it might seem that this virtue is not *valued for the sake of* the excellence of the good; rather, the virtue is more-or-less *constitutive of* the excellence of the good.

<sup>35</sup> Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985).

<sup>36</sup> I thank Paul Weithman for encouraging me to discuss this point.

But the line of thought in the last paragraph conflates necessary and sufficient conditions with simple identity. The excellence of the scientist is necessary and sufficient for the excellence of the representation, but since the scientist and the representation are distinct, so are their excellences. This gives us enough room to maintain that the representation is primary, and the virtue merely a means to achieve it: the excellence of the scientist is valuable (that is, is an excellence) because of or for the sake of the excellence of the representation. Or, to put the point another way, the feature of the good *logically* and *causally* depends on the virtue, but the value of the virtue *axiologically* depends on the good. It is this last, axiological, sense that we have in mind when we say that goods are more important or prior to virtues.

### 3.3.3 Progress

The definition of the narrow view may be thought to imply a telic conception of progress: the progress of scientific inquiry is measured against an ideal (perhaps only a regulative ideal) of a complete and perfectly accurate — we might say, maximally true — representation of the world.

This implication does not follow. We already encountered two serious problems with it in §2.4.3, when we considered MacIntyre’s view that the conception of practice requires a telic conception of progress. First, while MacIntyre, Popper, and other scientific realists do seem to think that some sort of maximally true representational knowledge is the *telos* of scientific inquiry, van Fraassen and Michael Friedman do not do so. According to these anti-realists, scientific inquiry does have a *telos*, but it is not maximal truth. For example, on Friedman’s Habermasian view, the ideal is “a convergent sequence of successive frameworks or paradigms, approximating in the limit . . . an ideal state of maximally comprehensive communicative rationality,” but this “need not imply a second and further conception of convergence, according to which successive scientific theories are viewed as ever better approximations to a radically external world.”<sup>37</sup> The second problem was that Kuhn, Laudan, and Dewey all have melioric conceptions of progress for scientific inquiry. As we’ll see later, Dewey holds something more like the broad view than the narrow view; but Kuhn and Laudan both seem to hold the narrow view.

More recently, some philosophers of science have argued explicitly against telic conceptions of progress for scientific inquiry from within the narrow view (though, of course, not using that terminology). Philip Kitcher, for example, has borrowed the following argument from Ronald Giere:

[H]uman interests change and, in consequence, maps are drawn with very different reading conventions. We now have little use for the Tudor maps that showed shepherding trails and the boundaries of manors. Nor do we expect that our maps of the globe will display the significance of

<sup>37</sup> Friedman, *Dynamics of Reason*, 67.

Christ's passion. The reading conventions for many older maps are very different from those of the present, but the change should not surprise us. Reading conventions identify the ways of dividing the spatial domain that are of interest to the map-maker, and those conventions depend on the goals and the institutions of the society in which the map is to be used.<sup>38</sup>

A map, of course, is a piece of representational knowledge; this suggests that Kitcher takes the production of representational knowledge to be the sole, primary, or at least paradigmatic internal good of scientific inquiry. But Kitcher does not expect the production of representations to converge over time to some one, maximally best, representation. Rather, as the features of the world in which we are interested change, the standards for our representations will also change. Insofar as the former change is non-telic, so is the latter.

In short, the narrow view as such is not identified with any conception of scientific inquiry on which a complete, perfectly accurate, or trans-historical true theory is taken to be the ultimate aim. Certainly many who have held the narrow view have also held this telic conception of progress, but we have seen several examples of those who do not.

### 3.3.4 Eudaimonia

As I defined it in §2.3.2, the internal good of eudaimonia is the transfer of the internal goods of one practice to another activity or practice.<sup>39</sup> In the discussion of the justification of practices in §2.6, I argued that the activity of joint practitioners is essential for realizing eudaimonia in this sense. Combined with the definition of the narrow view, these points suggest defining eudaimonia for scientific inquiry as the application of representational knowledge to practical or non-scientific problems: scientists as such produce true theories (or excellent representations in some other sense) and applied scientists and engineers (who occupy the margins of the practice, between scientists and non-scientists, on the narrow-view) apply those theories to build bridges, waste treatment plants, cars, MRIs, computers, or other useful artifacts. Applied scientists and engineers do not, as such, contribute to the production of the primary internal goods of scientific inquiry; they merely apply those primary internal goods to non-scientific concerns. Call this conception of the internal good *eudaimonia as applied science*.

<sup>38</sup> Kitcher, *Science, Truth, and Democracy*, 58.

<sup>39</sup> As a point for clarity, recall that this definition is *not* intended to somehow match up with “eudaimonia” as understood in the Aristotelean tradition. I am merely borrowing the term analogically.



### 3.3.5 The connection argument

Eudaimonia as applied science is one way of understanding the traditional distinction between pure and applied science. Combined with the other resources of the general conception of practice and a few additional assumptions, the narrow view's version of the distinction leads to a belief — accurate or inaccurate — in what I call the threat of domination; this claim is the *connection hypothesis*. More precisely, it runs as follows:

**connection hypothesis:** If a scientist holds the narrow view of scientific inquiry then *ceteris paribus* she will believe any possible influence of any non-scientific practice on the standards for scientific inquiry to threaten institutional domination.

The connection hypothesis itself may not seem like an especially novel or surprising claim. What is novel is the explanation that I offer for it, which I call the *connection argument*. This argument is an instance of explanation by mechanism. I explain the connection between the narrow view and worries about institutional domination by describing how certain entities (including scientists who hold the narrow view) and their activities (especially their self-understanding of the social organization of scientific inquiry), arranged in certain ways, tend to produce certain results or terminate in certain end-states (worries about institutional domination).<sup>40</sup> The mechanism by which the narrow view produces these worries is, for lack of a better term, sociological; hence, the connection argument is a sociological explanation for a relationship between (some) beliefs of (some) agents. It is not, to be clear, a logical explanation; I do not claim that the narrow view entails or implies the threat of domination.

The connection argument is somewhat long but, when taken slowly, is, I hope, straightforward. The subarguments or major moves of the connection argument can be thought of as stages in the description of the activity of the mechanism, or transitions between stages. These major moves are illustrated in fig. 3.1. The entire formal statement of the argument is presented in fig. 3.2.

(CA-1) If a scientist holds the narrow view of scientific inquiry then her beliefs about applied science will conform to eudaimonia as applied science.

<sup>40</sup> Cf. Peter Machamer, Lindley Darden, and Carl Craver, "Thinking about Mechanisms," *Philosophy of Science* 67, no. 1 (March 2000): 1–25. I take no position on the debates that have followed this paper, concerning the relative priority of laws and mechanisms. I simply adopt the conception of mechanism presented by Machamer *et al.*, however it relates to laws. Also, this definition implies neither that mechanisms are determinist nor that they are unconscious. That is, mechanisms need only function generally or for the most part or reliably, not with any sort of necessity; and there is no reason to think that the entities that a mechanism comprises cannot include self-conscious persons who deliberate about the operation and function of the mechanism.



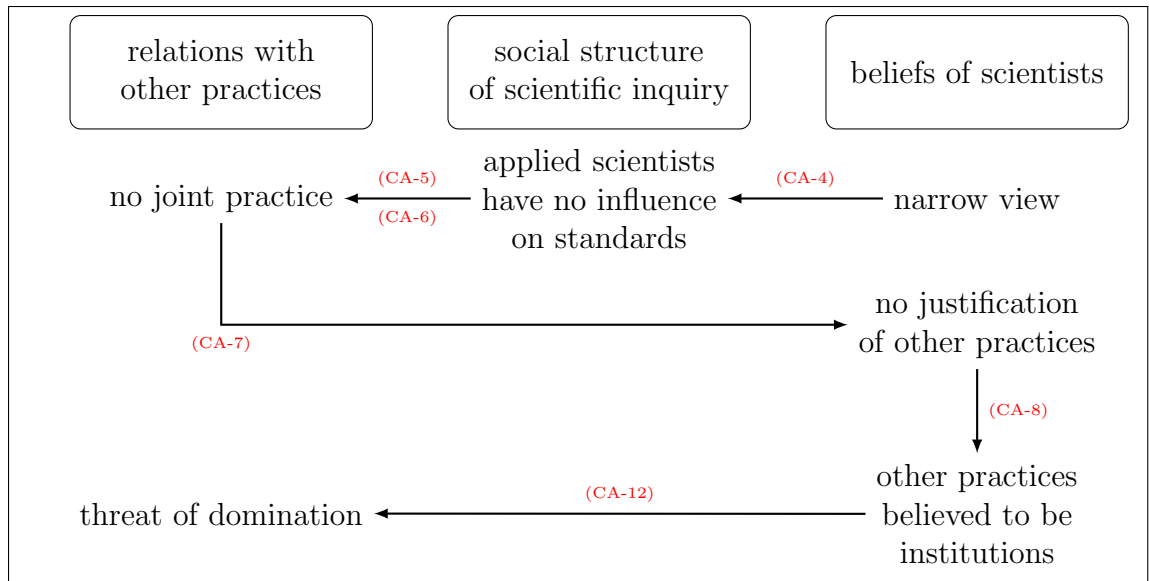


Figure 3.1: The Connection Argument

The first premise simply summarizes the argument of the last subsection.

The next premise describes a familiar thought.

- (CA-2) Eudaimonia as applied science rationalizes an asymmetry of influence between the work of pure and applied scientists, as such.

According to eudaimonia as applied science, there are two distinct kinds of scientists: pure scientists, who produce representations, and applied scientists, who apply representations to non-scientific activities. Only pure scientists are scientists in the primary sense that they produce the primary internal goods; applied scientists may be called scientists, but only by an analogy between their work and the work of pure scientists. Because only pure scientists produce the primary internal goods, the work of applied scientists does not contribute to the work of pure scientists. If this were not the case, the representations produced by the pure scientists would not be the primary internal good. Hence the work of the pure scientists influences the work of applied scientists, but not vice versa. In this sense, there is an asymmetry of influence between the work of pure and applied scientists.

However, the line of thought of the last paragraph is too simple, in two respects. First, some individuals may make contributions to both representations and the application of those representations to non-scientific problems; to return to the photography example from the previous chapter, a chemist might start in one lab, studying the photoreactive properties of certain substances, and later move to another lab, where she uses the representations produced in the first lab to develop a new sort of film. This can be accommodated by dividing the work of these individuals into work as pure scientists and work as applied scientists: as pure scientists they develop new

- (CA-1) If a scientist holds the narrow view of scientific inquiry then her beliefs about applied science will conform to eudaimonia as applied science.
- (CA-2) Eudaimonia as applied science rationalizes an asymmetry of influence between the work of pure and applied scientists, as such.
- (CA-3) If there is an asymmetry of influence between the work of pure and applied scientists, as such, then the work of applied scientists, as such, does not influence the standards for pure scientific inquiry.
- (CA-4)  $\therefore$  If a scientist holds the narrow view of scientific inquiry then she believes that when scientific inquiry is organized properly the work of applied scientists, as such, does not influence the standards for pure scientific inquiry. (1-3)
- (CA-5) If the work of applied scientists, as such, does not influence the standards for pure scientific inquiry then their work, as such, cannot provide a basis for the justification of a non-scientific practice to scientists.
- (CA-6) *Ceteris paribus*, on the narrow view, the work of applied scientists, as such, provides the only possible basis for a justification of any non-scientific practice to scientists.
- (CA-7)  $\therefore$  If the work of applied scientists does not influence the standards for pure scientific inquiry then *ceteris paribus*, on the narrow view, there is no basis for a justification of any non-scientific practice to scientists. (5,6)

Figure 3.2: The Connection Argument

representations of photoreactivity; as applied scientists they apply these representations to design new kinds of film. The asymmetry of influence is not between the work of two classes of individuals, but rather between two social roles; and there is nothing wrong with individuals moving between these roles, even quite frequently. So we add the clause “as such,” to indicate that we are dealing with roles, not individuals.

Second, the last few paragraphs describe the way things work when they are working properly, not the way they must always work. So eudaimonia as applied science *rationalizes* this asymmetry.

In the third premise, we specify one feature of this asymmetry of influence.

- (CA-3) If there is an asymmetry of influence between the work of pure and applied scientists, as such, then the work of applied scientists, as such, does not influence the standards for pure scientific inquiry.

Nothing about the non-scientific or practical purposes to which the applied scientists will put the representations is used to evaluate those representations. The standards for excellent representations are those of normative epistemology as classically understood. In this sense, the work of applied scientists is marginalized.

- (CA-8) If there is no basis for a justification of a non-scientific practice to scientists then that practice will be believed, by scientists, to be an institution.
- (CA-9)  $\therefore$  If the work of applied scientists does not influence the standards for pure scientific inquiry then *ceteris paribus*, on the narrow view, every non-scientific practice will be believed, by scientists, to be an institution. (7,8)
- (CA-10)  $\therefore$  If a scientist holds the narrow view of scientific inquiry then *ceteris paribus* either she will believe scientific inquiry to be not properly organized or every non-scientific practice is believed to be an institution. (4,9)
- (CA-11) If scientific inquiry is not properly organized then it is threatened by institutional domination.
- (CA-12) If the standards for scientific inquiry are possibly influenced by (what is believed to be) an institution, then scientific inquiry is threatened by institutional domination.
- (CA-13)  $\therefore$  If a scientist holds the narrow view of scientific inquiry then *ceteris paribus* she will believe any possible influence of any non-scientific practice on the standards for scientific inquiry to threaten institutional domination. (10-12)

Putting together the premises so far, we have the following:

- (CA-4)  $\therefore$  If a scientist holds the narrow view of scientific inquiry then she believes that when scientific inquiry is organized properly the work of applied scientists, as such, does not influence the standards for pure scientific inquiry.

In short, the narrow view leads to certain views about the way the tribe of science should be organized.

Next, consider what happens to scientific inquiry when it is organized in this way. With respect to other practices whose primary internal goods are representational knowledge — in short, other kinds of scientific inquiry and perhaps also other intellectual practices and academic disciplines — pure scientists may be joint practitioners: there is no problem, on the narrow view, with someone pursuing both physics and philosophy of physics, and doing so in such a way that their physics influences their philosophy of physics and vice versa. However, with respect to any other activity, the relationship between the representations of scientific inquiry and the aims of the other activity will be thought of as the *application* of the representations to the aims of the other activity. That is, joint practice can only be applied science. So, for any non-scientific practice, the only possible joint practitioners are applied scientists.

But applied scientists are marginalized. Their work, as such, does not influence the standards of excellent representational knowledge. Hence their work cannot stimulate partly external progress in scientific inquiry, and hence cannot provide a basis for the justification of some other practice to their fellow scientists.

If, in addition, the work of joint practitioners is the *only* basis for such a justification, then it follows that there will be *no* such justification. Scientists, as such, will have no reason to accept the claim that the aims of this other activity are good and worth pursuing.

The move of the last paragraph is a simplification. In line with several of the examples surrounding the recognition claim in the last chapter, it may be possible for a non-practitioner to come to understand and value the practice. For example, even though it is impossible for any living human to practice the life of a *samurai*, we may read about and watch dramatizations of the lives of samurai (real and fictional) and thereby come to understand and value their practice, without any joint practice that relates their practices to our own. But considering all of the possible ways in which a practice may come to be justified to a non-practitioner, and ruling them out in the case of the narrow view, would significantly complicate and lengthen this already-overlong dissertation. So, to cover such possibilities while keeping my discussion relatively perspicuous, I introduce a *ceteris paribus* clause in the relevant steps below.

As an illustration of the point I am after, consider a psychologist who objects to the use of representations concerning (purported) race-linked innate differences in I.Q. in the formation of education policy. Her criticisms invoke both issues of racial justice and issues of normative epistemology and methodology: these policies ignore the lingering effects of white supremacy and the research ignores possible environmental influences on I.Q. On the narrow view, insofar as the latter critiques (should) influence the standards for I.Q. research, she is arguing as a pure scientist, pointing out a purely epistemological problem. The former critiques are, at most, a matter of the application of the representations produced by psychologists to issues of public policy, and have nothing to do with the work of (pure) psychologists as such; these critiques are irrelevant to research on I.Q., and should not influence the standards of excellence for such research. Even if the psychologist intertwines these critiques in a single essay, the narrow view will see her, at best, as moving between the roles of pure and applied scientist. Her methodological point may be well-received, even stimulate radical changes in the field, but the anti-racist point is (or should be) utterly irrelevant and uninfluential. Hence any progress within psychology that results from her work will be purely internal, not partly external, and so it does not provide a basis for the justification of the anti-racist movement to other psychologists.<sup>41</sup>

We can sum up the observations of the last few paragraphs in the following sub-argument.

(CA-5) If the work of applied scientists, as such, does not influence the standards for

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<sup>41</sup> This example suggests that my account may be able to capture certain key aspects of the experience of the “outsider within,” an important concept for standpoint epistemology. See, for example, Patricia Hill Collins, “Learning from the Outsider Within: The Sociological Significance of Black Feminist Thought,” in *The Feminist Standpoint Theory Reader*, ed. Sandra Harding (Routledge, 2004), 103–28.

pure scientific inquiry then their work, as such, cannot provide a basis for the justification of a non-scientific practice to scientists.

- (CA-6) *Ceteris paribus*, on the narrow view, the work of applied scientists, as such, provides the only possible basis for a justification of any non-scientific practice to scientists.
- (CA-7) ∴ If the work of applied scientists does not influence the standards for pure scientific inquiry then *ceteris paribus*, on the narrow view, there is no basis for a justification of any non-scientific practice to scientists.

Next, in line with the weak recognition claim, when there is no justification for a practice (to some group of non-practitioners), it will seem (to those non-practitioners) to not be a practice at all. It may not be recognized as organized, purposeful activity; if it is recognized as organized and purposeful, then it will seem that it is engaged in the pursuit of such goods as wealth, power, fame, status, or happiness. That is, it will seem to be an institution. Hence,

- (CA-8) If there is no basis for a justification of a non-scientific practice to scientists then that practice will be believed, by scientists, to be an institution.
- (CA-9) ∴ If the work of applied scientists does not influence the standards for pure scientific inquiry then *ceteris paribus*, on the narrow view, every non-scientific practice will be believed, by scientists, to be an institution.

Return now to our narrow view-holding scientist. Even if she does not think in terms of the conception of practice, she will understand the organization of scientific inquiry, and the dynamics of its social interactions, in line with the argument above: if scientific inquiry is organized correctly, the work of joint practitioners will be understood as applied science and (yet) any progress this work might stimulate will be understood as purely internal progress. Any efforts by joint practitioners to justify other practices in terms of partly external progress will seem, to her, to be nonsensical and non-sequiturs. Because of her understanding of scientific inquiry, it is absurd to think that, say, the anti-racist movement can influence psychological methodology, at least so long as things are going well in psychology. Hence the argument for the last subconclusion (CA-9) can be carried out with respect to her — not, as it were, in terms of what she consciously believes, but in terms of how she understands her practice. So we have the following:

- (CA-10) ∴ If a scientist holds the narrow view of scientific inquiry then *ceteris paribus* either she will believe scientific inquiry to be not properly organized or every non-scientific practice is believed to be an institution.

(Note that I have rewritten the conditional, “if scientific inquiry is organized properly then ...,” as the equivalent disjunction, “either scientific inquiry is not organized properly or ...”)

Finally, assume as a simplification that if scientific inquiry is not perceived to be properly organized then it is perceived to be suffering institutional domination. And, for the other half of the dilemma, if a non-scientific activity is perceived to be an institution, then any potential influence of this activity on scientific inquiry will (be perceived to) threaten scientific inquiry with institutional domination. For example, if the anti-racist movement is (perceived to be) nothing other than the pursuit of political power for certain racial groups at the expense of others, then any influence of this movement on scientific inquiry threatens to sacrifice the internal goods of scientific inquiry when it is politically expedient. As we shall see in the next chapter, this worry arises repeatedly in discussions of the role of ethical values in scientific inquiry. In the meantime, we can finish the connection argument with this dilemma:

- (CA-11) If scientific inquiry is not properly organized then it is threatened by institutional domination.
- (CA-12) If the standards for scientific inquiry are possibly influenced by (what is believed to be) an institution, then scientific inquiry is threatened by institutional domination.
- (CA-13)  $\therefore$  If a scientist holds the narrow view of scientific inquiry then *ceteris paribus* she will believe any possible influence of any non-scientific practice on the standards for scientific inquiry to threaten institutional domination.

And this last is the connection hypothesis.

The connection hypothesis suggests that a widespread acceptance of the narrow view explains the widespread acceptance by scientists (including philosophers of science) of the ideal of value-free or autonomous science — science that is isolated from other practices. This argument will be made in the next chapter, but the move following the connection hypothesis should be clear: if interaction with any other practice threatens institutional domination, then scientific inquiry shouldn't interact with any other practice, or only do so in a very limited way. For now, I want to clarify the claim of the connection hypothesis and consider a few objections to the connection argument.

To be clear, the connection hypothesis does not claim that a commitment to the narrow view of scientific inquiry strictly implies a worry about the institutional domination; there are too many additional assumptions that must be brought into play for the implication to hold, and it is for exactly this reason that the *ceteris paribus* clause was introduced at step (CA-6). What the hypothesis — and the argument for it — explain is how, all else being equal, someone who holds the narrow view will be led to worry about the threat of institutional domination. To the extent that someone holds — at least implicitly — the narrow view of scientific inquiry and is subject to the additional assumptions identified above, we can expect them to also hold the ideal of value-freedom. Hence, to the extent that the narrow view is prevalent, the hypothesis predicts that the ideal of value-freedom will also be prevalent.

Alternatively, we could replace the *ceteris paribus* clause with a “generally and for the most part” clause: generally and for the most part, people who hold the narrow view also worry are led to worry about institutional domination.

Whichever alternative is adopted, I am not claiming that scientists who are not worried about institutional domination while also taking the narrow view of scientific inquiry are thereby inconsistent. In such cases, I would suggest that these scientists enjoy some understanding of the internal goods of non-scientific practices through some other route (that is, other than the influence of applied scientist-joint practitioners). Perhaps, for example, they themselves are participants of the other practice. Hence, for these scientists, a version of (CA-6) without the *ceteris paribus* clause is false: the work of applied scientists does *not* provide the only possible basis for a justification of some non-scientific practice. But this is not a problem for the overall argument. For, precisely because there is some other basis for a justification of the other practice, these cases are exceptional and not all else is equal. The “official” version of (CA-6), thanks to its *ceteris paribus* clause, still stands.

However, the mechanism presented by the connection argument does give us reason to believe that the position of such scientists is unstable. This is easiest to see if these scientists are attempting to be joint practitioners. Consider the anti-racist psychologist discussed above, who is trying to promote the internal goods of both psychology and the anti-racist movement. If she takes the narrow view of psychological inquiry, it will be quite difficult for her to understand just how her activities relate these two sets of internal goods. She will understand her anti-racist work as, at most, applied science, and as such not (legitimately) influencing her methodological critiques. But if, in addition, she takes these two kinds of critique to be one single kind of activity — a critical reaction to I.Q. research — the view articulated in the previous sentence will probably seem absurd. She is not moving back and forth between two roles, applied and pure scientist; rather, she is combining the two roles of psychologist and anti-racist activist. Such a scientist, I suggest, is not logically inconsistent to hold the narrow view; but her understanding of her activities will be much more coherent on the broad view. This may provide her with some non-deductive reason to abandon the narrow view. And, similarly, it may provide science scholars, studying the work of this scientist, reason to abandon the narrow view.

It may be objected that the relationship between pure and applied scientific inquiry is not as asymmetrical as required by the central moves of the connection argument. There is not a bright line dividing the work and contributions of pure scientists from that of applied scientists and engineers. As several historians of science have pointed out, much of the science of the last hundred years, after all, has been industrial and military science — pure research designed to produce commercial or military applications.<sup>42</sup> In obvious ways, this requires that the work of applied sci-

<sup>42</sup> See, for example, Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008), 97-9; Lillian Hoddeson and Michael Riordan, *Crystal Fire: The Invention of the Transistor and the Birth of the*

entists and engineers have some influence on the pure scientific research in question. And this influence, in turn, would seem to provide a basis for joint practice, partly external progress, and so on.

My response to this objection begins by pointing out that, even if there has not in fact been a sufficiently robust *actual* or *effective* asymmetry in the relationship between pure and applied scientific inquiry, there has been a robust *normative belief* in such an asymmetry — that is, a belief that such an asymmetry should be established or maintained. As historian of science Steven Shapin puts it, in the middle of the last century,

The goal of scientific inquiry was Truth; the goal of business was Profit. The natural agent of pure scientific inquiry was the free-acting individual; the natural agent of applied research and development was the organized team. The incompatibilities were treated as both important and evident [in the commercial research labs]. Organized research, whether in industry (where it had its natural home) or in government or university laboratories, was said to be a prostitution of the very idea of science and a visible index of how modernity was going disastrously wrong.<sup>43</sup>

There are two things worth noting here. First, there is the normative belief in the asymmetry between pure and applied research, exactly as the connection argument requires. Second, the asymmetry is normative because of the threat of institutional domination — the division into pure and applied scientific inquiry is needed to protect scientific inquiry from “prostitution.” Perhaps applied scientific inquiry *does in fact* influence pure scientific inquiry; still, normatively, it *should not* do so, because this may lead to institutional domination.

This second note suggests another potential objection. In Shapin’s narrative, the order of explanation runs from the threat of domination to the social structure of scientific inquiry — from worries about “prostitution” to the normative asymmetry between pure and applied scientists. In the connection argument, by contrast, the order of explanation runs the other direction, from the social structure to the threat of domination. This may be thought a problem for my view, but that is not the case at all: both explanations may capture important parts of the relation between the social structure of scientific inquiry and the threat of domination. Shapin’s narrative describes one direction of this causal connection. The connection argument describes the other.

### 3.4 The broad view

I turn now to the broad view of scientific inquiry as a practice. Note that my aim here is not to give a thorough argument for the adequacy of the broad view and the

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*Information Age* (New York and London: Norton, 1997), ISBN: 0393041247.

<sup>43</sup> Shapin, *The Scientific Life*, 96.



inadequacy of the narrow view. Instead, my aim is merely to lay out the broad view and make it seem at least plausible.

The broad view can be defined as the conjunction of two claims:

- (1) There are two primary goods of scientific inquiry, representation and technology, and one primary virtue, practical knowledge.
- (2) These three are the primary internal goods of scientific inquiry and, as such, are equally important.

The new terminology in these two claims will be introduced formally below. For now, recall from §3.3 that the narrow view takes representational knowledge to be the sole or primary good of science as practice, and takes the practice to be goods-oriented.<sup>44</sup> The broad view's defining claims are incompatible with both of the narrow view's claims. On the broad view, there is at least one other good of scientific inquiry besides representational knowledge — technology, as discussed in the next subsection — and these two goods are equally important, on the whole, as a certain family of virtues — practical knowledge, discussed in §3.4.2. After introducing these internal goods, in §3.4.3 I briefly discuss the appropriate conception of eudaimonia for the narrow view and, in §3.4.4, I present a second connection hypothesis. In general, throughout this section I speak from the perspective of the broad view.

### 3.4.1 Goods

In this subsection I discuss the good of technology, and argue for the plausibility of, first, the distinction between representations and technology, and second, the claim that the two goods are interdependent and equally important as internal goods.

The second distinct good of scientific inquiry is *technology*, by which I will mean artifacts that reliably produce certain end-states or products within a range of environments (possibly many, possibly only a few), typically given certain resources and when used by a properly-trained operator. In the broadest sense, technology includes everything from iPods and anti-retrovirals to inclined planes and tables (which reliably produce the end-state of my dinner not falling on the floor); however, just as there are more general and more specific senses of representation, there are more general and more specific senses of technology. Inclined planes and tables are not attached to contemporary scientific inquiry in the way that iPods and anti-retrovirals are. I assume that arguments similar to those in §3.3.1 are sufficient to make plausible the claim that technology, at the appropriate level of specificity, is attached to contemporary scientific inquiry in the way necessary for the former to be a good of

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<sup>44</sup> Recall the terminological distinction between internal goods and goods. Briefly, goods are those internal goods that are artifacts; other internal goods include virtues, which are properties or capacities of practitioners. For a goods-oriented view, goods enjoy a certain primacy over virtues.

the latter. Finally, note that technology is a good, and not a virtue, as an artifact can be transferred from one person to another.<sup>45</sup>

Efforts to create new technology are often attempts to solve specific problems — to produce an artifact that reliably produces better (less problematic) end-states, or one that reliably creates a presently scarce yet valuable product. For example, one might be attempting to create an instrument that can reliably produce data concerning some phenomenon — data that are extremely difficult or downright impossible to produce without this instrument. Or one might be attempting to create a machine that can reliably filter toxins from the blood of someone suffering kidney failure, perhaps in a less time-intensive way than current dialysis machines. Or one simply might be attempting to create a smaller, more durable, less expensive way to store digital information. Where the standards for representational knowledge are typically *epistemic* — given by normative epistemology — the standards for technology are typically *pragmatic* — given by the standards for a successful solution to the problem in question. As the first example in this paragraph shows, standards may be both epistemic and pragmatic: the instrument solves the problem by producing good representations. But the other two examples show that these kinds of standards can diverge: a better dialysis machine is not *epistemologically* better. Throughout this dissertation, I will frequently speak of *epistemic aims* and *pragmatic aims*. The former is the aim or goal of producing good representations and the latter is the aim or goal of producing technology that reliably solves a given problem.

The broad view claims that technology and representation are distinct yet equally important aims of scientific inquiry. The previous paragraph gives us a preliminary reason to think this is the case: pragmatic and epistemic standards can diverge quite sharply. To develop this argument, I draw on the work of Martin Carrier.

Carrier argues that “technological change derives from science but only in part.”<sup>46</sup> By “science,” he means something like what I have called “pure scientific inquiry,” and so I will adopt this terminology for this discussion; note that part of Carrier’s point is to problematize the narrow view’s conception of the relationship between pure and applied scientific inquiry, that is, eudaimonia as applied science. Carrier argues that the representational knowledge produced by pure scientific inquiry is often too general and too abstract to be directly applicable to particular cases — including the particular cases in which someone is trying to create a piece of technology that can solve a certain problem. In particular, abstract and general “theories” must be

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<sup>45</sup> Don Howard poses the question of what technologies are produced by the social sciences. One possible response would be new social organizations, especially new governmental and economic institutions, such as central reserve banks or social welfare bureaucracies. However, it’s not clear that such things would qualify as goods rather than virtues.

<sup>46</sup> Martin Carrier, *Theories for Use: On the Bearing of Basic Science on Practical Problems*, paper presented at the first conference of the European Philosophy of Science Association. Available at <http://philsci-archive.pitt.edu/archive/00003690/>. Madrid, Spain, 2007, 1.

modified and supplemented by lower-level “observational generalizations, experience-based regularities, ad-hoc assumptions, or rules of thumb.”<sup>47</sup> He gives the example of giant magnetoresistance, a phenomenon that is the basis for hard disks and magnetic tapes. According to Carrier, “the qualitative explanation of the effect was suggested immediately after its discovery” by a straightforward application of quantum theory. However, this representational knowledge by itself is insufficient to create new technology: “if a read head is supposed to be constructed, the influence of layer thickness, material properties, temperature variations and changes of the magnetic field need to be known exactly.”<sup>48</sup> This requires the development of what Carrier calls a “local model,” which combines the abstract and general features of one or several “theories” with the details and ad hoc adjustments relevant to the particular piece of technology in question.

A local model, of course, is another representation; thus far we have no reason to think that developing useful technology is a very different aim from developing good representations. Carrier’s most significant point here is that *the standards for good local models are not, by and large, the standards of normative epistemology.*

Phenomenological models are shaped conceptually by the demands of the problem-situation at hand. They are not necessarily completely independent of theory, but they contain comparatively few elements that transcend the particulars of the explanatory challenge to be dealt with . . . . For instance, the prediction of the tidal flow of a particular harbor is not based on the known causal mechanism underlying the phenomenon but is rather achieved by performing a Fourier analysis of the tidal oscillations observed in the past. The reason is that the influence of a multiplicity of factors relevant for the quantitative details of tidal flow (such as coastline, water depth, currents) can hardly be assessed on first principles so that the phenomenological analysis is more accurate. The drawback is that results gained by this method cannot be transferred to different coastal areas; the latter need to be addressed completely afresh.<sup>49</sup>

In particular, the kind of representational knowledge sought in applied science is subordinated to pragmatic aims — solving the problem at hand. Epistemic aims and standards — such as empirical adequacy — are valued only because they help solve the problem; meanwhile, epistemic aims and standards — such as generality and explanatory power — that conflict with pragmatic aims — say, by being a waste of valuable resources, relative to solving the problem at hand — are considered less important, and can even be sacrificed completely. This observation is only coherent if, at least in these cases where standards for epistemic and pragmatic success diverge, epistemic and pragmatic aims are distinct. Epistemic standards cannot be

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<sup>47</sup> [Ibid.](#), 5.

<sup>48</sup> [Ibid.](#), 6.

<sup>49</sup> [Ibid.](#), 7.

subordinated to pragmatic standards if they are the same thing, and the two sets of standards are distinct only if the two kinds of aims are distinct.

To be clear, neither Carrier nor I claim that the two aims and sets of standards never go together; thus neither of us claim that pure scientific inquiry (or, better, the pursuit of representational knowledge) and applied scientific inquiry (or, better, the pursuit of technology) are completely different practices. In particular, it is no objection to point to cases in which generality and explanatory power are conducive to the achievement of pragmatic aims, or vice versa; the claim is not that in *all* cases epistemic standards diverge from pragmatic standards. The point is merely that insofar as there are cases in which representations are good by epistemic standards but inadequate by pragmatic standards, producing representations is not sufficient for producing technology. Hence in these cases these are two distinct goods of the single practice of scientific inquiry.

While technology and representational knowledge are distinct goods, they are still closely related; this is what Carrier means when he says that “technological change derives from science.” In general, the production of new representational knowledge enables the production of new technology. New representational knowledge identifies previously unknown connections (causal or merely correlative) between phenomena — identifies regularities of cause and effect — that can be utilized in a piece of technology — bringing about the cause is, under the right conditions, a reliable way to bring about the effect. Carrier has argued elsewhere that the understanding of these connections provided by explanatorily powerful representations are important for producing genuinely reliable technology: without understanding why the device operates in the way that it does, we cannot anticipate under what range of circumstances it will operate as desired.<sup>50</sup> Going in the other direction, as some of the examples above have indicated, scientific instruments are themselves technology. The most obvious examples here are imaging technologies — microscopes, photography, MRIs — and technologies that generate quantitative data — thermometers, hygrometers, seismographs. But model organisms are also forms of technology, in my sense.<sup>51</sup> Further, even new technology that is not a scientific instrument can enable the production of new representational knowledge — consider the impact of computers, or the radio, rocket, and robotics technologies that we use to explore the solar system.

We can make the thought of the last paragraph more precise using the language of mechanisms.<sup>52</sup> Think of scientific inquiry as a mechanism for producing representations: as a set of scientists and their experimental apparatus and instruments, organized (spatially, socially, and so on) in such a way that their activities more-or-

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<sup>50</sup> Martin Carrier, “Science in the Grip of the Economy: On the Epistemic Impact of the Commercialization of Research,” in *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, ed. Martin Carrier, Don Howard, and Janet Kourany (Pittsburgh: University of Pittsburgh Press, 2008), 225.

<sup>51</sup> Rheinberger, *An Epistemology of the Concrete*, part II.

<sup>52</sup> Machamer, Darden, and Craver, “Thinking about Mechanisms.”

less reliably produce new representations (among other things). The experimental apparatus and instruments, of course, are technology. Without this technology, the mechanism either cannot function at all — scientists simply cannot produce new representations — or functions poorly — less reliably, more slowly, less efficiently, and so on — or requires radical reorganization to function properly — replacing recording instruments with lab assistants, for example. So technology has a crucial — though perhaps less than strictly necessary — function in the representation-producing mechanism. In this sense, representations depend on technology.

Now think of scientific inquiry as a mechanism for realizing the pragmatic aim of producing new technology. Without representations — global, local, or both — that can provide at least modestly accurate predictions and some degree of understanding, the mechanism will not be able to produce, reliably, reliable technology. We may simply be completely unaware that such an effect could be produced at all — imagine trying to build a reliable electromagnet in the twelfth century. Without suitable representational knowledge, such potential technology is inconceivable or absurd. In this case, the mechanism simply cannot function at all. Or, we may be aware that an effect can be produced, but only under very specific conditions. Lacking an understanding of the causes of the effect, the only way to develop new technology is through sheer experimental guesswork and more than a little luck.<sup>53</sup> In this case, the mechanism for technological development functions poorly. So representations have a crucial — though, again, perhaps less than strictly necessary — function in the technology-producing mechanism. In exactly the same sense as the last paragraph, technology depends on representations.

Davis Baird’s account of technology may seem to disagree with both the narrow and broad view.<sup>54</sup> Baird’s “instrument epistemology” explicitly rejects the “text bias” that, in my terminology, takes the production of representations to be the unique aim of scientific inquiry.<sup>55</sup> Thus, he explicitly rejects the narrow view. But, on the other hand, he seems to claim that technology does not depend on representation. His discussion of Thomas Davenport, a Vermont blacksmith who developed one of the first electric motors in 1835, is representative:

The motor works by switching the polarity of four electromagnets in synchronicity with the motion of the wheel so that the wheel is always drawn forward. All of this was accomplished despite the fact that *Davenport did not know electromagnetic theory*. When he first saw [physicist

<sup>53</sup> We might call this “tinkering,” but that term has pejorative, classist connotations; see my discussion of Thomas Davenport in the following paragraphs. The line of thought in the current paragraph is largely inspired by Nancy Cartwright, “How to Do Things with Causes,” *Proceedings and Addresses of The American Philosophical Association* 83, no. 2 (November 2009): 5–22.

<sup>54</sup> Davis Baird, *Thing Knowledge: A Philosophy of Scientific Instruments* (Berkeley and Los Angeles: University of California Press, 2004), ISBN: 0520232496.

<sup>55</sup> *Ibid.*, 6ff.

Joseph] Henry’s electromagnet, *he had never heard of any of the main contributors to the science of electromagnetism*. But he did have an appreciation for the phenomenon exhibited by the electromagnet, and he was able to use this knowledge — *presented by the device itself* — to make other devices. Davenport was interested in developing devices that would have practical utility, and he did succeed in using his motor to drive a printing press.<sup>56</sup>

Davenport, it seems, had no problem creating a reliable electric motor without the use of representational knowledge.

There are several ways of reconciling Baird’s account with the broad view as I have presented it so far. First, Davenport’s case may be exceptional — the untrained blacksmith simply got lucky. But Davenport’s case is not exceptional: until roughly the middle of the last century, technological innovations were much more likely to be produced by blacksmiths, midwives, mechanics, and housewives than highly-educated intellectuals.<sup>57</sup> So, second, I hypothesize that, in cases such as Davenport’s, representational knowledge is and was crucial — just not the abstract representational knowledge of highly-trained physicists. Davenport, I suspect, did not just build various electromagnetic devices;<sup>58</sup> rather, he probably also kept notes, tried to think through and understand the behavior of these devices, used this understanding to make predictions about the behavior of future devices, and in turn tested this understanding and these predictions against the actual behavior of these devices. All of this involves the creation and improvement of representations. Almost certainly, these representations were not as abstract or sophisticated as those of early nineteenth century professional physicists; but (assuming the overall hypothesis is accurate) they are representations, and they were crucial to Davenport’s work.

However, it is possible that Davenport kept only a few notes, and that almost all of his understanding of the operation of his motor was tacit knowledge or “working knowledge.”<sup>59</sup> More specifically, this would be knowledge of how to arrange the parts of the motor so as to realize a certain effect (say, the switching of the polarity of the magnets, or later the synchronization of this switching with the turning of the wheel) or avoid a certain problem (say, excess friction from the axle), without the use of representations. In this case, I suggest, Davenport substituted practical knowledge — knowing *how* — for representational knowledge — knowing *that*. However, as discussed in the next subsection, practical knowledge is also a primary internal good

<sup>56</sup> Baird, *Thing Knowledge*, 10, my emphasis and brackets.

<sup>57</sup> While frequently too polemical to be taken without a large grain of salt, one highly intriguing and entertaining discussion is Clifford Conner, *A People’s History of Science* (New York: Nation Books, 2005), ISBN: 1560257482.

<sup>58</sup> I do not have easy access to the biography of Davenport that Baird cites, so I cannot claim to have any evidence to support this or any of the other responses that I propose. I suggest them only as hypotheses.

<sup>59</sup> Cf. Baird, *Thing Knowledge*, 12.



of scientific inquiry on the broad view. While this may require a small modification to the claims of this section — in some cases, representational knowledge can be replaced with practical knowledge in the overall mechanism for technological development — this modification is entirely compatible with the overall claim that each of the three primary internal goods of scientific inquiry — technology, representational knowledge, and practical knowledge — is crucial for the production of the other two.

In short, so long as one of the three possibilities given above accurately describes Davenport’s case, the best interpretation of Baird’s account of technology seems to be a broad view interpretation.

To conclude this subsection, return to the (simplified) claim that each of representation and technology is crucial — albeit not strictly necessary — for the reliable operation of the mechanism that produces the other. In this sense, each is dependent on the other. Or, more succinctly, representation and technology are *interdependent*. So, each can be justified as means to the production of the other; but, at the same time, each is justified on its own as one of the primary internal goods of science as a practice. This line of thought, I suggest, gives reason to consider them, in general or on the whole, to be equally valuable. They need not always be equally important in every research program; developing a particular new instrument may legitimately be merely a means to the end of producing a representation, or vice versa. But, when considering the practice of scientific inquiry as a whole and within the context of our whole society, we should regard both goods as equally important. Neither should be taken to always be less important or subsidiary to the other. Or, in the language of mechanisms once again, it is misleading to think that we have two kinds of mechanism, one for representations and one for technology. Rather, we have one kind of mechanism, scientific inquiry, for the development of *both* representations and technology.

### 3.4.2 Virtues

In §3.2.2, I argued that the virtues of scientific inquiry are, in the first instance, those excellences of (the behavior of) scientists that are conducive to the production of representational knowledge. I went on to problematize this initial definition, showing that there are certain virtues of scientists, viz., Merton’s ethos of science, that do not directly conduce to the production of representational knowledge. However, Merton does claim that these four virtues are considered valuable because they *indirectly* conduce to the production of representational knowledge. As in §3.3.2, this is consistent with the fact that the narrow view is a goods-oriented view of scientific inquiry as practice, with the sole good being representational knowledge: the whole practice is organized around the production of representational knowledge, even if every feature of the practice is not valued directly for its contribution to the production of this good.

By contrast and as I explained in the last subsection, the broad view does not

take representational knowledge to be the sole good of scientific inquiry. In this subsection I show how the broad view is not a goods-oriented view either. There is, on the broad view, at least one virtue that is just as important for science as practice as representational knowledge and technology. I call this virtue *practical knowledge*, defined as the ability or disposition to behave so as to produce reliably a certain outcome given appropriate resources (perhaps including both materials and technology) and environment or context. For example, the practical knowledge of baking a loaf of bread is the ability to produce reliably a loaf of bread — produce a loaf of bread on several different occasions, in several different kitchens, and so on — given the ingredients, a mixing bowl, an oven, and so on. Similarly, the practical knowledge of a dance routine is the ability to perform the routine on several different occasions and in several different venues, given suitable shoes, non-restrictive clothing, and an appropriate stage surface.

From the discussions of Carrier and Baird we can produce several examples of practical knowledge in scientific inquiry: the use of instruments to produce data, the correct conduct of field observations, the proper design and implementation of an experiment itself. To be clear, a dataset itself is a piece of representational knowledge, and the instrument used in gathering the data is a piece of technology, but the ability to (reliably) use the instrument produce the dataset is practical knowledge. Other important examples include the ability of doctors and nurses to diagnose and treat injury and illness, the ability of researchers to give and respond to objections to their methods and arguments, and the ability of engineers to anticipate weaknesses in the design of a proposed piece of technology.

Through all these examples, note that the standards for practical knowledge are, like the standards for technology, pragmatic. One is said to have practical knowledge when one is able to produce reliably a certain end-state or product, that is, to solve some specific problem or problems. In some cases, pragmatic standards will converge with epistemic standards, as in the practical knowledge of the use of an instrument to produce data. But in others, such as the practical knowledge of a skilled surgeon, the relevant pragmatic standards will not be epistemic.

It is important for the broad view that there be a distinction between representational and practical knowledge; if the ability to achieve some outcome or behave in a certain way is nothing more than possessing a certain representation, then this “virtue” is really just the good of representational knowledge. Something sufficiently close to the distinction between representational and practical knowledge seems to be widely accepted among philosophers, at least in the wake of Gilbert Ryle’s influential “Knowing How and Knowing That.”<sup>60</sup>

Jason Stanley and Timothy Williamson seem to reject this distinction. On their view, practical knowledge is a matter of knowing a relevant bit of propositional knowl-

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<sup>60</sup> Gilbert Ryle, “Knowing How and Knowing that,” *Proceedings of the Aristotelian Society, New Series* 46 (1945-1946): 1–16.



edge “under a practical mode of presentation.”<sup>61</sup> So this mode of representational knowledge is both necessary and sufficient for practical knowledge. However, Stanley and Williamson do not appear to explain the cryptic, quoted phrase, nor why we should take the difference to be a matter of mode rather than the deeper difference between knowing how and knowing that. Furthermore, at least two critics have argued that this practical mode involves the ability, skill, disposition, or capacity that Ryle had in mind in the first place and so practical knowledge cannot be reduced to a propositional attitude or representational knowledge.<sup>62</sup> Hence, even if Stanley and Williamson are right when they claim that representational knowledge is necessary for practical knowledge, they are wrong when they claim that representational knowledge is sufficient for practical knowledge.

Another challenge to the distinction between representational and practical knowledge runs in the other direction: representational knowledge, on this pragmatist view, is nothing but a kind, mode, or part of practical knowledge.<sup>63</sup> While I am much more sympathetic to this proposal than Stanley and Williamson’s, I believe that it involves an equivocation when stated this simply. It may be true that, generally and for the most part, we (do or should) value representational knowledge for the actions that it enable us to carry out, that is, for the practical knowledge that it supports. As I will argue below myself, representational knowledge is crucial for the production of practical knowledge. In addition, I take it to be clear that representational knowledge can only function as such when it is situated in a social context that includes people who know how to use it as a representation. For example, a map can only function as a map when it is in the hands of people who understand the mapmaker’s conventions and know how to use the map to navigate.<sup>64</sup> Similarly, a system of tensor equations can only be used as a representation of the structure of spacetime when there are physicists around who know how, say, to use these equations to calculate the structure of the stress-energy tensor and thus make predictions about the trajectory of a

<sup>61</sup> Jason Stanley and Timothy Williamson, “Knowing How,” *Journal of Philosophy* 98, no. 8 (August 2001): 429ff.

<sup>62</sup> Tobias Rosefeldt, “Is Knowing-How Simply a Case of Knowing-that?” *Philosophical Investigations* 27, no. 4 (October 2004): 370–9; Jeremy Fantl, “Knowing-How and Knowing-That,” *Philosophy Compass* 3, no. 3 (2008): 451–70.

<sup>63</sup> See, for example Otto Neurath, “The Lost Wanderers of Descartes and the Auxiliary Motive,” in *Philosophical Papers 1913-1946*, ed. Robert Cohen and Marie Neurath (1913; Dordrecht: D. Reidel, 1983); Nancy Cartwright, “Well-Ordered Science: Evidence for Use,” *Philosophy of Science* 73 (2006); Don Howard, “Lost Wanderers in the Forest of Knowledge: Some Thoughts on the Discovery-Justification Distinction,” in *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*, ed. Jutta Schickore and Friedrich Steinle (Dordrecht: Springer Verlag, 2006), 3–22, ISBN: 1402042507; Matthew Brown, “Science and Experience: A Deweyan Pragmatist Philosophy of Science” (PhD diss., University of California at San Diego, 2009), and §5.6.

<sup>64</sup> Cf. Kitcher, *Science, Truth, and Democracy*, ch. 5.

beam of light. That is, in general, I am willing to concede that practical knowledge is crucial for representational knowledge. But none of this implies that representational knowledge *just is* practical knowledge. The metaphysical claim does not follow.

We can also understand the problems of the views described in the last two paragraphs in terms of the taxonomy of internal goods. Representational knowledge, I take it, is a good — an artifact, transferable from individual to individual. For example, I can give you my copy of a textbook on relativity theory, and thereby give you the representations that it comprises. Or, if my computer contains (in the appropriate sense for electronic media) a public-license electronic textbook on relativity theory, I can copy this book on to a flash drive (thereby creating a second copy) and then copy it on to your hard drive (thereby creating a third copy), transferring it to you. Practical knowledge, by contrast, is a virtue — and not transferable from individual to individual. I can, for example, teach you how to solve the equations of relativity theory, thereby cultivating or instilling a certain ability in you, but I cannot transfer my ability, as such, to you. Practical knowledge and representational knowledge are different kinds of things, and so, however closely they might be related, they cannot be the same thing.

While representational knowledge is distinct from practical knowledge, these two internal goods are interdependent in the same way as representational knowledge and technology. After all, the instruments utilized to produce, gather, and analyze data can only function properly when used and maintained by practitioners with the relevant practical knowledge. Practical knowledge is also required in those areas of scientific inquiry that use only minimal technology, such as field observations and sample-collecting. Live field observations of large mammals, for example, require the practical knowledge of how to track such animals, identify areas where they frequent and have been recently, adequately disguise oneself, and so on. And I suggest that various abilities to manipulate and analyze representations and use these to produce new representations are also practical knowledge. Statistical hypothesis testing, for example, requires knowledge of *how* to formulate an appropriate hypothesis, arrange one's data sets, identify the relevant probability distribution, conduct the corresponding numerical test, and respond to the result of the test. Without the relevant practical knowledge, in all four cases, the mechanism for producing representational knowledge either functions much more poorly or does not function at all. Hence, practical knowledge is crucial for producing representations.

It may be objected that, in many areas of contemporary scientific inquiry, there is a division of labor such that scientists — say, molecular biologists — need not have any but the merest knowledge of how to use some of their instrumentation — say, scanning electron microscopes. More specifically, they are not able to repair it should it stop working, or understand why and how it works; the instrument itself operates as a “black box” in this mechanism for producing representations.<sup>65</sup>

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<sup>65</sup> One could produce a similar example using statistical hypothesis testing and Excel. This objection is developed in part from comments by Don Howard and Baird, *Thing*

I take it that this account assumes that someone is able to repair the instrument should it stop working, and that this person (or someone with similar practical knowledge) first taught the scientists in question how to use the instrument. Molecular biologists did not, as it were, discover scanning electron microscopes in a long-abandoned physics laboratory, only discovering accidentally that it could be used to study, say, the structure of viruses. Rather, some joint practitioner in physics and biology realized that scanning electron microscopes could be utilized in this way, and some physics (perhaps under the job title of “technicians”) are available to keep the microscopes in good working order. As with the “non-mental” conception of representational knowledge, practical knowledge can be “distributed” unevenly throughout the community of practitioners. With an excessively specific conception of the community of practitioners — say, one that excludes the “technicians” — practical knowledge may even seem to disappear entirely. But, with a less specific conception of the community of practitioners — one that includes the “technicians” — practical knowledge is clearly seen to be both present within the practice and crucial for its success.

In addition, without at least some relevant practical knowledge, scientific inquiry is liable to produce bad (inaccurate, deceptive, unrepresentative) representations. This is easiest to see with the example of statistical hypothesis testing, mentioned in the footnote two paragraphs ago. For any reasonably large population of human beings, assembling an adequate sample is rather difficult and must be done with some care. Thus, a social scientist who lacks sufficient practical knowledge in recruiting subjects for a survey or laboratory experiment will probably assemble an utterly unrepresentative sample, and consequently produce a representation that both passes the relevant statistical tests and is epistemically worthless.

So practical knowledge is crucial for the production of representations. And, of course, representations are crucial for the production of practical knowledge, for basically the same reasons that representations were seen to be crucial for the production of technology in the last subsection: representations support the understanding that in turn supports reliably effective action. The practical knowledge of contemporary medicine, for example, would be impossible without the representational knowledge of modern biology. So, all together, practical knowledge and representational knowledge are distinct yet interdependent internal goods.

Again, as with representations and technology, it is important that there is a distinction between practical knowledge and technology. One might challenge this distinction by first repeating the observation above, that any technology requires someone to operate it. Without this operator — if the relevant practical knowledge were lost — the technology would be inert and useless, or at least function erratically and unreliably. Further, the technology must be periodically maintained and repaired, and this must be done by someone with the relevant practical knowledge. Hence, practical knowledge is necessary for technology. Next, both practical knowledge and technology are defined in terms of reliably producing outcomes; the objector

may reason that this means practical knowledge is sufficient for technology as well. Watching a surgeon removing a tumor, we may say that she is exercising her practical knowledge; or, turning our attention to the array of equipment and instruments found in the operating room, we may equally say that she is using technology. Technology and practical knowledge, so the objector concludes, are one and the same thing; the choice of terminology is merely a matter of emphasis.

While the interdependence of practical knowledge and technology is quite close, the objection neglects the distinction between goods and virtues. Technology — especially mass-produced technology — is not usually tied to one particular operator, and can be used by different people, sometimes even to address very different problems. Most of the equipment and instruments in the operating room can be used by any of the surgeons in the hospital, and indeed by any competent surgeon. The surgeon does not take the heart-rate monitor with her when she leaves. But she does take her practical knowledge with her. Her ability to use skillfully her scalpel to cut out the tumor is, at that level of description, similar to the ability of other surgeons, but it is still not something that can be transferred from her to someone else. Similarly, if she trains another surgeon, she cultivates similar abilities in him, but does not transfer her abilities to him in anything like the way she might give him her scalpel. While the two are generally necessary and sufficient for each other in the mechanistic sense — that is, a mechanism for producing or that utilizes one generally also involves the other in a crucial way — they occupy different places in the taxonomy of internal goods and are distinct.

What the objection does give us is reason to consider practical knowledge and technology to be equally important. We cannot be said to have a piece of technology without someone with the practical knowledge to operate it; and we cannot be said to have practical knowledge (in any but a handful of cases or in the most attenuated sense) without access to the technology required to exercise the ability. Hence we cannot pursue one without pursuing the other. When trying to develop new technology, it is important to keep in mind the need to train and support its operators; and when cultivating new practical knowledge, it is important to keep in mind the need for access to the relevant technology.

This interdependence implies that the broad view is neither goods-oriented nor virtue-oriented; in general, both certain goods and certain virtues are important aims for scientific inquiry.

### 3.4.3 Eudaimonia

In §3.3.4, I articulated a conception of eudaimonia as applied science for the narrow view, and went on in §3.3.5 to show how this feature of the narrow view leads to worries about institutional domination. The connection argument of this section assumed that applied scientists were the only joint practitioners within scientific inquiry and that the asymmetry between pure and applied scientists means the work of the latter

does not influence the standards for pure scientific inquiry.

The broad view leads to an importantly different conception of eudaimonia. According to this conception, as with the narrow view, there is an internal good that a scientist realizes by transferring her work as a scientist to other areas of her life. However, in most other respects this conception of eudaimonia is quite different from the conception of applied science. In particular, it rejects both of the assumptions identified in the final sentence of the last paragraph. As we shall see over the course of the next few chapters, these differences lead to very different positions on the proper role of ethical and political values in scientific inquiry.

As I have discussed at length already, the broad view conceives of several distinct yet interdependent and equally important primary internal goods for scientific inquiry. Since these goods are equally important, the broad view is incompatible with the asymmetry between epistemic and pragmatic aims — and thus the asymmetry between pure and applied scientists — that the narrow view embraces. This is not to deny any possible distinction between pure and applied science; as we shall see in chapter 6, the broad view is consistent with some versions of this distinction. It is also not to deny that some individual scientists, labs, even broad-scale projects and research programs may emphasize one of these internal goods — say, producing a new kind of biomedical imaging technology — and more-or-less ignore or only incidentally develop the other two. The work of such scientists will depend on the achievements of other scientists, labs, and so on, pursuing the other two internal goods — say, the practical knowledge of established methods of diagnosis and representational knowledge concerning the features of patients to which new technology should be sensitive — or will be utilized by still other scientists, and so on, in the pursuit of the other internal goods — say, new representational knowledge concerning the progression of certain diseases. To switch metaphors: the development of one internal goods depends on the upstream development of the other two, and contributes in turn to their development downstream. Representational knowledge is not a reservoir sitting above the stream of technology and practical knowledge (this is the narrow view's picture); rather, on the broad view, the three are continuous currents that flow into, through, and out of each other over the course of the entire river.

Hence, on the broad view, the work of applied scientists is not the application of previously-produced pure representational knowledge to other, “external” practices. Applied scientists and engineers are not marginal scientists; they are as central to the practice of scientific inquiry as pure scientists. However, this means that the work of applied scientists and engineers cannot, as such, count as joint practice on the broad view. Their work is entirely “inside” the practice, not “external” to it or on the “boundary.” Consequently, on the broad view, eudaimonia cannot be thought of as applied science.

Instead, a conception of eudaimonia for the broad view should be something like the following: *the development of representational knowledge, technology, and practical knowledge that simultaneously promotes the internal goods of some other practice.*

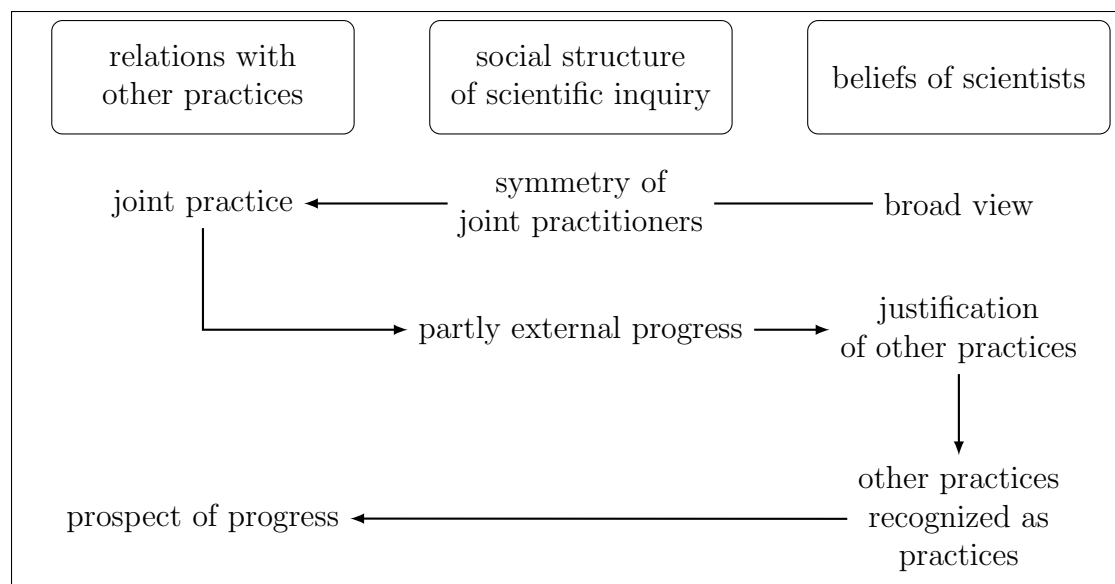


Figure 3.3: The Inverse Connection Argument

Call this *eudaimonia as joint practice*.

### 3.4.4 The inverse connection argument

Just as *eudaimonia as applied science* provided the basis for an argument that the narrow view leads to worries about the threat of institutional domination — the connection argument — the broad view’s conception of *eudaimonia as joint practice* provides the basis for an argument that the broad view leads to optimism about the prospects for partly external progress stimulated by joint practice. I call this the *inverse connection argument*.

The overall structure of the inverse connection argument is quite similar to that of the connection argument, though certain moves in the argument can be motivated (for many contemporary audiences) only after presenting a number of case studies and examples. So I will merely sketch the inverse connection argument here. On the narrow view, the asymmetry of pure and applied science prevents joint practice and so produces the belief that other practices are in fact institutions. On the broad view, pure and applied science are taken to be symmetrical and (thus) mutually influencing, and so joint practice is recognized as leading to partly external progress with respect to all three primary internal goods. This both provides a basis for the justification of other practices as practices and inductively supports the claim that future joint practice will also produce partly external progress. See figure 3.3.

The mechanism identified in the inverse connection argument is probably most plausible with respect to the pragmatic standards for technology and practical knowledge. These standards reflect certain pragmatic aims — the effectiveness of the piece

of technology or practical knowledge to solve a given problem. There is nothing in that last phrase that precludes reference to the internal goods of other practices in formulating these standards. That is, there is nothing that requires the problems that new technology and practical knowledge address be problems with producing new representational knowledge. Rather (or in addition), they can be the “external” problems of other practices.

Consider the work of several organizations of stove designers, attempting to create a new cookstove that satisfies the provisions laid out in the Waxman-Markey climate bill that was passed by the U.S. House in June 2009.<sup>66</sup> The open-pit cooking technologies that are used by approximately half the humans on the planet are inefficient (leading to deforestation), highly toxic, and release immense quantities of carbon dioxide and soot; one provision in Waxman-Markey calls for the development of new stove technology to address these problems. The organizations working on building new stoves comprise individuals who describe themselves as, for example, physicists, materials scientists, and engineers. To be clear, they are not, in the first instance, trying to produce new representations of combustion and stove design; they are, instead, trying to produce new technology. According to the head of one such organization, the standards for a good stove can be summarized in six criteria: a good stove

- reduces fuel use by more than fifty per cent,
- reduces soot by more than sixty percent,
- reduces childhood pneumonia by more than thirty percent,
- costs at most \$10 retail,
- is usable by cooks in poor, rural communities, and
- gets funded by governments, NGOs, and other sources of development funds.

None of these six criteria can be understood as “purely scientific”; all refer to or suggest the internal goods other practices, including those of environmental activism, health, human development, and of course food preparation. Hence the standards for a good stove — the standards for the practice of stove design — reflect partly external progress within scientific inquiry, and the work of the scientists in these organizations is an example of eudaimonia as joint practice. The transfer between practices runs both directions in the work of stove designers.

This account of partly external progress for pragmatic standards and their associated internal goods can be extended to include a simple account of partly external progress for the standards of representational knowledge as well. The interdependence

<sup>66</sup> Burkhard Bilger, “Hearth Surgery,” *The New Yorker*, 21st–28th December 2009, 84–97.



of the three primary internal goods suggests that, at least sometimes, representational knowledge will be pursued for the sake of some pragmatic aim and not (only) for the sake of some purely epistemic aim. For example, in clinical medical research, representational knowledge concerning the etiology of a disease and its response to certain sorts of interventions is pursued for the sake of producing new treatments. In these cases, the standards for excellent representational knowledge will include some criteria of usefulness.

In several contexts, Nancy Cartwright has argued along these lines to criticize the high status given to (representational knowledge that is produced by) randomized controlled trials (RCTs) in clinical medical research.<sup>67</sup> Cartwright begins by distinguishing “two broad categories” of “methods for warranting causal claims”:

- (1) “Those that *clinch* the conclusion but are *narrow* in their range of application,” and
- (2) “Those that merely *vouch for* the conclusion but are *broad* in their range of application.”<sup>68</sup>

Clinchers, she says, are deductive: “it can be proved that if the auxiliary assumptions are true, the methods are applied correctly and the outcomes are true and have the right form, then the hypothesis must be true.”<sup>69</sup> However, “the assumptions necessary for their successful application (a) tend to be extremely restrictive, (b) can only take a very specialized type of evidence as input, and (c) have only special forms of conclusion as output.”<sup>70</sup> Simple Popperian falsification, for example, is a clincher: if the hypothesis entails the predicted outcome and the predicted outcome doesn’t come out, then the hypothesis is false. Inductive generalizations, by contrast, are vouchers, and only provide fallible or probabilistic support for their conclusions.<sup>71</sup>

Next, Cartwright points out that, if *all* of the other causes (those not manipulated in the test) of some outcome  $O$  are distributed *identically* between the two groups (test and control) of a RCT, and we assume what she calls the probabilistic theory of causality,<sup>72</sup> then an ideal RCT will also be a clincher: we can prove, deductively, that the manipulated variable causes  $O$ . As Cartwright puts it, “So, what is established in the ideal RCT . . . is that  $T$  causes  $O$  in at least one maximally causally homogeneous subpopulation of  $\varphi$ .”<sup>73</sup> This is a large part of the appeal of RCTs: they have the “vanity of rigor.”<sup>74</sup>

<sup>67</sup> See, for example, Nancy Cartwright, “Are RCTs the Gold Standard?” *BioSocieties* 2 (2007): 11–20, doi:[10.1017/S1745855207005029](https://doi.org/10.1017/S1745855207005029).

<sup>68</sup> *Ibid.*, 12, her emphasis in this and all other quotations.

<sup>69</sup> *Ibid.*, 14.

<sup>70</sup> *Ibid.*, 12–13.

<sup>71</sup> *Ibid.*, 13.

<sup>72</sup>  $T$  causes  $O$  in population  $\varphi$  if  $Pr(O|T\&K) > Pr(O|\neg T\&K)$  for some subpopulation  $K$  of  $\varphi$  with  $Pr(K) > 0$ .

<sup>73</sup> Cartwright, “Are RCTs the Gold Standard?” 16.

<sup>74</sup> *Ibid.*, 18.



But the sort of argument used in RCTs, for all its deductive validity, tells us absolute nothing about what happens when the other causes of  $O$  are not distributed identically:

Just imagine a case where there are only two relevant subpopulations, in one of which  $T$  is strongly positive for  $O$  and in the other it is equally strongly negative. The results will be positive in the RCT if the first subpopulation is more probable than the second, but will be reversed in targets where the second outweighs the first.<sup>75</sup>

A RCT is “internally valid”: it establishes its conclusions with certainty. But it thereby sacrifices “external validity”: its conclusions cannot be reliably extended beyond the test population without (highly dubious) further auxiliary assumptions.

Hence, she concludes, RCTs do not have much to contribute to the pragmatic aims of clinical medicine: “The RCT, with its vaunted rigor, takes us only a very small part of the way we need to go for practical knowledge.”<sup>76</sup> But note two things about Cartwright’s conclusion. First, she has identified a failure of the representations produced by RCTs as representations. And second, this failure is a failure to contribute to pragmatic aims. Her argument assumes that the standards of excellent representations are, at least in the context of clinical medical research, influenced by the pragmatic aim of developing reliable medical interventions. If this were not the case — if the standards for representations were independent of the pragmatic aim — then it would be no criticism of the representations to say that they are pragmatically useless.

Finally, granted that (1) the standards for excellent representations can be influenced by the standards for technology and practical knowledge and that (2) the latter can enjoy partly external progress thanks to the work of joint practitioners, it follows that (3) the standards for excellent representations can enjoy partly external progress, although possibly only indirectly or at some remove. Cartwright’s own arguments for lowering the status of RCTs, for example, might qualify as an attempt to stimulate partly external progress for the standards of representational knowledge in clinical medical research.

### 3.5 An appendix on ends

For some readers, it may be perspicuous to understand the two views of scientific inquiry in terms of final ends. In this appendix to the chapter, I briefly introduce that terminology and provide the relevant characterization.

First we shall need the concept of a final end. Here I mostly follow Henry Richardson’s account, which is based on the following “counterfactual test”:

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<sup>75</sup> [Ibid.](#), 17-8.

<sup>76</sup> [Ibid.](#), 18.

End  $x$  is sought [equivalently, pursued] for the sake of end  $y$  only if  $x$  would still be sought even if the only value to be realized in so doing would be (that described by)  $y$  — setting aside even the advantages of  $x$  itself insofar as they are not conceptually carried by  $y$ .<sup>77</sup>

Then, a *final end* (for an agent  $S$ ) is an end  $x$  such that  $x$  satisfies the counterfactual test when  $y = x$ . Note that, on this definition, it is consistent to claim that both  $x$  is a final end and  $x$  is pursued for the sake of some other end,  $y$ .

The narrow view can be characterized as the conjunction of the following two claims:

- For any internal good of scientific inquiry  $x$ ,  $x$  is pursued for the sake of representational knowledge.
- Representational knowledge is the unique final end of scientific inquiry.

The first claim implies (by instantiating  $x$  as representational knowledge) that representational knowledge is a final end. The second claim implies no other internal good of scientific inquiry is a final end. Then, if we include (a) the obvious axioms for reasoning about ends and the “for the sake of” relation and (b) the axiom that only final ends are (independently) reason-giving,<sup>78</sup> it follows from these two claims that only representational knowledge is reason-giving. The only good reason to engage in scientific inquiry, on the narrow view, is for the sake of representational knowledge, even if some other goods (say, socially-valuable technology) are produced in the process. (For all of this, of course, the agent in question is taken to be a scientist or collective agent of scientists.)

To characterize properly the broad view, we must diverge from Richardson. In particular, Richardson takes the “for the sake of” relation to be antisymmetrical.<sup>79</sup> For reasons that will be clear below — if they are not obvious already — I reject antisymmetry for this relation. I therefore take a few paragraphs to address Richardson’s arguments for antisymmetry.

Richardson’s initial argument for antisymmetry is deeply obscure;<sup>80</sup> indeed, it may not be intended to be an argument for antisymmetry in the first place. Part of the difficulty is presentation. At the beginning of the relevant paragraph, Richardson has  $x$  in the second place of the relation (“for the sake of  $x$ ”); at the end of the paragraph,  $y$  is in this place (“for the sake of  $y$ ”), so it is not clear which place he

<sup>77</sup> Henry Richardson, *Practical Reasoning about Final Ends* (Cambridge, UK and New York: Cambridge University Press, 1994), 54, my brackets; note that Richardson has suppressed references to the agent  $S$  who pursues  $x$  and  $y$ . ISBN: 0521464722.

<sup>78</sup> That is, if an end gives an agent a good reason to engage in a certain course of action that does not depend on some further reason then that end is final. Non-final ends can give reasons as means to final ends, but in this case the reason is not independent.

<sup>79</sup> If  $x$  is pursued for the sake of  $y$  then  $y$  is not pursued for the sake of  $x$ .

<sup>80</sup> Richardson, *Practical Reasoning about Final Ends*, 54-5.

is referring to when he simply uses these variables. Further, the middle sentences speak of “one end,” “the other end,” and “the second end,” and it’s unclear which positions these refer to. Finally, even reading in various substitutions to clear up these ambiguities, nothing in this paragraph appears to be an argument for antisymmetry. The best argument that I can extract runs as follows: First we introduce another necessary condition on the “ $x$  for the sake of  $y$ ” relation, namely,  $y$  appropriately regulates the pursuit of  $x$ . Then we assert a premise that the appropriate regulation relation is antisymmetric. Hence the “for the sake of” relation is also antisymmetric. But the second premise strikes me as completely unmotivated, and perhaps is even question-begging.

Richardson’s second argument (if it is indeed the second) is much clearer. It takes the form of an intuition pump — a professor who values computing power both for its own sake and for the sake of efficiently completing writing projects — and then the following:

Would it be inconsistent of him to accept both directions of regulation? Although the answer is that bidirectional regulation of this sort is clearly possible, this need not disturb the general and tradition point about the antisymmetry of pursuit [that is, the “for the sake of” relation]. On a given occasion or in a given respect, either the writing or the computing power regulates.<sup>81</sup>

However, if I am reading this argument correctly, it has the following structure:

- (1) In this case, it seems that  $\neg p$ .
- (2) But a weaker version of  $p$ , namely  $p'$ , is consistent with this case.
- (3)  $\therefore p'$

In other words, we can weaken antisymmetry so that it is compatible with an apparently-conflicting intuition pump. While I suppose this might be consoling to someone who is inclined already, for other reasons, towards antisymmetry, it provides no new argument for antisymmetry.

If anything, Richardson’s intuition pump seems to support rejecting antisymmetry. More concretely, for at least some  $x$  and  $y$  (say, computing power and writing), I take the following claims to be jointly consistent:

- $x$  is a final end.
- $y$  is a final end.
- $x$  is pursued for the sake of  $y$ .

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<sup>81</sup> [Ibid.](#), 55-6.

- $y$  is pursued for the sake of  $x$ .

Call the conjunction of these claims — and, by extension, an instantiation for  $x$  and  $y$  — *weak symmetry of final ends*.

Now we can characterize the broad view, as the conjunction of the following four claims:

- For any internal good of scientific inquiry  $x$ ,  $x$  is pursued for the sake of representational knowledge.
- For any internal good of scientific inquiry  $x$ ,  $x$  is pursued for the sake of practical knowledge.
- For any internal good of scientific inquiry  $x$ ,  $x$  is pursued for the sake of technology.
- No other internal good of scientific inquiry is a final end.

This characterization immediately implies weak symmetry of final ends for representational knowledge, practical knowledge, and technology. And this symmetry, in turn, gives us another way to characterize the interdependence of the three primary internal goods: on the broad view, each is pursued both for its own sake and for the sake of the other two.

Finally, the basic disagreement between the narrow and broad view can be seen to be the status of practical knowledge and technology: while these are not final ends on the narrow view, they are final ends on the broad view.

# Chapter 4

## Science and values from the perspective of practice: Isolationism and the narrow view

### 4.1 Introduction

The conception of science as practice is now sufficiently developed to do philosophical work. In this chapter, I apply it to the science and values debate. In §4.1.1, I present this debate as it is usually construed; in §4.1.2, I start to recast this debate in terms of the conception of science as a practice developed in the last two chapters. The remainder of the chapter develops this reconstruction further, with respect to the narrow view. In §3.3.5, I argued (the connection argument) that the narrow view leads to worries about the threat of institutional domination (the connection hypothesis). In this chapter, I suggest that this explains three common features of the science and values debate. One of these features is related to the appeal to versions of the pure/applied distinction; a second is related to worries about institutional domination; and the third concerns who bears the burden of proof in this debate. In §4.2 I examine the pure/applied distinction and, in §4.3, the threat of institutional domination.

#### 4.1.1 The standard construal

As it is usually construed, the *science and values debate* is a debate over the “role” a set of “non-cognitive,” “non-epistemic,” “ethical,” “political,” “social” or “subjective” “values” and “value judgments” may “legitimately” play in “science” — in a phrase, over the ideal of value-free science. As the scare quotes suggest, the claim (and its negation) is frequently quite vague, and a productive exchange among interlocutors has failed often simply because the participants tend to talk past each other. For example, Helen Longino has argued that much of the debate has falsely

presupposed an incompatibility between “cognitive” values or approaches — roughly defined as “those that focus on evidential or justifying reasons in accounting for scientific judgment” — and “social” values or approaches — roughly defined as those that “focus ... on social interactions among the members of a [scientific] community.”<sup>1</sup> That is, many participants in the debate have assumed that “if an epistemic practice is cognitively rational, then it cannot be social” and “if an epistemic practice is social, then it cannot be cognitively rational.”<sup>2</sup> Hence, in several exchanges in the science and values literature, “one” side argues that scientific inquiry is rational while the “other” side maintains that it is social. Without the assumption that Longino calls the “rational/social dichotomy,”<sup>3</sup> there is no reason to think that there is any actual disagreement here. But, stated explicitly, the dichotomy is not very compelling. Why should rationality and sociality be thought to be incompatible?<sup>4</sup>

A standard narrative traces the modern form of the science and values debate back to Kuhn’s *The Structure of Scientific Revolutions*,<sup>5</sup> which argued that the processes by which a community of scientists form a consensus around a scientific theory are not “algorithmic” — as certain forms of logical empiricism and its descendants had hoped — and hence involved ineliminable subjective elements and, sometimes or even frequently, political conflict.<sup>6</sup> Very generally, the narrative continues, reactions to Kuhn over the next thirty years were closely correlated with disciplinary divisions: historians and social scientists accepted Kuhn’s conclusions enthusiastically, and began new research programs to examine the social processes by which consensus was formed; while philosophers and working scientists were critical of Kuhn, and their contributions to the debate generally attempted to limit the role that the sorts of features identified by the historians and social scientists could play within the abstract

<sup>1</sup> Longino, *The Fate of Knowledge*, 2.

<sup>2</sup> *Ibid.*, 1-2.

<sup>3</sup> *Ibid.*, 1.

<sup>4</sup> For simplicity of presentation, I’ll use “non-epistemic” values as a catchall term for those values, traditionally thought to play no “legitimate” role in “theory choice,” that critics of the “ideal of value-free science” argue do or should play such a “legitimate role.” These will be contrasted with “epistemic values,” which are those that participants generally agree do or should play such a “legitimate role.” I’ll also be dropping the scare quotes — I take it that my point about vagueness has been made. The epistemic/non-epistemic terminology seems to have been introduced by Ernan McMullin, “Values in Science,” in *PSA 1982: Proceedings of the 1982 Biennial Meeting of the Philosophy of Science Association*, ed. Peter Asquith and Thomas Nickles, vol. 2 (Philosophy of Science Association, 1983), 3–28.

<sup>5</sup> Kuhn, *The Structure of Scientific Revolutions*.

<sup>6</sup> For examples of the narrative, see Zammito, *A Nice Derangement of Epistemes*; and Steven Shapin, “Lowering the Tone in the History of Science: A Noble Calling,” in *Never Pure: Historical Studies of Science as if it was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority* (Baltimore: Johns Hopkins University Press, 2010), esp. pp. 4-5 and 7.

relations between evidence and theory. This culminated in the so-called Science Wars of the 1980s and 1990s. Over the last decade, with the emotional energy of the Science Wars exhausted, the positions have become more rigorous and sophisticated, and a fair amount of common ground has emerged between the two sides, but no general consensus has formed.

This standard narrative is inadequate in a number of ways. First and foremost, Kuhn's work was not the challenge to logical empiricism that it the standard narrative has taken it to be. Rudolph Carnap, logical empiricist *par excellence*, not only approved the manuscript version of Kuhn's book for publication in the *Encyclopedia of Unified Science* — the last major project of logical empiricism, in the early 1960s — but even said that Kuhn's work was similar to Carnap's own, calling it “really a fine piece of work.”<sup>7</sup> Indeed, on Carnap's view, the choice to adopt one scientific theory or another is ultimately based on “practical considerations” and “a given set of purposes,” “without recourse to overarching criteria” — including (on Carnap's noncognitivist metaethics) ethical and political values.<sup>8</sup> Second, the value-free ideal has never actually enjoyed the hegemonic status that the standard narrative attributes to it, even among logical empiricists. As several philosophers and historians of philosophy of science have shown,<sup>9</sup> philosophers (especially philosophers of science) have been deeply engaged in the science and values debate almost continuously for at least a century. Reisch, for example, shows that, prior to the Second World War, one of the most heated debates in academic philosophy was between Neo-Thomist proponents of the value-free ideal (most prominently Robert Hutchins and Mortimer Adler) and pragmatist and logical empiricist *opponents* of this ideal (most prominently John Dewey, Charles Morris, Otto Neurath, and Philipp Frank).<sup>10</sup>

One particular version of this history, which we might call the “Midwest narrative” or “Steel Belt narrative”, argues that these misperceptions of the standard narrative are due to the depoliticization and social dis-engagement of philosophy of science (and philosophy more generally) in response to McCarthyism.<sup>11</sup>

<sup>7</sup> George Reisch, “Did Kuhn Kill Logical Empiricism?” *Philosophy of Science* 58, no. 2 (June 1991): 266-7.

<sup>8</sup> *Ibid.*, 271; 274.

<sup>9</sup> C.F. Delaney, “The Changing Temper of American philosophy,” *The Review of Politics* 34, no. 4 (1972): 129–37; Don Howard, “Two Left Turns Make a Right: On the Curious Political Career of North American Philosophy of Science at Midcentury,” in *Logical Empiricism in North America*, ed. Gary Hardcastle and Alan Richardson (University of Minnesota Press, 2003), 25–93; Reisch, *How the Cold War Transformed Philosophy of Science*; John McCumber, *Time in the Ditch: American Philosophy and the McCarthy Era* (Northwestern University Press, 2001), among others.

<sup>10</sup> Reisch, *How the Cold War Transformed Philosophy of Science*, chs. 2-7.

<sup>11</sup> Three of the most prominent proponents of this narrative are Don Howard, of the University of Notre Dame in South Bend, Indiana; George Reisch, an independent scholar trained at the University of Chicago who still resides in Chicago; and Heather Douglas, who was trained at the University of Pittsburgh and is currently at the University



However, I feel that this narrative is limited as it has been written so far. None of the Steel Belt histories that I have read go beyond about 1962 — the year Students for a Democratic Society signed the Port Huron statement, Rachel Carson’s *Silent Spring* was published, and Vatican II was convened. The later 1960s and 1970s were a period of extensive *repoliticization* throughout the academy. For example, the rise of social history and the development of such “interdisciplinary disciplines” as gender studies and African-American studies during this time brought several humanistic and social science disciplines into close contact with prominent social and political issues. Ethics and political philosophy were also *repoliticized* during this period, whether by way of Rawls’s *A Theory of Justice*, Singer’s work in applied ethics, or increased Anglophone attention to the work of Herbert Marcuse and Jürgen Habermas.

I take it that the *explanandum* of the Steel Belt narrative is the contemporary, largely-depoliticized state of philosophy of science. Certainly the depoliticization of philosophy of science during the Cold War should be part of the *explanans*. But, given the extensive *repoliticization* of other sectors of the humanities between the early Cold War and today, it seems that the *explanans* should also explain why philosophy science either (a) was not *repoliticized* in the 1960s and 1970s or (b) was depoliticized once again, perhaps in the 1980s. This, more recent, period of time seems to me more important for explaining the current state of philosophy of science than the early Cold War, and yet so far this part of the narrative has not been written.

In any case, there is little disagreement among these alternative narratives about the Science Wars and its aftermath. Today, there seems to be broad agreement that something called “social values” or “ethical values” play a legitimate role in identifying which questions, topics, issues, or lines of research should be pursued and in whether and how the results of that research should be “applied.”<sup>12</sup> One might therefore think that the philosophical debate can bracket issues of science policy and applied science, focusing instead on the abstract question of the legitimate role non-epistemic values may play in epistemic or theory choice phases of scientific inquiry.

The contemporary version of the debate usually focuses on two or three arguments made by proponents of the view that non-epistemic values do play a legitimate role. All of these arguments are epistemological, in the sense that they deal with the influence of non-epistemic values on representational knowledge and its production and standards and generally operate at the abstract or conceptual level of mainstream analytic epistemology. An instance of the *no distinction argument* starts by giving some reasons (the particular reasons vary widely) for thinking that there is no, or no significant, distinction between epistemic and non-epistemic values; then, appealing to some sort of premise that epistemic values do play a legitimate role, the argument concludes that non-epistemic values also play a legitimate role. The no distinction

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of Waterloo in southern Ontario. A fourth prominent proponent, John McCumber, received his Ph.D. from the University of Toronto and has taught at Northwestern University and the University of Michigan at Dearborn.

<sup>12</sup> We’ll see several examples of this consensus over the next few chapters.



argument may be cast in a general form — attacking the general conceptual distinction in the first premise — or in a more particular form — arguing that some particular values occupy a gray area between epistemic and non-epistemic. For example, Longino has argued in at least two papers that a set of five values widely held to be epistemic actually has a conservative “political valence,” and further, that an alternative set is more appropriate for feminist scientists.<sup>13</sup>

An instance of the *underdetermination argument* starts by giving some reasons — typically logical or epistemological in nature, though in some cases from considerations of practical reason or the use of instruments — in support of a premise that epistemic values do not suffice to uniquely determine which of various rival representations should be accepted, concluding that epistemic values must be supplemented in some way by non-epistemic values. The most famous versions of underdetermination are associated with Pierre Duhem and W.V.O. Quine, but versions have also been made by Otto Neurath, Helen Longino, and others.<sup>14</sup> For example, Neurath’s version appeals to the general practical necessity of choosing a course of action given limited information. On Neurath’s view, the choice to accept or reject a hypothesis, or the choice from among various rival alternative hypotheses, is a choice of a course of action like any other, and must be made on the basis of limited information. But this limited information does not determine a unique best course of action — a unique correct hypothesis to accept — and so the decision must be made, in part, by appeal to an “auxiliary motive,” “which has nothing to do with the concrete aims in question.”<sup>15</sup>

The *inductive risk argument* is, I think, either a variation on underdetermination or a close cousin of underdetermination, and over the past decade it has received a great deal of attention. The most prominent recent version of this argument is due to Heather Douglas; it is, explicitly, a development of arguments originally given independently by Richard Rudner and Carl Hempel.<sup>16</sup> The simplest form of the inductive risk argument deals with thresholds of statistical significance in tests against a null hypothesis — that is, where a hypothesis  $H$  is tested by examining the likelihood  $p = Pr(O|H_0)$  of a null hypothesis  $H_0 = \neg H$  against a set of observations  $O$ . If

<sup>13</sup> Helen Longino, “Gender, Politics, and the Theoretical Virtues,” *Synthese* 104, no. 3 (September 1995): 383–97; Helen Longino, “Cognitive and Non-Cognitive Values in Science: Rethinking the Dichotomy,” in *Feminism, Science, and the Philosophy of Science*, ed. Lynn Hankinson Nelson and Jack Nelson (Dordrecht: Kluwer Academic Publishers, 1996), 39–58.

<sup>14</sup> Duhem, *La théorie physique*, ch. VI; Willard van Orman Quine, “Two Dogmas of Empiricism,” in *From a Logical Point of View*, ed. second (1951; Harvard University Press, 1961), 20–46; Longino, *Science as Social Knowledge*, ch. 3.

<sup>15</sup> Neurath, “The Lost Wanderers of Descartes and the Auxiliary Motive,” 4.

<sup>16</sup> Richard Rudner, “The Scientist Qua Scientist Makes Value Judgments,” *Philosophy of Science* 20, no. 1 (1953): 1–6; Carl Hempel, “Science and Human Values,” in *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: The Free Press, 1965), 81–96.

$p < p^*$ , where  $p^*$  is some fixed threshold likelihood value, then  $H_0$  is said to be unlikely and  $H$  is said to be statistically significant. There is then some reason to accept  $H$ . In this sort of test, statistical significance — that  $p$  is less than the threshold value  $p^*$  — is an epistemic value. But the threshold value  $p^*$  must be set, in some way or another. That is, how unlikely must the null hypothesis be for us to have reason to accept the hypothesis?

Douglas argues that the threshold value must be set at least in part by considering the potential consequences of accepting and acting on  $H$  should  $H$  be false. Suppose  $H$  is the claim that a certain level of exposure to bisphenol A (BPA) causes a significant increase in the incidence of certain kinds of cancer.<sup>17</sup> If we accept and act on  $H$  and it turns out to be false, we will implement some unnecessary regulations on plastics manufacturers, at some economic cost. On the other hand, if we reject  $H$  and act on the null hypothesis  $H_0$  and it turns out to be false then we will have failed to prevent some cases of cancer, at a cost of human lives.

If we are wary of unnecessary regulation and the economic costs thereby incurred, we will have reason to say that a high threshold value, say  $p^* = 0.05$ , is not stringent enough, and prefer a stronger test, say  $p^* = 0.01$ . On the other hand, if we are wary of additional deaths due to cancer, we will have reason to say that  $p^* = 0.05$  is stringent enough. Say  $0.01 < p < 0.05$ . In this case, the decision to accept  $H$  or not depends on whether we are more wary of unnecessary regulation or additional cancer deaths. But, of course, this is a matter of ethical and political values. It is an ethical question whether a small hit to the economy, say, is more important than tens of thousand of additional deaths due to cancer.

More generally, as I read her, Douglas claims that empirical evidence  $O$  and statistical analysis are insufficient to determine whether to accept or reject  $H$ . There is an epistemological gap — setting the threshold value  $p^*$  for acceptance in particular — that must be filled, in part, by an appeal to ethical and political values. Her argument thereby seems to me to be an example of an underdetermination argument. But others read the inductive risk argument as distinct from underdetermination. The details of the taxonomy will not concern us here.

Construed as focusing on these epistemological arguments, the science and values debate does not appear to require the full resources of the conception of science as practice. The debate is far more narrow, conceptualized, or abstract than even the narrow view, dealing almost exclusively with the standards for representational knowledge. It does not appear to require any considerations of the organization and virtues of practitioners, institutional domination, or the relation to other practices — what we might call the concrete aspects of the practice. This appearance is highly misleading. In the next subsection — and, more extensively, over the next few chapters — I argue that both the content and some of the underlying concerns of the two

<sup>17</sup> Debates over this claim were prominent in the US news a few years ago. BPA was, until recently and among other things, a common ingredient in rigid, clear, plastic bottles, including baby bottles.

basic positions in the debate are closely related to the two views of science as practice that I presented in chapter 3. Hence the conception of practice — the *full* conception of practice — provides tools for both understanding what is at stake in the debate and actually conducting it. In other words, the debate is best construed from the perspective of practice.

#### 4.1.2 From the perspective of practice

As I see it, the science and values debate is a debate over the proper relationship between scientific inquiry and a somewhat nebulous set of *ethical and political practices*. This set may be defined as those whose goods and virtues are “ethical” or “political” — but lacking an independent characterization of both “ethical” and “political,” this definition is of little help. Instead, like most other participants in the debate, I will define the set of ethical and political practices with a list of exemplars: feminist movements, anti-racist movements, anti-poverty movements, labor and unionization campaigns, the Catholic Worker movement, and various civil and human rights movements, both national and international in scope. Later in this dissertation, we will consider whether the Nazi movement might have been a practice, and whether certain contemporary libertarian-pro-technology movements (for lack of any neat term) might be practices. One might also be able to make a case that entrepreneurship is an ethical or political practice, if and insofar as its internal goods, thrift and hard work and petty bourgeois neighborliness, are indeed internal goods. More controversially, ethical and political practices might also include nationalist movements, white supremacist movements, Right to Life, and fundamentalist religious movements.

It may be objected that these activities all aim primarily — if not exclusively — at achieving political power (or wealth), and hence at external goods. However, in at least some cases, this is a mischaracterization. The primary aims of feminist movements, for example, might best be described as the liberation of women from male domination, the elimination of rigid gender roles and the strict divisions of labor on the basis of gender and sex, and full opportunities for people of all genders and sexes to lead rich and flourishing lives and enjoy autonomy and agency (of various kinds) more generally. As Martha Nussbaum puts it,

One’s feminism is not mere identity politics, putting the interests of women as such above the interests of other marginalized groups. It is part of a systematic and justifiable program that addresses hierarchy across the board in the name of human dignity.<sup>18</sup>

While Lisa Schwartzman has criticized Nussbaum’s “individualist” feminist methodology,<sup>19</sup> she identifies somewhat similar aims for feminist movements:

<sup>18</sup> Martha Nussbaum, “The Feminist Critique of Liberalism,” in *Sex and Social Justice* (Oxford and New York: Oxford University Press, 1999), 71.

<sup>19</sup> Lisa Schwartzman, *Challenging Liberalism: Feminism as Political Critique* (University

Discussing their concrete experiences with one another, women see patterns and similarities emerge; problems that formerly seemed to be rooted in women’s “nature” or in their own personal failures become recognizable as products and manifestations of male dominance and female subordination.<sup>20</sup>

While promoting these aims requires such external goods as political power, this is the case with any practice. The aims, I suggest, are best understood as internal goods, articulated in and justified by the work of feminist theorists and only achievable by feminist and similar movements and their work.<sup>21</sup>

Granting that “ethical and political practices” does indeed pick out some coherent family of practices, we can then define *ethical and political values* by reference to the internal goods — most obviously the goods and virtues, but often also the conception of eudaimonia — of the practices in this family. In the quotation from Nussbaum, for example, the virtue of respecting the dignity of women leads to the further virtue of self-respect that this virtue produces in women whose dignity is respected. Schwartzman discusses “consciousness raising,” a certain kind of activity — a virtue — of sharing one’s experiences with others and thereby identifying common experiences. This activity may lead to representational knowledge of these common problems and their causes, or to challenges to widely-accepted accounts of them — feminist theory. Similar virtues and good can be identified in the anti-racist, post-colonial, labor, and gay rights movements. As internal goods of non-scientific practices, ethical and political values count, from within scientific inquiry, as among (potential) pragmatic aims and standards.

A variation on the objection of two paragraphs ago has more force. This variation points out that many of these practices suffer institutional domination. This is arguably the case with the mainstream feminist movement,<sup>22</sup> the largest American

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Park: The Pennsylvania State University Press, 2006), ch. 5.

<sup>20</sup> Schwartzman, *Challenging Liberalism*, 107.

<sup>21</sup> Schwartzman’s own methods emphasize participation in feminist practices in ways that I find highly reminiscent of the labor movement. See, for example, *ibid.*, 90 and 103-9. This is probably not surprising — Schwartzman makes numerous references to the work of Catharine MacKinnon when describing her method, and MacKinnon’s own approach, while not Marxist, is deliberately developed in dialogue with Marxism. See Catharine MacKinnon, *Toward a Feminist Theory of the State* (Cambridge, MA: Harvard University Press, 1991), esp. part I, chs. 1-4.

<sup>22</sup> For examples of feminist critiques of the nearly exclusive focus on “choice” on the part of mainstream feminists in the abortion rights movement, see Catharine MacKinnon, “The Male Ideology of Privacy: A Feminist Perspective on the Right to Abortion,” *Radical America* 17, no. 4 (1983): 23–35 and Andrea Smith, ““Better Dead Than Pregnant”: The Colonization of Native Women’s Reproductive Health,” in *Conquest: Sexual Violence and American Indian Genocide* (Cambridge, MA: South Bend Press, 2005), especially 98-106. Women of color have also continuously criticized the mainstream

unions, Right to Life, and most if not all fundamentalist religious movements. To the extent that this objection is accurate it suggests that interactions with these practices must be handled quite carefully. The question, then, is whether these practices do in fact suffer institutional domination. As we shall see in later chapters, this is a matter for thoughtful, open-minded empirical investigation.

The *isolationist* position in the science and values debate is that *there are especially important or “central” aspects, parts, stages, or moments of scientific inquiry that ethical and political practices may not legitimately influence or inform.*<sup>23</sup> This position can be analyzed into two parts: first, a distinction (or perhaps gradation) between “central” and “marginal” aspects of scientific inquiry, and second, a claim that these central aspects must be protected or isolated from the influence of ethical and political practices. I do not define this influence, as it is understood in importantly different ways by different isolationists. Hopefully, my usage will be clear from the examples of isolationist views discussed over the course of this and the following chapters. At any rate, isolationism is intended to include the ideal of value-free science, though it may go beyond this.

The isolationist position does allow ethical and political practices some influence on scientific inquiry. As mentioned above and as we’ll see in more detailed below, many isolationists think that pragmatic aims and standards, understood as “social significance” or “ethical values,” can legitimately influence the choice of topics of scientific inquiry. For example, the pragmatic aim of treating a prevalent disease is a legitimate reason for pursuing a research program into the etiology and progression of that disease, that is, attempting to produce representational knowledge concerning the disease. However, the isolationist stresses, the standards for excellent representation — epistemic standards — of scientific inquiry must not be influenced by the pragmatic aim of developing a treatment for the disease. A single small study showing that incidence of the disease is moderately negatively correlated with ingesting a certain chemical compound, say, should not be taken to show that ingesting the compound prevents the disease simply because we are desperate for a treatment.<sup>24</sup>

The conception of practice can be utilized in an argument for isolationism, along the following lines:

- (I-1) The central aspects of scientific inquiry are the standards for its primary internal goods.
- (I-2) If the central aspects are influenced by ethical and political practices (more

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feminist movement for focusing exclusively on gaining political power for wealthy white women. See, for example, bell hooks, *Ain’t I a Woman: Black Women and Feminism* (Boston: South End Press Collective, 1981).

<sup>23</sup> I will generally use the verbs “influence” and “inform” synonymously.

<sup>24</sup> As the examples suggest, I take it that isolationists generally have no problem with influence running in the opposite direction, that is, from scientific inquiry to ethical and political practices.

generally, pragmatic aims and standards) then scientific inquiry will (or “is quite likely to,” or similar weakening) suffer institutional domination.

(I-3) Institutional domination should be avoided.

(I-4) ∴ Ethical and political practices (more generally, pragmatic aims and standards) should not influence the standards for the primary internal goods of scientific inquiry.

The plausibility of this argument depends on (I-2) and, less contentiously, (I-1) and the support these provide for the conclusion. (While the argument-sketch is deductively invalid as written, the premises still provide some or even good support for the conclusion.) Of course, a weaker version of (I-2) is more plausible, *ceteris paribus*, but then the move to (I-4) is more suspect.

In the connection argument of 3.3.5, I argued that the narrow view leads to concerns over institutional domination — the threat of domination — and what we can now identify as isolationism. This connection I called the connection hypothesis, and I supported it and describe the mechanism by which it occurs with the connection argument. The connection argument does not *support* (I-2) — it does not give reason to believe (I-2) — but it does *predict* that and *explain* why someone who holds the narrow view would tend to believe (I-2). Then, in the context of the argument for (I-4), the connection argument explains why the narrow view leads to isolationism.

The *transactionist* position, on the other hand, is that ethical and political practices may legitimately influence scientific inquiry in a very general way. Transactionism need not deny the first claim of isolationism (the distinction between central and marginal aspects); indeed, a version of transactionism cast in the language of practice probably should accept premise (I-1) of the argument-sketch above. Transactionism will differ from isolationism, then, in either rejecting (I-2) or claiming that the premises do not support the conclusion, and going on to claim that *ethical and political practices may legitimately influence even such central aspects of scientific inquiry as the standards for (any of) its internal goods*. Call this the *characteristic claim* of transactionism. Generally and for the most part, scientific inquiry may and perhaps even should be deeply engaged with ethical and political movements. Transactionism is intended to include the most prominent rejections of the value-free ideal.

The conception of practice can also be utilized in an argument for transactionism, along the following lines:

(T-1) Progress is an internal good of scientific inquiry.

(T-2) Progress can best be achieved by letting ethical and political practices influence the standards for the primary internal goods of scientific inquiry.

(T-3) The internal goods of scientific inquiry should be achieved.



(T-4) ∴ Ethical and political practices should influence the standards for the primary internal goods of scientific inquiry.

Like the argument-sketch for isolationism, this argument is deductively invalid as it stands, and the premises can be specified in various ways, some of which are more plausible than others. Isolationists probably should accept (T-1) and (T-3); the most contentious premise is thus (T-2). And, just as the connection argument explains why someone who holds the narrow view would tend to accept the crucial premise (I-2) of the argument for isolationism, the inverse connection argument of §3.4.4 explains why someone who holds the broad view would tend to accept the crucial premise (T-2) of this argument for transactionism. That is, just as the narrow view leads to isolationism, the broad view leads to transactionism.

Obviously the two basic positions, isolationism and transactionism, are extremely broad, and can be developed in various conflicting ways. Despite the complexities and the possibility of equivocation, I will generally speak of “the” isolationist position or “the” transactionist view and refer to “isolationism” and “transactionism.” In the argument-sketch for isolationism, for example, I included some possible weakenings or qualifications in parentheses. Isolationists, as I will understand them in this dissertation, need not be committed to the view that the standards for *all* the internal goods of scientific inquiry should not be influenced by ethical and political practices. More plausibly and moderately, they may think that only some special set of standards — the “epistemic” ones — should not be influenced in this way. That is, their concern may be limited to the role of ethical and political values in the logic of theory choice. Consequently, transactionism is a much smaller or strictly-defined set of views, and hence a much stronger claim (in a strictly logical sense!), than isolationism.

The connection argument’s support for the connection hypothesis is as *a priori* as any mathematical proof. In this chapter, I provide it with empirical support; the connection argument, and the accounts of scientific inquiry as practices that it presupposes, thereby also gain some abductive support. In particular, the description of the mechanism that connects the narrow view to isolationism — the connection argument — lets us make three predictions about the views and arguments of isolationists within the science and values debate. First, we predict that, when isolationists either defend isolationism or criticize transactionism, they will do so by appealing to *narrow view* versions of the pure/applied distinction.<sup>25</sup> And, second, isolationists will generally articulate specifications of the internal/external goods distinction for scientific inquiry, but fail to recognize specifications of this same distinction for ethical and political practices. For example, isolationists will make distinctions between knowl-

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<sup>25</sup> For their part, transactionists will either appeal to *broad view* versions of the pure/applied distinction or deny this distinction entirely. So this is one respect in which isolationists and transactionists will often argue past each other, implicitly appealing to very different conceptions of the primary internal goods of scientific inquiry. We shall pick this up again, as part of our examination of transactionism, in §4.2.



edge on the one hand and wealth and power on the other; but they will fail to make distinctions between feminism and environmentalism and wealth and power.

Third, given the prevalence of the narrow view among philosophers of science, the connection argument explains the burden of proof in the contemporary science and values debate. Again, the standard construal sees the debate as epistemological and focuses on two epistemological arguments for transactionism. Isolationists generally do not give positive arguments for their position; instead, they criticize transactionists.<sup>26</sup> Transactionism has the burden of proof in the debate: because the narrow view is widely accepted and is connected to isolationism, isolationism seems to be “natural” or “intuitive,” and it is up to transactionists to support and defend their position.<sup>27</sup>

Again, my aim in this chapter is to provide empirical support for the connection argument by showing each of these three features in the contemporary science and

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<sup>26</sup> Or at least attempt to. Again, vagueness, equivocation, and ambiguity often lead interlocutors to talk past each other. Furthermore, isolationists often attack very general versions of “social constructivism” and “relativism” that have little or nothing to do with any view actually held by any transactionist. See, for example, Susan Haack, “Science as Social? Yes and No,” in *Feminism, Science, and the Philosophy of Science*, ed. Lynn Hankinson Nelson and Jack Nelson (Dordrecht: Kluwer Academic Publishers, 1996), 79–93; Kitcher, *Science, Truth, and Democracy*, ch. 3; Paul Boghossian, “Constructivist and Relativist Conceptions of Knowledge in Contemporary (Anti-)Epistemology: A Reply to Barbara Herrnstein Smith,” *South Atlantic Quarterly* 101, no. 1 (Winter 2002): 213–27. Boghossian’s habit of misreading his targets is especially noteworthy. See, for example, Paul Boghossian, “The Maze of Moral Relativism,” *Opinionator*, July 24, 2011, <http://opinionator.blogs.nytimes.com/2011/07/24/the-maze-of-moral-relativism/> (accessed October 27, 2011), in which Boghossian cites an op-ed by Stanley Fish as an example of relativism — an op-ed in which Fish explicitly denies holding the position that Boghossian attributes to him. For a similar response to generalized isolationist arguments, see Elizabeth Anderson, “How Not to Criticize Feminist Epistemology: A Review of *Scrutinizing Feminist Epistemology*,” 2006, <http://www-personal.umich.edu/~eandersn/hownotreview.html> (accessed August 22, 2011).

For their part, transactionists do sometimes attack very nebulous or oversimplified versions of “value-free science” or “objectivism” that have little or nothing to do with any view actually held by any isolationist. See, for example, Sandra Harding, *Whose Science? Whose Knowledge?: Thinking from Women’s Lives* (Ithaca: Cornell University Press, 1991), ch. 6. But it seems to me that the latter are much less common than the former. I suggest that this is also due to the presumption the narrow view gives to isolationism: since transactionists have had to give positive arguments for their view, their views have required more sophistication and development than simply taking potshots at strawsociologists and strawfeminists. Then again, this observation may be simply due to sample bias on my part.

<sup>27</sup> Cf. Brown, “[Science and Experience](#),” 14–7.

values debate. In §4.2, I discuss three forms of the pure/applied distinction that are utilized to develop and defend versions of isolationism. In each case, the form of the distinction involves narrow view assumptions concerning the relative importance of representational knowledge, compared to practical knowledge and technology, in line with the first prediction. Then, in §4.3, I discuss the prominence of worries about the threat of institutional domination in isolationist criticisms of transactionism. As we shall see, articulating these worries generally involves associating ethical and political practices with commercial and political institutions; this is in line with the second prediction.

## 4.2 Pure and applied

In isolationist arguments, the distinction between pure and applied scientific inquiry takes at least three forms: with respect to activities, significance, and attitudes. Before discussing each of these forms, however, it is important to note that *none* of these are versions of the distinction between a “context of discovery” and a “context of justification,” or the similar (if not identical) distinction between psychology and sociology of belief formation on the one hand and epistemology on the other. While some isolationists criticize transactionists by claiming that the latter have violated something like this distinction,<sup>28</sup> I argued in §3.2.2 that this is inconsistent with production-dependent standards for scientific inquiry, which are widely recognized among contemporary philosophers of science. Hence, while this sort of strategy may seem appealing so long as one is dealing exclusively with abstract relations between evidence and theory, it falls apart as soon as one turns to the concrete activities of scientific practice. Melinda Fagan has argued, for example, that the discovery/justification distinction leads (in my jargon) to standards of normative epistemology that fail to provide any guidance to the ongoing production of representational knowledge.<sup>29</sup>

### 4.2.1 Activities

The first form of the pure/applied distinction is a distinction between the activities of developing representational knowledge and applying that knowledge to pragmatic aims. For example, Hugh Lacey, an isolationist, distinguishes between three “moments” of scientific inquiry: “adoption of strategy,” “theory appraisal/knowledge confirmation,” and “application of scientific knowledge,” arguing that “social values often have important legitimate roles at the first core moment, adopting a strategy” and the third moment of application, but “at the second moment all that matters is

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<sup>28</sup> This is one way of reading Boghossian, “[Constructivist and Relativist Conceptions of Knowledge in Contemporary \(Anti-\)Epistemology](#).”

<sup>29</sup> Melinda Bonnie Fagan, “Social Construction Revisited: Epistemology and Scientific Practice,” *Philosophy of Science* 77 (January 2010): 92–116.

whether the cognitive values are manifested in the light of available empirical data to a sufficiently high degree so that further investigation is unwarranted.”<sup>30</sup> We’ll examine Lacey’s view in more detail later in this section; for now, simply note that the second moment is distinguished from the third in a way that isolates the activity of pure science from any influence by the activity of applied science. In other words, Lacey’s distinction leads to the asymmetry between pure and applied scientific inquiry assumed in the connection argument.

### 4.2.2 Significance

The second form of the distinction between pure and applied scientific inquiry is a distinction between epistemic and pragmatic significance or value. Isolationists within the narrow view generally seem to agree that the aim of scientific inquiry is not just true belief (or veristic representations more generally) but *significant* true belief.<sup>31</sup> An excellent representation of the complex processes of photosynthesis, for example, is more valuable than a count of the number of blades of grass in my backyard. This is so not because of the stimulating challenge of producing the former or the tedium of producing the latter, but because, in some sense, the former is more important or valuable than the latter. Further, this form of the distinction says, an excellent representation of photosynthesis is valuable even apart from its relevance to pragmatic aims (trying to improve crop yields, say); it has value “on its own,” simply as a representation. The production of this representation, whatever its “pragmatic significance” or “pragmatic value,” also has “epistemic significance” or “epistemic value.” Or, the standards of this epistemic significance are independent of or not influenced by the standards of pragmatic significance. Or, the value of a representation is not (or not just) the uses to which it can be put. Or (one last formulation), epistemically significant representation is a final end.

There is a loose connection between this second form of the pure/applied distinction and the first form, in that the two kinds of activities are attempts to realize the two kinds of value:

The disciplines we pick out as science [might be thought to] count as part of science because they aim at, and sometimes deliver, truths of this special kind [viz., having epistemic significance], and they can be distinguished from technology precisely because the latter is focused on

<sup>30</sup> Hugh Lacey, “On the Interplay of the Cognitive and the Social in Scientific Practices,” *Philosophy of Science* (2005): 979; 980; Hugh Lacey, *Is Science Value Free?: Values and Scientific Understanding* (Routledge, 1999), chs. 8-10; Lacey, “On the Interplay of the Cognitive and the Social in Scientific Practices,” §5; Lacey, “On the Interplay of the Cognitive and the Social in Scientific Practices,” 980.

<sup>31</sup> See, for example, Haack, “Science as Social? Yes and No,” 80 and Kitcher, *Science, Truth, and Democracy*, ch. 6.

the practical.<sup>32</sup>

This form of the distinction may be threatened by a version of the no distinction argument: If epistemic standards are connected (in the right way) to epistemic significance and pragmatic standards are connected (in a similar way) to pragmatic significance, then an argument against the distinction between epistemic and pragmatic standards could be extended to an argument against the distinction between epistemic and pragmatic significance. On the other hand, an argument starting with the distinction in terms of significance could conclude with a claim that a distinction in terms of standards should be respected, that is, isolationism.

Chapter 6 of Philip Kitcher's *Science, Truth, and Democracy* seems to challenge the distinction between epistemic and pragmatic significance. While Kitcher is an isolationist,<sup>33</sup> he claims to reject any conception of "an allegedly context-independent notion of epistemic significance" that "has nothing to do with us and our ephemeral practical concerns" and therefore "take[s] moral and social values to be intrinsic to the practice of the sciences."<sup>34</sup> And, indeed, about half of this chapter is spent critiquing various such conceptions of epistemic significance.

However, Kitcher's positive proposal incorporates a conception of epistemic significance that, while contingent in a sense, is context-independent, irrelevant to ephemeral and practical concerns, and not influenced by the internal goods of ethical and political practices. To be sure, Kitcher's complete proposal also incorporates pragmatic standards of significance: significance in general is represented by diagrams that he calls "significance graphs," and which "do not embody the idea that significance (or epistemic significance) is always a matter of achieving, or pointing to, universal laws," or any of the other criteria of epistemic significance considered earlier in his discussion.<sup>35</sup> But Kitcher does want to defend the significance of such questions as "why the heavenly bodies move as they do, or ... the evolution of our hominid ancestors" — questions whose pragmatic significance is rather small, especially compared to the large amount of attention they have received over the whole history of modern scientific inquiry.<sup>36</sup> Hence he introduces a notion of "natural curiosity," "natural human curiosity," or "healthy curiosity," asserting that "*partly as a result of our having the capacities we do*, partly because of the cultures in which we develop, some aspects of nature strike us as particular salient or surprising."<sup>37</sup> These aspects of nature — along with the questions we pose about them — are significant in part because of their relation to certain aspects of a universal human nature, and therefore contingent (because the human relata are contingent) but context-independent (because they are taken to be significant by all non-"pathological" humans). In particular,

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<sup>32</sup> *Ibid.*, 65.

<sup>33</sup> *Ibid.*, ch. 3.

<sup>34</sup> *Ibid.*, 65 and 66.

<sup>35</sup> *Ibid.*, 80, his parentheses.

<sup>36</sup> *Ibid.*, 81.

<sup>37</sup> *Ibid.*, 81, my emphasis.

they are independent of any practices — including ethical and political practices — that are not likewise universal. While I assume that there are at least some universal practices (family and friendship; the provision of food, shelter, and clothing; music, dance, and visual arts, for example), I also assume that all of the particular ethical and political practices found in our society are not universal in this way. Hence any significance that depends on “natural human curiosity” must be independent of all less-than-strictly-universal ethical and political practices. I conclude that this sort of significance must be epistemic, in a way that at least suggests the connection argument’s asymmetry between the epistemic and the pragmatic.

To be sure, Kitcher’s version of the second form of the pure/applied distinction does not support his own version of isolationism.<sup>38</sup> Indeed, he takes the significance of most if not almost all particular research programs to derive from complex considerations that work to all but dissolve the division between pragmatic and epistemic significance; hence Kitcher’s *complete* account of significance may actually challenge the asymmetry between pure and applied scientific inquiry. Yet a version of isolationism could develop Kitcher’s account of epistemic significance in a more robust way, argue that the threads of significance are not nearly as tangled as Kitcher claims, and finally conclude with a robust distinction between pure and applied scientific inquiry and a novel version of isolationism.

### 4.2.3 Attitudes

The third form of the distinction between pure and applied scientific inquiry that I will consider here is a distinction between what is often called *acceptance* and other attitudes that agents might take to a representation.<sup>39</sup> For example, Hugh Lacey distinguishes acceptance from

some other important stances that may be taken toward theories (hypotheses, proposals, posits, or conjectures): provisionally entertaining them, adhering to them in research practices, endorsing their greater evidential support (compared to rival theories) and *applying them in practical life*.<sup>40</sup>

He goes on to define the acceptance of a theory  $T$  as the judgment that, “in the light of the available evidence,  $T$  . . . is sufficiently well supported that it need not be

<sup>38</sup> His isolationism is based instead on the third form of the distinction.

<sup>39</sup> The differences between the first and third forms may not be immediately clear. The first form makes a distinction between the production of a representation and its subsequent use. It is therefore a distinction between kinds of *activities*. The third form makes a distinction between different *attitudes* we might take towards the products of these activities.

<sup>40</sup> Lacey, *Is Science Value Free?* 13, my emphasis, his parentheses. Compare the first two examples to those of Haack, “Science as Social? Yes and No,” 84-5.

submitted to further investigation.”<sup>41</sup> Acceptance is therefore a “strong stance to take toward a theory,” and only “comes at the end of a process of research . . . after having made numerous theory choices” between provisional versions of *T* and its rivals.<sup>42</sup>

Similarly, in response to Douglas’s argument from inductive risk, Sandra Mitchell argues that Douglas confuses the attitudes of accepting a hypothesis and acting on a belief (such as taking a hypothesis as a basis for public policy).<sup>43</sup> Very different kinds of “values,” she argues, are relevant to each of these two very different attitudes, and the distinctions between values and attitudes are all related to the different “role obligations” of scientists and policy advisors. Hence, although Douglas is correct to argue that “both epistemic and nonepistemic values contribute to judgment[s]” made in “policy contexts,” Mitchell claims the two sets of values must be kept both “analytically separate” and “institutionally recognize[d]” as distinct.<sup>44</sup>

Acceptance plays a key role in Lacey’s sophisticated version of isolationism. On Lacey’s view, the primary aim of science is “to gain understanding of phenomena,” which “always involves being able to explain it [“an object”] and to identify the possibilities open to it.”<sup>45</sup> As I read him, this aim of understanding is largely, if not exclusively, an aim of representational knowledge. That is, Lacey is assuming the narrow view. Rather than giving a determinate account of explanation or understanding (DN, causal, unificationist, and so on), Lacey lists 7-12 (depending on how they are individuated) “forms” of understanding. For example, understanding an object may involve “the other objects or systems with which they share the same explanatory principles, and the particular conditions (often instances of variables of the principles) that account for different variations in the different systems” or “their relationships with the environments — physical, ecological, human, social, (sometimes) spiritual — in which they are located, and the sort of reciprocal interactions they exhibit with one another, including how they relate to us (human beings) and what we can do with them.”<sup>46</sup>

While these forms of understanding are not mutually exclusive in a logical sense, limitations on our material and cognitive resources require us to choose certain forms of understanding to pursue at the expense of others. For example, Lacey argues at length that modern scientific inquiry has generally pursued the first form of understanding quoted above — which he calls “wide-ranging understanding” or “materialist strategies” — at the expense of the second — which he calls “full understanding,”

<sup>41</sup> Lacey, *Is Science Value Free?* 13. Note that Lacey uses “theory” as a generic, catchall term, in much the same way that I use “representation.”

<sup>42</sup> *Ibid.*, 13, 14.

<sup>43</sup> Sandra Mitchell, “The Prescribed and Proscribed Values in Science Policy,” in *Science, Values, and Objectivity*, ed. Peter Machamer and Gereon Wolters (Pittsburgh: University of Pittsburgh Press, 2004), 251.

<sup>44</sup> *Ibid.*, 252; 246, my brackets.

<sup>45</sup> Lacey, *Is Science Value Free?* 102; 96, my brackets.

<sup>46</sup> *Ibid.*, 97, my brackets, his parentheses.



and associates with the “research strategies” of feminist scientists and Latin American “grassroots” or “popular” agroecological movements.<sup>47</sup> Lacey calls this choice of which form of understanding to pursue the choice of “strategy,” identifies it as the first of three moments of scientific inquiry, and argues that “social values” play a role in this choice of strategy.<sup>48</sup> In particular, the choice of materialist strategies is related to a particularly modern “social value” of “control over nature,”<sup>49</sup> while the choice of full understanding is related to an array of “social values” that include egalitarianism and an attitude of respect towards and reciprocity with nature.<sup>50</sup> That is, strategies or forms are chosen on the basis of the reasonable belief that the understanding produced by them will promote those “social values.”

Recall the second prediction made by the connection argument: that philosophers of science working within the narrow view would make a distinction between external goods and representations, but fail to make a similar distinction between external goods and the internal goods of ethical and political practices. This is exactly what Lacey does. Lacking a distinction between internal and external goods, Lacey cannot distinguish between the internal goods of ethical and political practices — including feminism and Latin American grassroots movements — and such external goods as money, fame, and power. For example, he characterizes all of the following as potential “outside influences” on scientific inquiry: “values and (for example) metaphysics, power, personal ambition, popular appeal, government, economic interests, law, religion, the military, ideology, the ‘will of the majority’ and special interests of any kind.”<sup>51</sup>

Consider Lacey’s account of the “social value” of control over nature. The materialist strategies associated with control “aim[] to . . . produce understanding across the widest range of experimental, technological and natural spaces” and “abstract[] from the human, social, and ecological characterizations that also fit these spaces.” Wide-ranging understanding thereby “aims to be context-free understanding.”<sup>52</sup> Detached from practices in this way, control sounds very much like the application of representational knowledge to external goods, power in particular. Indeed, Lacey rejects the purported “neutrality,” “evenhandedness,” or universal applicability of the understanding produced under material strategies in terms that I find strongly reminiscent of the application of representational knowledge to external goods:

the significance [applicability] of the scientific theories that inform the green revolution [twentieth and twenty-first century biotechnology, espe-

<sup>47</sup> Lacey, *Is Science Value Free?* ch. 6; chs. 9 and 8, respectively.

<sup>48</sup> See the discussion above, p. 111, of Lacey’s views with respect to the first form of the distinction between pure and applied scientific inquiry.

<sup>49</sup> Lacey, *Is Science Value Free?* 117.

<sup>50</sup> *Ibid.*, 112.

<sup>51</sup> *Ibid.*, 82, his parenthesis. Note that ethical and political practices are intermingled with external goods in this list.

<sup>52</sup> *Ibid.*, 98, my brackets.



cially genetic engineering] is largely restricted to the the interests of the market and of other values closely linked with the modern values of control — and does not extend, for example, to those viable value complexes that highlight local well-being, agency and community.<sup>53</sup>

This account of the relationship between social values and the choice of research strategy may make Lacey sound more like a transactionist than an isolationist: pragmatic aims and standards for technology seem to play a considerable role in the standards for representational knowledge.<sup>54</sup> This, however, is a deep misunderstanding of Lacey’s view.<sup>55</sup> Pragmatic aims and standards do play a role in choosing a strategy and which form of understanding to pursue, and hence in certain features of the representations produced by subsequent scientific inquiry. But they play absolutely no role in the standards by which those representations are evaluated, that is, in the standards for acceptance. To simplify slightly, a representation  $T$  is acceptable if and only if  $T$  achieves the aim of understanding to such a high degree that any further development or testing of either  $T$  or its rivals is not expected to produce any increase in understanding.<sup>56</sup> This criterion of acceptability does not refer to any specific form of understanding, and hence neither depends on nor implies any strategy. Further, it does not take any considerations of significance or applicability into account. Hence pragmatic aims and standards play absolutely no role in, have absolutely no influence on, the standards of acceptability.

To finally come to my point: Lacey’s isolationism is based on the special status he assigns to acceptance. It is unique among the attitudes one might take towards a representation in two ways. First, it is given its own “moment” of scientific inquiry; all other attitudes are relegated to the third and, in a few cases, first moment.<sup>57</sup> It is thereby isolated from other attitudes. Second, by isolating it in its own “moment,” Lacey can claim that it is not influenced by other attitudes, though it may have some

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<sup>53</sup> *Ibid.*, 239, my brackets.

<sup>54</sup> Peter Machamer and Heather Douglas, “Cognitive and Social Values,” *Science and Education* 8 (1999): 45–54, for example, argue for a transactionist position on Lacey’s grounds. In a reply, Lacey argues that Machamer and Douglas have misunderstood his position, including the special status he gives to acceptance, discussed below, and his attitude towards materialist strategies. Hugh Lacey, “Scientific Understanding and the Control of Nature,” *Science and Education* 8 (1999): 13–35

<sup>55</sup> Lacey’s writing style may be at least partly responsible here. This is unfortunate, as I believe Lacey’s version of isolationism is much more sophisticated and relevant than Kitcher’s. Yet Kitcher is much more prominent than Lacey.

<sup>56</sup> Compare this to the second component of Lacey’s definition of “impartiality,” Lacey, *Is Science Value Free?* 230. I am simplifying this summary by skipping his account of “cognitive values,” which connect the standards for acceptance to the aim of understanding.

<sup>57</sup> Thus Lacey, in contrast with Kitcher (n. 38), builds the first form of the distinction between pure and applied science on the third form.

influence over them (if  $T$  is acceptable then there is some reason to think that  $T$  would be useful in trying to satisfy some pragmatic aim, and so on). The attitude of acceptance is thus situated asymmetrically relative to other attitudes, including those that are influenced by ethical and political values.

All of the other isolationists we have encountered take a similar approach: some sort of distinction is made between pure science or epistemic interests and applied science or pragmatic interests, and even when it is conceded that the internal goods of ethical and political practices may legitimately influence the pragmatic, isolationists claim that they may not legitimately influence the epistemic. Note that this approach claims, not just that representational knowledge is an important internal good of scientific inquiry, but that it is *more* important than technology and practical knowledge. And this is exactly the asymmetry between pure and applied scientific inquiry that the connection argument assumes: while the applied or pragmatic side of science may be influenced by the pure or epistemic side, the converse is not true.

Furthermore, this asymmetrical version of the pure/applied distinction is not acceptable on the broad view.<sup>58</sup> So isolationists, in appealing to this distinction when they articulate or defend isolationism, are assuming the narrow view of scientific inquiry, just as the connection argument predicts.

## 4.3 The threat of domination

### 4.3.1 The threat

I turn now to the consequent of the connection hypothesis: the threat of institutional domination. I first briefly recount the nature of the threat, discussing particular examples in the next subsection. Overall, my aim is to show not just that isolationists are motivated by this threat, but that they understand it in a characteristic way: they distinguish representation from external goods, but do not distinguish between external goods and the internal goods of ethical and political practices. In other words, characteristically, isolationists will separate wealth and power from representational knowledge, but not from ethical and political values. On the narrow view, as per the connection argument, ethical and political practices do not appear to be practices at all; rather, they appear to be institutions, and so their aims (namely, ethical and political values) appear to be external goods. Representational knowledge, as an internal good, is a very different kind of good from wealth and power; on the narrow view, this does not appear to be the case for ethical and political values. This peculiar feature of isolationism is predicted and explained, I claim, by the recognition claim as it operates in the connection argument. And so this point is the second piece of empirical evidence for my construal of the debate.

The threat of domination might be made more concrete as follows. First, a cer-

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<sup>58</sup> I shall discuss broad view versions of this distinction in §6.4.

tain ethical or political practice  $P$  is, from within scientific inquiry, either mistaken for the pure pursuit of external goods or is perceived (correctly or incorrectly) to suffer institutional domination. The threat of domination is then the worry that, if scientific inquiry is influenced by  $P$ , it will (also) suffer institutional domination. I take it that the two major political parties in the United States, for example, either suffer overwhelming institutional domination or are nothing more than coordinated campaigns to acquire and keep political power.<sup>59</sup> From the perspective of scientific inquiry, then, if either political party is permitted to influence the standards for representational knowledge of scientific inquiry, these standards will likely come to reflect, not the internal good of excellent representational knowledge, but the external good of partisan political power. Scientific inquiry, and the representational knowledge it produces, will be reduced to nothing more than a weapon with which to fight political battles.

It is not obvious that any of the isolationist views and arguments discussed in the last section have anything to do with worries about the threat of domination. But Elizabeth Anderson — herself a transactionist — has argued that the threat of domination is extremely relevant to the science and values debate and the failure to address it is a considerable weakness of underdetermination arguments for transactionism.<sup>60</sup> Daniel Steel — also a transactionist — has recently made a similar point against no distinction arguments.<sup>61</sup> Both critiques point out that, while these arguments may give some reason to think that something other than the traditional epistemic values influence epistemic aims and standards, they do not generally describe how this does or should work, nor “help us evaluate the different ways that values might be deployed in inquiry” so as to distinguish those “uses of values . . . [that] are illegitimate” from those that are legitimate.<sup>62</sup> That is, certain kinds of interactions between scientific inquiry and ethical and political practices can harm scientific inquiry; transactionists must recognize this, and give an account of how this harm works and can be avoided. This harm, I suggest, should be understood as institutional domination, and so isolationists are worried about the threat of domination.

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<sup>59</sup> On any conception of a political movement on which the primary aim is the achievement of political power, the movement cannot be a practice — they are pursuing external goods, not internal goods. Thus any agonistic or interest-group model of politics that views, say, anti-capitalist, feminist, or postcolonial movements in this way is incompatible with viewing these movements as practices. Political parties can, however, serve as *institutions* for practices. Hence, while it is plausible to maintain that the Democratic Party *itself* is engaged in little more than the pursuit of political power, it is almost as plausible to maintain that it does so as an institution for an array of egalitarian liberal political practices. The role of the state will be discussed in §7.5.

<sup>60</sup> Elizabeth Anderson, “Uses of Value Judgments in Science,” *Hypatia* 19, no. 1 (Winter 2004): 1–24.

<sup>61</sup> Daniel Steel, “Epistemic Values and the Argument from Inductive Risk,” *Philosophy of Science* 77 (January 2010): 14–34.

<sup>62</sup> Anderson, “Uses of Value Judgments in Science,” 2.

The next subsection attempts to do two things: first, to document the extent of worries about institutional domination in the science and values debate, and second, to show isolationists associating ethical and political values with external goods in the way described in the opening paragraph.

### 4.3.2 And isolationism

Anderson identifies the threat of domination in the work of Susan Haack, Noretta Koertge, and Robert Almeder, isolationist critics of feminist versions of transactionism.<sup>63</sup> It also appears in at least some of Paul Boghossian's attacks on transactionism. In his contribution to an exchange with literary theorist Barbara Herrnstein Smith, Boghossian lays out a "classical conception of knowledge" using the "justified true belief" schema.<sup>64</sup> Boghossian's classical conception includes three claims; the first of these is the claim that "the only considerations that are reasons for believing something are those that bear on the truth of the belief."<sup>65</sup> He then considers whether a "constructivist" — some, or perhaps any, sort of transactionist — might deny this claim. His response to this possibility is admittedly "short" — a single paragraph — and comprises, not argument, but caricature.<sup>66</sup>

It is as if to say, 'Look, I know that the fact that it will advance my career if everyone were to believe Maxwell's equations for electromagnetism is irrelevant to whether the equations are true, but I nevertheless think that it's a reason for believing them.'<sup>67</sup>

<sup>63</sup> Anderson, "How Not to Criticize Feminist Epistemology," §1(a). See also the examples in her discussion of the "psychological" argument for isolationism, Anderson, "Uses of Value Judgments in Science," 4ff.

<sup>64</sup> Barbara Herrnstein Smith, "Cutting-Edge Equivocation: Conceptual Moves and Rhetorical Strategies in Contemporary Anti-Epistemology," *South Atlantic Quarterly* 101, no. 1 (2002): 187–212; Boghossian, "Constructivist and Relativist Conceptions of Knowledge in Contemporary (Anti-)Epistemology"; Barbara Herrnstein Smith, "Reply to an Analytic Philosopher," *South Atlantic Quarterly* 101, no. 1 (2002): 229–42.

<sup>65</sup> Boghossian, "Constructivist and Relativist Conceptions of Knowledge in Contemporary (Anti-)Epistemology," 218.

<sup>66</sup> Unfortunately, it seems to me that this is often Boghossian's strategy: rather than engaging in the hard work of developing a charitable interpretation of Herrnstein Smith's position in the sociolect of analytic philosophy, Boghossian quotes Herrnstein Smith or other transactionists, professes confusion and difficulty understanding the quoted views, and then attacks what appears to be a figment of his own imagination. Boghossian's more recent book on relativism and transactionism seems to "argue" along broadly similar lines. Paul Boghossian, *Fear of Knowledge: Against Relativism and Constructivism* (Oxford and New York: Oxford University Press, 2006) For a positive review of Boghossian's book, see John Searle, "Why Should You Believe it?" *The New York Review of Books*, September 24, 2009, no. 14.

<sup>67</sup> Boghossian, "Constructivist and Relativist Conceptions of Knowledge in Contemporary

The attitude expressed within the innermost quotes is, I suggest, institutional priority: *kudos* or status is an external good, and the hypothetical speaker is taking *kudos* to be more important than the proper epistemological standards for the internal good of excellent representational knowledge. While Boghossian might concede that there's nothing too troubling about the occasional individual scientist thinking this way, should this sort of attitude become widespread among scientists, institutional domination — indeed, institutional domination of an especially nasty and crude sort — is quite likely. Thus, to the extent that Boghossian has an argument against Herrnstein Smith or any other transactionist here, it is that *transactionism threatens institutional domination*.

The threat of domination is also apparent in Sandra Mitchell's critique of Heather Douglas's transactionism. After reconstructing Douglas's argument she says the following:

But this [Douglas's argument] does not yet point clearly to the values that legitimately play a role in science policy and those that should be eschewed. Surely nonepistemic consequences can always follow, even for the most politically innocuous theories. *Individual fame, fortune, possibility of promotion, and so on* are at risk when a scientist declares the truth of a hypothesis, since it can turn out later to be demonstrably false.<sup>68</sup>

In the next sentence, Mitchell characterizes the things that I have emphasized as “idiosyncratic personal values,” as distinguished from both “narrowly epistemic” values — which Mitchell believes do have a legitimate role to play in scientific inquiry — and the “political values” whose influence on scientific inquiry are the primary point of contention between Mitchell and Douglas. Thus, Mitchell is closer than other isolationists to recognizing the distinction between external goods and internal goods; indeed, her argument here may be that *Douglas* lacks an adequate conception of the distinction, and consequently an adequate distinction between the clearly illegitimate influence of external goods on scientific inquiry — which threatens institutional domination — and the arguably legitimate influence of the internal goods of ethical and political practices. In any case, her point is basically the same as Boghossian's: transactionism threatens institutional domination.

Lacey's isolationism may be thought to be an exception to this pattern. Where the other contemporary isolationists that we have encountered thus far are clearly worried about the harmful influence of leftist politics — especially feminist politics — on scientific inquiry, Lacey views both feminist movements and Latin American leftist popular movements quite favorably, and calls for their aims and standards to replace or at least supplement the aim of control of nature.<sup>69</sup> In his most recent book, he argues at length that a research strategy he call “agroecology” should supplement

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(Anti-)Epistemology,” 218.

<sup>68</sup> Mitchell, “The Prescribed and Proscribed Values in Science Policy,” 254, my emphasis.

<sup>69</sup> Cf. my presentation of Lacey's views in §4.2.1.

and potentially even replace research into transgenic agricultural technologies.<sup>70</sup> This argument is worth examining in detail.

Lacey frames his discussion in terms of competing strategies — agroecology and transgenics — and surveys both the quantity and quality of research under each and considers their value for “developing” communities.<sup>71</sup> While transgenics have received a great deal of attention, and consequently been subject to high-quality scientific inquiry (that is, scientific inquiry producing excellent representational knowledge), development of new transgenic crops is slow and expensive (due to the need to subject them to extensive risk analysis testing) and patent protections on transgenic seeds require farmers to make significant capital investments in seeds — and consequently take on significant debt — at the beginning of each growing season.<sup>72</sup> Transgenics therefore seem to be appropriate only for crops grown commercially on relatively large commercial farms. Thus, however excellent they are by epistemic standards, transgenic technologies are somewhat lacking by the pragmatic standards of small farmers in “developing” communities.<sup>73</sup>

Lacey presents agroecology as a pragmatically significant rival to research on transgenics. Agroecological approaches attempt to manage and care for crops and animals by treating them as parts of a complex local ecological system; the goal is to keep this system stable and largely self-regulating in ways that promote the growth of desirable crops and animals. For example, suppose a species of caterpillar is normally the

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<sup>70</sup> Hugh Lacey, *Values and Objectivity in Science: The Current Controversy about Transgenic Crops* (Lanham, Maryland: Lexington Books, 2005), part 2.

<sup>71</sup> While Lacey focuses on communities in Latin America and the Global South more generally, several economists, historians, and sociologists have argued that Appalachia — especially the rural areas dominated by mining — should be understood as an undeveloped or periphery region or an “internal colony” of the US. See, for example, Eban Goodstein, “Landownership, Development, and Poverty in Southern Appalachia,” *Journal of Developing Areas* 23, no. 4 (July 1989): 519–34.

<sup>72</sup> For example, chemical company Monsanto owns the patent on a transgenic technique that makes crops immune to its herbicide Roundup (glyphosate). “Roundup Ready” seed lines were used in 93% of the soybeans and 82% of the corn produced in the US in 2009. Monsanto’s contracts with farmers forbid them from saving any seeds produced by “Roundup Ready” crops and planting them at the beginning of the next season; farmers who do so are sued for patent violations. Farmers are offered rebates on the royalties paid to Monsanto for use of the glyphosate-resistant strains if they use Monsanto herbicides as opposed to generic herbicides imported from China. This dominance of the soybean and corn seed markets prompted antitrust investigations in seven states in 2010. See Alison Fitzgerald, “Monsanto 7-State Probe Threatens Profit From 93% Soybean Share,” *Businessweek*, March 10, 2010, <http://www.businessweek.com/news/2010-03-10/monsanto-7-state-probe-threatens-profit-from-93-soybean-share.html>.

<sup>73</sup> Cf. Marion Nestle, *Safe Food: Bacteria, Biotechnology, and Bioterrorism*, second edition (Berkeley and Los Angeles: University of California Press, 2010).



prey of the adult females of a wasp species, and that the males of this wasp species especially like the nectar of certain flowers. Planting these flowers amid the desired crops would attract the wasps and help regulate the caterpillars. As Miguel Altieri, an ecologist and prominent proponent of agroecology, puts it,

The idea is to apply the best management practices in order to enhance and /or regenerate the kind of biodiversity that can subsidize the sustainability of agroecosystems by providing ecological services such as biological pest control, nutrient cycling, water and soil conservation, etc.<sup>74</sup>

The practical knowledge and technology developed by agroecology is meant to be a rigorous development of the practical knowledge and technology used by poor and peasant farmers in “developing” communities, to require no-to-modest capital investment, and to be economically and environmentally sustainable.<sup>75</sup> It is thus, by design, generally appropriate for use in “developing” communities; Altieri has argued numerous times that agroecology is far superior to transgenics in this respect. Yet agroecology has largely been ignored and unfunded. It has a strong potential for both epistemic and pragmatic excellence, but has been neglected.

Lacey goes on to argue that criticisms of the agroecological strategy — roughly equivalent to arguments for an exclusive focus on transgenics — are therefore premature. Agroecology may or may not be viable as a substantial, long-term line of scientific inquiry; we have no way of knowing until it has received at least some significant attention. He concludes that criticism of agroecology depends in large part on the following premise:

There are no alternative kinds of farming — *within the trajectory of the socioeconomic system based on capital and the market*—that could be deployed instead of the proposed [transgenic]-oriented ways, without occasional unacceptable risks . . . , and that reasonably could be expected to produce greater benefits concerning productivity, sustainability, and meeting human needs; *and outside of this trajectory there are no genuinely realizable possibilities.*<sup>76</sup>

Leaving out some of the qualifiers and clauses that are important for Lacey’s purposes but a distraction from mine, the premise can be simplified:

There are no alternatives to transgenics within the socioeconomic system based on capital and the market.

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<sup>74</sup> Miguel Altieri, “Multifunctional Dimensions of Ecologically-Based Agriculture in Latin America,” *International Journal of Sustainable Development and World Ecology* 7, no. 1 (2000): 70.

<sup>75</sup> *Ibid.*, 71.

<sup>76</sup> Lacey, *Values and Objectivity in Science*, 231, his emphasis, my brackets.



As I read him, Lacey is arguing here that proponents of the transgenics-only strategy believe that market forces are both incompatible with the practices of agroecology and sufficiently stronger than these practices to destroy them.<sup>77</sup> The institutional domination of contemporary agricultural practices (and associated practices of scientific inquiry) is so thorough that any alternative practice will also suffer institutional domination; since agroecology could not survive institutional domination but transgenics can, agroecology is doomed to failure and agronomy should focus exclusively on transgenics.

Notably, in his response to this argument, Lacey does not deny that contemporary agriculture and scientific inquiry suffer institutional domination: responding to this premise, he says, “would depend (in part) on the possibility of creating spaces for the development of alternatives within the currently market-dominated system.”<sup>78</sup> Instead, he challenges the second part of the claim, pointing to the possibility of alternative socioeconomic systems in which institutional domination is not a pervasive problem and agroecology is not doomed to failure:

Certainly, there are movements that question the inevitability of the actual trends and the values of the institutions which are restructuring the world . . . . They do not deny the actuality of the social trends . . . and the powerful forces driving them, so that they recognize that no alternatives could be implemented without a struggle . . . . When movements that embody competing values are organized in “developing” countries and gain some political strength, and . . . [the argument above] is [then] posed against their endeavors, holding the values of capital and the market (and the modern valuation of control) is the key support of [the argument].<sup>79</sup>

Insofar as the argument against transgenics takes a certain socioeconomic system for granted, it is a piece of ideology,<sup>80</sup> and a rhetorical club used against ethical and political practices that aim to create alternatives.

Lacey’s isolationism, I suggest, is less a response to the *threat* of institutional domination and more to the *fact* of institutional domination — not the threat that scientific inquiry *may be* politicized, but instead that it *has been* commercialized. His broad conception of understanding as the aim of scientific inquiry allows him to characterize both agroecological and transgenic strategies as genuinely scientific, and his account of acceptance — which is not influenced by choice of strategy, including the choice of materialist strategies — requires that the kind of understanding produced by

<sup>77</sup> Compare this reconstruction to the paragraph on Lacey, *Values and Objectivity in Science*, 231-2.

<sup>78</sup> *Ibid.*, 233, his parenthesis.

<sup>79</sup> *Ibid.*, 232.

<sup>80</sup> In the sense of Iris Marion Young, *Justice and the Politics of Difference* (Princeton: Princeton University Press, 1990), 74, ISBN: 9780691023151.

transgenic strategies be compared fairly with the understanding produced by agroecological strategies before either is accepted. The failure to pursue agroecological research is, on Lacey's view, an *epistemic* failure. Agroecological strategies cannot be dismissed as mere romanticism, blind adherence to tradition, pseudoscience, or artisan tinkering; like the "doomed to fail" argument, these are all pieces of capitalist ideology.<sup>81</sup>

Note that, if my reconstruction of Lacey's isolationism is accurate, it shows that isolationism is not an essentially politically conservative position. It can be used to generate powerful criticisms of the social and political status quo, including the deleterious effects of commercialization on both scientific inquiry and leftist ethical and political practices (indeed, and the connection between these two). Similarly, there are good reasons to think that transactionism is not an essentially politically leftist position.<sup>82</sup>

To sum up this chapter: We have seen that a narrow view versions of the distinction between pure and applied scientific inquiry and concerns about the threat of institutional domination are pervasive features of isolationism. The connection argument explains this correlation: the pure/applied distinction prevents the partly external progress that is necessary for any extra-scientific practice to be perceived as a practice rather than the pursuit of external goods. So the connection hypothesis — along with the associated apparatus of the connection argument and the general conception of practice — enjoys some abductive support.

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<sup>81</sup> Lacey, *Values and Objectivity in Science*, 233, *P<sub>4b</sub>*.

<sup>82</sup> See the discussion of the case of Pascual Jordan in §5.5.



# Chapter 5

## Some case studies in transaction

### 5.1 Introduction

We turn now to the transactionist side of the science and values debate. This chapter has two aims, one expository and one argumentative. The expository aim is simply to present and give some analysis of a variety of transactionist views; since narrow view isolationism is, I claim, prevalent and often taken to be “obvious” among contemporary philosophers of science, I do not wish to assume that my readers are all intimately familiar with transactionism.

The argumentative aim is to show that appeals to and attempts to stimulate progress within scientific inquiry — especially partly external progress — figure prominently in the work and arguments of transactionists. Recall that, in §4.1.2, I sketched an argument for transactionism whose most controversial premise, (T-2), appealed to the possibility of partly external progress. As I pointed out, this premise is liable to be rejected by someone who holds the narrow view, and accepted, by virtue of the inverse connection argument, by someone who holds the broad view. In short, transactionists will make appeals to (partly external) progress and implicitly assume the broad view of scientific inquiry. So the argumentative aim of this chapter provides empirical support for the inverse connection argument, and thereby my overall reconstrual of the science and values debate.

I begin, §5.2, with feminist versions of transactionism, as they are probably the most prominent contemporary versions — as we saw in the last chapter, many isolationists treat feminist scientists and philosophers of science as their primary opponents. I then, §5.3, examine the work of evolutionary biologist Stephen Jay Gould, who was often accused of being some sort of Marxian transactionist. I then turn to three important historical examples. In §5.4 I examine Otto Neurath’s socialist economics. §5.5 deals with the specter of politicized Nazi science — a version of transactionism that most contemporary transactionists would want to repudiate. And, finally, in §5.6, I compare John Dewey’s account of inquiry with my own.

## 5.2 Longino and feminist criticism in the human sciences

### 5.2.1 Longino's contextual empiricism

Helen Longino has developed two distinct yet similar versions of transactionism. In one, ethical and political practices influence the standards of scientific inquiry by way of the “political valence” of these standards — roughly, in a given context, some standards are more likely to promote the internal goods of one ethical or political practice and less likely to promote those of another, and scientists who are also involved in these ethical and political practices may legitimately choose which standards to adopt in part based on this likelihood.<sup>1</sup> Note that this is a version of the no distinction argument for transactionism. While this version is intriguing, I will focus here on the other version, as presented in *Science as Social Knowledge* and *The Fate of Knowledge*.<sup>2</sup>

This second argument depends on Longino's empiricist account of underdetermination.<sup>3</sup> Many of the representations produced at the final stages of scientific inquiry (“theory”) deal with complex entities with an array of properties, many of which cannot be directly observed or interacted with: subatomic quanta, long-extinct organisms, distant galaxies, the neurological activity of living human beings, patterns in the behavior of thousands or millions of citizens in a society. The data supporting these representations, on the other hand, are typically produced through direct interactions with macroscopic objects: meter readings, fossilized bones, images created by fMRIs and telescopes, statistical generalizations from survey results. Connecting the two classes of representations requires “background assumptions” concerning, for example, fossilization and homologies between long-extinct and contemporary organisms. Since these background assumptions are epistemologically prior to the data-theory relation, they are not determined by the data.

Longino identifies “at least” five, “not exclusive” “ways in which values apparently contextual” (roughly, ethical and political values as I have defined them) “can shape the knowledge emerging from” scientific inquiry:

**practices:** Contextual values can affect practices that bear on the epistemic integrity of science.

**questions:** Contextual values can determine which questions are asked and which ignored about a given phenomenon.

**data:** Contextual values can affect the description of data, that is, value-laden terms

<sup>1</sup> Longino, “Gender, Politics, and the Theoretical Virtues”; Longino, “Cognitive and Non-Cognitive Values in Science.”

<sup>2</sup> Longino, *Science as Social Knowledge*; Longino, *The Fate of Knowledge*.

<sup>3</sup> Longino, *Science as Social Knowledge*, ch. 3.

may be employed in the description of experimental or observational data and values may influence the selection of data or of kinds of phenomena to be investigated.

**specific assumptions:** Contextual values can be expressed in or motivate the background assumptions facilitating inferences in specific areas of inquiry.

**global assumptions:** Contextual values can be expressed in or motivate the acceptance of global, frameworklike assumptions that determine the character of research in an entire field.<sup>4</sup>

Over the course of three chapters and in several articles up to the present, Longino presents several case studies from various disciplines — including anthropology, neuroscience, and behavioral psychology — and argues these cases show the influence of ethical and political values on scientific inquiry in all five ways.<sup>5</sup>

One final note: While Longino does include “practices” as one of the five ways, it is by far the vaguest of the items on her list and it receives the least attention in her case studies. It might mean something like “methodology.” She tends to focus instead on the last three items, and recurrently on androcentric or feminist assumptions that influence the production of representational knowledge. Her discussion of research on human evolution, for example, focuses on the particular assumptions made by advocates of the rival “man-the-hunter” and “woman-the-gatherer” views in interpreting archeological data.<sup>6</sup> Thus she works largely — though perhaps not entirely or uncritically — within the narrow view of science as practice.

It may be possible to read this summary of Longino in a way compatible with isolationism. Recall that the characteristic claim of transactionism is that ethical and political values may legitimately influence the *standards* for the internal goods of scientific inquiry — paradigmatically, of course, the standards for representational knowledge. That is, transactionism is about the influence of values on normative epistemology, rather than (“just”) the content of the knowledge itself. It is not obvious that any of the five items on Longino’s list correspond to standards rather than content. Consider data, and empirical adequacy understood very generally as a property of correspondence, coherence, or consistency with some given data. Empirical adequacy is clearly among the standards for representational knowledge. Longino argues that empirical adequacy by itself does not determine the particular set of data against which a representation must be tested.<sup>7</sup> For example, if there are rival methods of

<sup>4</sup> *Ibid.*, 86; the characterizations in this list are hers.

<sup>5</sup> Longino, *Science as Social Knowledge*, chs. 5-7; Helen Longino, “Theoretical Pluralism and the Scientific Study of Behavior,” chap. 19 in *Scientific Pluralism*, ed. Stephen Kellert, Helen Longino, and C. Kenneth Waters, vol. XIX, Minnesota Studies in the Philosophy of Science (University of Minnesota Press, 2006), among others.

<sup>6</sup> Longino, *Science as Social Knowledge*, 104-11.

<sup>7</sup> Longino, “Gender, Politics, and the Theoretical Virtues,” 294-5.

measurement and preliminary dataset analysis that produce incompatible datasets  $D$  and  $E$ , empirical adequacy by itself does not tell us which to use. A model that coheres with  $D$  and not  $E$  is, as such, as empirically adequate as a model that coheres with  $E$  and not  $D$ . At this point, ethical and political values may legitimately influence the decision to use one set of methods, and its consequent dataset, rather than the other. For example, feminist and antiracist values can lead to a preference for interactive and interview-based methods in the social sciences over survey- and questionnaire-based methods.<sup>8</sup> But this choice does not influence empirical adequacy as such; it is a choice of one of the relata —  $D$  rather than  $E$  — not a choice of relation. The influence of ethical and political values on data influences the content of representations, but not, so this reading goes, the standards by which those representations are evaluated.

And likewise with questions and specific and global assumptions. Consider the rival “man-the-hunter” and “woman-the-gatherer” representations of human evolution. Advocates of man-the-hunter deploy a number of assumptions when studying, say, a cluster of similarly chipped stones — that they were deliberately made by humans as tools and that the most important activities for these humans were hunting and the preparation of meat, for example — and use these assumptions to conclude, say, that they were weapons used by hunters for killing and butchering. Longino points out that advocates of woman-the-gatherer make parallel assumptions.<sup>9</sup> Whether these assumptions are taken to be local or global, it seems straightforward that the influence of ethical and political values here is on the content of the resulting representations (the narratives of human evolution) and not on the standards by which those representations are evaluated. The assumption that hunting was so important is an assumption, not a datum drawn from direct experience, and hence there is an important difference between this assumption and the observations of the stones themselves. But the difference does not seem to track a difference between the standards for good representation and the content of a representation.

This is not to say that Longino *must* be read as an isolationist. Rather, by appealing to the standards/content distinction, isolationists may be able to accept many of her claims without falling into logical inconsistency. But, in numerous fields, feminists have challenged specific and global androcentric assumptions in ways that, I think, are best understood as critiques of the standards, not the content, of representational knowledge. Longino’s framework can be used to give an account of this critical work, and thus a feminist transactionism. I will focus here on feminist criticism in archeology and primatology.

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<sup>8</sup> See, for example, Ann Oakley, “Interviewing Women: A Contradiction in Terms,” in *The Gender of Science*, ed. Janet Kourany (Upper Saddle River, NJ: Prentice Hall, 2002), 136–52, ISBN: 0133479722.

<sup>9</sup> See, for example, Longino, *Science as Social Knowledge*, 109.



### 5.2.2 Archeology

Archeology is a particularly interesting case. Alison Wylie points out that, while feminist critiques of archeology date back only to 1984 and “a number of those currently active in the area disavow any explicitly feminist commitments,”<sup>10</sup> there is “widespread interest in the archeology of gender” and “much of the research done under the rubric of the archeology of gender embodies at least a minimal commitment . . . to take women and gender seriously that has resulted in contributions to archeology that are changing its practice, its research agenda, and its understanding of the cultural past.”<sup>11</sup> That is, while there are relatively few feminist archeologists, archeology has been influenced in significant ways by feminism and especially feminist critique.

For example, Denise Donlon has argued that the “implausible preponderance of male specimens” in Australian archeology is due to “systematic errors in sex identification,” which in turn are caused by a “pervasive reliance on measures of ‘robustness’ to determine the sex of skeletal material.”<sup>12</sup> An assumption that men are more “robust” than women was used, not as a piece of data for tests of empirical adequacy, but rather as a method for making and a standard for testing claims about the sex of individual skeletons.

Similarly, a number of archeologists have argued against a tendency to cast men “in the role of catalysts and innovators whenever human agency is invoked to explain the transition from a foraging to a horticultural subsistence base.”<sup>13</sup> To an extent, the criticisms here are that these accounts are empirically inadequate or fail to cohere with accepted accounts of other phenomena: “considering . . . that the change coincides with the adoption of pottery, technology usually attributed to women, an alternative explanation must be considered.”<sup>14</sup> But Wylie suggests that other criticisms deal more with the standards for a good explanation, that there is a tension between assumptions of male agency (that a good explanation is one in which the agents are male) and empirical adequacy or coherence, for example: “Watson and Kennedy say they are ‘leery’ of explanations that remove women from the one domain granted them as soon as an exercise of initiative is envisioned.”<sup>15</sup> That is, there is a tension between two standards, implying that at least one must be modified or rejected.

A deeper critique challenges the assumptions “that particular clusters of attributes are invariably associated with gender categories” and “social roles and relations

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<sup>10</sup> Alison Wylie, “Doing Social Science as a Feminist: The Engendering of Archeology,” in *Feminism in Twentieth-Century Science, Technology, and Medicine*, ed. Angela Creager, Elizabeth Lunbeck, and Londa Schiebinger, Women in Culture and Society (Chicago and London: University of Chicago Press, 2001), 23-4, ISBN: 0226120236.

<sup>11</sup> *Ibid.*, 25, 29.

<sup>12</sup> *Ibid.*, 32.

<sup>13</sup> *Ibid.*, 33.

<sup>14</sup> Sassaman, quoted in *ibid.*, Wylie’s ellipses.

<sup>15</sup> *Ibid.*, 34.

... were structured by the kind of gender segregation familiar from contemporary contexts.”<sup>16</sup> As I read the examples Wylie gives, the target of this critique is a standard for representational knowledge in anthropology that requires the societies represented by those representations to resemble our own, especially in terms of sex and gender roles. This standard is similar to empirical adequacy: a certain relation of resemblance must hold between (at least certain aspects of) the represented society and our own. The critics are not just challenging the particular relations chosen for this representation — as in Longino’s example of the use of feminist values to choose between two datasets — but instead are challenging the standard itself: “if we have learned anything [from] the emerging feminist critique of modern archeology, it is perhaps the danger in viewing gender relationships as static, or at least limited in range”,<sup>17</sup> “it is sometimes necessary to rethink conventional explanatory models quite fundamentally if they are to deal adequately with the insight that sex/gender systems may have taken quite different forms in the past than they do now.”<sup>18</sup> Below, I will argue that these challenges to the explanatory models of archeology are challenges to the standards of representational knowledge in archeology, that is, are challenges to the normative epistemology that was accepted by archeologists at the time. To the extent that these challenges lead to progress and are influenced by feminism, we thus have examples of partly external progress in archeology due to the influence of feminism.

### 5.2.3 Primatology

Like archeologists, “most primatologists vehemently deny that theirs is a feminist science,” yet “many primatologists carry out work that closely adheres to the tenets of feminism.”<sup>19</sup> More specifically, Linda Fedigan argues that eight tools of gender analysis identified by Londa Schiebinger can be utilized to show that “primatologists have become increasingly gender sensitive and gender inclusive over the past twenty-five years.”<sup>20</sup> She discusses her own critique of assumptions of male agency in primatology that look just like those of archeology, and relates it to “a larger move in primatology and anthropology to depict the ‘object of study’ as an actor or agent rather than as a passive resource” and challenges by herself and other feminist primatologists to the “preference for hostile and combative metaphors” and explanations of group dynamics according to the physical power of (typically male) individuals.<sup>21</sup> Her account of

<sup>16</sup> Wylie, “[Doing Social Science as a Feminist](#),” 34.

<sup>17</sup> McGuire and Hildebrandt, quoted in [ibid.](#), Wylie’s brackets.

<sup>18</sup> [Ibid.](#), 38.

<sup>19</sup> Linda Marie Fedigan, “The Paradox of Feminist Primatology: The Goddess’s Discipline?” In *Feminism in Twentieth-Century Science, Technology, and Medicine*, ed. Angela Creager, Elizabeth Lunbeck, and Londa Schiebinger, Women in Culture and Society (Chicago and London: University of Chicago Press, 2001), 46, ISBN: 0226120236.

<sup>20</sup> [Ibid.](#), 48.

<sup>21</sup> [Ibid.](#), 57, 56, 59.

the feminist influence on primatology thus resembles Wylie’s account of the feminist influence on archeology.

Especially interesting for my purposes is Fedigan’s discussion of the work of Jeanne Altmann. In a germinal 1974 paper, Altmann discredited an at-the-time-widespread primatological methodology and simultaneously improved sampling practices by “codify[ing], clarify[ing], and label[ing]” them:

[Her work] discredited for most purposes what is called ‘ad lib sampling’ — the practice of opportunistically recording whatever strikes the observer’s eye and gains one’s attention. In particular, Altmann established that it is inappropriate to use such opportunistic sampling to compare rates of behavior between individual subjects or between males and females, for example.<sup>22</sup>

Recall, from 3.2.2, the distinction between production-dependent and production-independent standards for goods. For representational knowledge, a production-dependent standard is one that depends on the way in which the representation was produced. In other words, a production-dependent epistemological norm is one that depends on how the knowledge was produced. Methodological standards for representations are, of course, production-dependent; thus methodological critiques are critiques of production-dependent standards, and hence, in more general terms, critiques of normative epistemological standards. While Altmann “did state that her involvement in the feminist movement contributed to her awareness of and dissatisfaction with previous androcentric sampling practices” — and thus her critique is an instance of feminist practice influencing the standards for representational knowledge for primatology — she cast her critique entirely in terms of other widely-accepted standards:

Altmann made no mention in this famous publication that she was dissatisfied with the previous bias toward observing male primates more than females — rather, what she did was to convince scientists, through the use of their own methodological tools, to raise their standards of evidence, a task for which she was well qualified by her background training as a mathematician.<sup>23</sup>

#### 5.2.4 Explanatory models as standards

Longino’s work includes resources for an account of these examples of feminist science and their influence on the standards of mainstream (non-feminist) scientific inquiry. Thus, her views should not be interpreted as a moderate version of isolationism on

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<sup>22</sup> [Ibid.](#), 51.

<sup>23</sup> [Ibid.](#)

which ethical and political practices may influence the content of representations but not their standards, as proposed above.

In chapter seven of *Science as Social Knowledge*, Longino discusses “an important class of background assumptions” that she calls “explanatory models.”<sup>24</sup> An explanatory model is a background assumption that constrains “the sorts of items that can figure in explanations of a given sort of phenomenon and . . . the relationships those items can be said to bear to the phenomena being explained.”<sup>25</sup> She illustrates with an example:

For example, in behaviorist psychology explanations must appeal to environmental stimuli as independent variables and treat externally (extensionally) described behavior as the variable dependent on these environmental stimuli. Explanations that describe behavior by means of agents’ intentions or that treat states of consciousness as independent variables do not conform to this model and are ruled out by the behaviorist program.<sup>26</sup>

Explanatory models, I suggest, are background assumptions that are best understood as standards for representations — more specifically, standards for explanations. More formally, define an *explicit explanatory model* to be a set of representations of things and properties that may be used in an explanans; a set of representations of things and properties that may be used in an explanandum; a set of relations between members of these two sets (explanatory relations between explanans and explanandum); and a set of rules for, for example, which representations of things and properties may stand in which positions in the relations of the third set (that is, which roles the representations of things and properties may play in the explanations). An explicit explanatory model may also include sets of representations of things and properties that may *not* be used in an explanans, an explanandum, or both, or these sorts of restrictions may be presented in the set of rules. Any proposed representation that conflicts with the rules given by a given explicit explanatory model is, by the standard of that model, non-explanatory, and in that sense deficient as a representation. For example, if we have an commitment to mechanistic explanations (in the seventeenth century sense, that is, with only push-pull immediate contact interactions of rigid components), any explanation that does not fit this model (say, instantaneous action at a distance) will not count as explanatory.

An *implicit explanatory model* for a given explicit explanatory model comprises a set of examples or templates satisfying the rules of the explicit model. As Longino points out, “in large part . . . explanatory models exist through their exemplars in scientific research design and reports rather than as explicit statements”;<sup>27</sup> that is,

<sup>24</sup> Longino, *Science as Social Knowledge*, 134.

<sup>25</sup> *Ibid.*

<sup>26</sup> *Ibid.*, 135, her parentheses.

<sup>27</sup> *Ibid.*, 135.

scientists generally assess a proposed explanation by comparing it to some of the examples or templates of an implicit explanatory model — which thereby function as exemplars — rather than consulting an explicit explanatory model directly. Of course, any given set of examples or templates is an implicit explanatory model for any of numerous and strictly incompatible explicit explanatory models; this is one important cause of intractable disagreement over what constitutes a good explanation among scientists.<sup>28</sup>

Several of the examples of the influence of feminist scientists discussed above are clearly challenges to androcentric explanatory models. I glossed “androcentric” above as a standard for good explanations: a good explanation is one in which the agents are male. This can be cast easily as an explanatory model: the types of things and properties that may be used in an explanans are males (male animals, human men), the types of things and properties that may not be used include females (female animals, human women), the types of things and properties that may be used in an explanandum are the archeological or primatological phenomena under investigation, and the explanatory relations between these males and the phenomena are what we might call agential-causal relations. This explanatory model, it seems, was mostly implicit, but it still exerted a great deal of normative force. To give an acceptable explanation was to get an androcentric explanation. The various feminist critiques, then, are critiques of then-widely-accepted standards for representational knowledge: the standards given by the implicit explanatory model of male agency lead to empirical inadequacies, depend on demonstrably false assumptions, and depend on flawed methodologies. These critiques — and the work of the feminist joint practitioners who produced them — appear to have led to partly external progress in these fields of scientific inquiry.

Isolationists, however, can resist this conclusion, if they continue to maintain that these implicit explanatory models concern the content, not the standards, of representational knowledge. For example, an isolationist might deny that the implicit explanatory model was androcentric. They might maintain that, at least in principle, both males and females could have stood in the explanans; it was sexist bias that led social scientists to only consider androcentric explanations, not the standards of the explanatory model as such. These examples of progress, isolationists might say, were not improvements in the standards for representations in archaeology and primatology; rather, they were improvements in the content of these representations, and indeed improvements in the sense that these representations better met the requirements given by the purely epistemological, non-androcentric standards. The feminist critiques led scientists to recognize that both males and females could stand in the explanans, not just males.

Again, my aim here is not to settle the debate between isolationists and transactionists. Rather, it is to lay out transactionism as — like isolationism — a coherent and consistent view, and to show the importance of progress to this view. As we

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<sup>28</sup> Longino, “[Theoretical Pluralism and the Scientific Study of Behavior.](#)”

see here, the specific kind of progress can be important. If we make much of the distinction between internal goods and their standards — between representations and normative epistemology — it's at least logically possible to maintain that even the examples from this section did not amount to really radical progress.

### 5.3 Stephen Jay Gould: Marxism and evolutionary biology

The contributions of paleontologist and evolutionary biologist Stephen Jay Gould may also be a case of partly external progress due to joint practice. Unlike some of the other case studies, however, a transactionist interpretation of Gould's work is highly problematic. While I cannot yet make a solid case, and hence my interpretation is at best preliminary, in this section I identify the major problems for a transactionist interpretation of Gould's work, and indicate how they might be addressed. To the extent that this interpretation and defense is plausible and can be sufficiently developed in the ways that I indicate, it is plausible to consider Gould's work an example of partly external progress.

#### 5.3.1 Gould as transactionist? Some preliminary considerations

The basic case for a transactionist interpretation of Gould is built on one section of one of his earliest papers — coauthored with Niles Eldredge — on the punctuated equilibrium model of evolutionary change.<sup>29</sup> Most of this paper is a technical discussion of the weaknesses of the then-prevalent gradualist model — on which speciation occurs slowly, over long spans of geological time — and the strengths of their somewhat novel punctuationalist model — on which speciation occurs over relatively short, “geologically instantaneous” spans of time.<sup>30</sup> In the (in)famous and penultimate section V, however, Gould and Eldredge turn to what can only be called historical, sociological, and philosophical reflections on the ways in which “even the greatest scientific achievements are rooted in their cultural contexts”.<sup>31</sup> They first discuss Darwin's gradualist model and cite mainstream historians and biographers who relate this gradualism to “English political conservatism.”<sup>32</sup> They then turn to

<sup>29</sup> Stephen Jay Gould and Niles Eldredge, “Punctuated Equilibria: The Tempo and Mode of Evolution Reconsidered,” *Paleobiology* 3, no. 2 (Spring 1977): 115–51.

<sup>30</sup> *ibid.*, 115. As Shermer points out, the central working parts of the punctuationalist model were first developed by Mayr in the 1950s. Michael Shermer, “The Punctuated Politics of Stephen Jay Gould: Science and Culture in Evolutionary Theory,” *Rethinking Marxism* 15, no. 4 (October 2003): 505

<sup>31</sup> Gould and Eldredge, “Punctuated Equilibria,” 145.

<sup>32</sup> *Ibid.*, 145, quoting Irvine.

punctuationalism in Marxism and its descendants, paraphrasing Engels (without citation, but presumably to the philosophy of science that he presents in *Anti-Dühring*<sup>33</sup>) and quoting the “official Soviet handbook of Marxism-Leninism” before finally posing a rhetorical question: “May we not also discern the implicit ideology in our Western [as opposed to Soviet] preference for gradualism?”<sup>34</sup>

To this point, Gould and Eldredge have not said anything especially controversial or outlandish, especially in the middle of what might be called the early years of transactionist historiography of science. What is interesting — and will later be the topic of much controversy — is the reflective discussion on their own rootedness that follows. It will be helpful to have this complete discussion available, omitting only some of the details that will not be relevant for my analysis. Immediately after posing the rhetorical question quoted at the end of the last paragraph, Gould and Eldredge start a new paragraph:

In the light of this official [Soviet] philosophy, it is not at all surprising that a punctuational view of speciation, much like our own, but devoid [of certain particular features] . . . has long been favored by many Russian paleontologists . . . . It may also not be irrelevant to our personal preferences [for punctuationalism] that one of us [Gould] learned his Marxism, literally at his daddy’s knee.

The punctuational view is also congenial with some important trends of Western thought during the twentieth century. Information theory . . . provides a goldmine of metaphor for advocates of punctuationalism.

We emphatically do not assert the ‘truth’ of this alternate metaphysic of punctuational change. Any attempt to support the exclusive validity of such a monistic, a priori, grandiose notion would verge on the nonsensical. We believe that gradual change characterizes some hierarchical levels, even though we may attribute it to punctuation at a lower level . . . . We make a simple plea for pluralism in guiding philosophies — and for the basic recognition that such philosophies, however hidden and inarticulated, do constrain all our thought.<sup>35</sup>

The rest of the section is a discussion of punctuationalist models of change in other disciplines, such as mathematician René Thom’s catastrophe theory, a punctuationalist model in ecology attributed to H.L. Carson, and a model of geological strata attributed to D.V. Ager.

In the intervening decades, numerous critics of punctuated equilibrium and Gould in general have used the “daddy’s knee” sentence, often quoted in isolation, to argue that, as Gould puts it, he is “unconcerned with scientific truth, but motivated by

<sup>33</sup> Friedrich Engels, *Anti-Dühring: Herr Eugen Dühring’s Revolution in Science* (1878; Moscow: Foreign Languages Publishing House, 1962), especially chs. 6-8.

<sup>34</sup> Gould and Eldredge, “Punctuated Equilibria,” 146.

<sup>35</sup> [ibid.](#)



some ulterior (and nefarious) goal,” such as “foisting Marxism upon an unwitting public.”<sup>36</sup> The arguments and rhetoric used to make these charges, unsurprisingly, are thoroughly isolationist. In short, the threat of domination looms large in many critical discussions of Gould’s work.

### 5.3.2 The isolationist response

As best as I can tell, responses to this attack by both Gould and his third-party defenders generally do not attempt to defend transactionism; instead, they either deny outright or attempt to mitigate the influence of Gould’s ethical and political values (or any other form of participation in ethical and political practices) on either the content of punctuated equilibrium or the standards by which it is evaluated. Call this the *isolationist response* to the threat of domination, for its apparent assumption of isolationism. *This is the first problem with a transactionist interpretation of Gould:* as we’ll see in passing in §6.2, he himself appears to embrace a moderate version of isolationism.

Historian of science and public intellectual Michael Shermer has presented five responses to the charge of institutional domination in Gould’s work, including several isolationist ones. I’ll briefly quote Shermer’s responses and discuss each in turn.

- (1) “*Gould was not a Marxist . . .* As he explained . . . , ‘I spoke only about a fact of my intellectual ontogeny; I said nothing about my political beliefs (very different from my father’s, by the way).’”<sup>37</sup>

*The second problem with a transactionist interpretation of Gould* is that it is not clear in just which ethical and political practices he might have participated. An obituary in a leftist magazine highlights his participation in an assortment of “left-wing causes during his whole life,” including active involvement in Science for the People,<sup>38</sup> but I have not found either an autobiography or an authoritative biography that discusses this in sufficient detail to support a transactionist interpretation.

Nonetheless, even if punctuated equilibrium was not the product of joint practice involving Marxism, evolutionary biology, and paleontology, Gould and Eldredge’s discussion of the context in which punctuated equilibrium was developed, combined

<sup>36</sup> Stephen Jay Gould, *The Structure of Evolutionary Theory* (Harvard University Press, 2002), 1017, 984. For examples of this critique, see *ibid.*, 1017; Ullica Segerstråle, *Defenders of the Truth: The Sociobiology Debate* (Oxford and New York: Oxford University Press, 2001), 344; and a brief summary of E.O. Wilson’s responses to Gould and Lewontin’s critiques of sociobiology, in Michael Yudell and Rob Desalle, “Sociobiology: Twenty-Five Years Later,” *Journal of the History of Biology* 33, no. 3 (2000): 582.

<sup>37</sup> Shermer, “[The Punctuated Politics of Stephen Jay Gould](#),” 507, Sherman’s emphasis, quoting Gould.

<sup>38</sup> Phil Gasper, “Stephen Jay Gould: Dialectical Biology,” *International Socialist Review* 24 (July-August 2002).

with Gould's activism, does suggest the possibility that punctuated equilibrium was the product of joint practice involving evolutionary biology, paleontology, and some *other* leftist ethical or political practices. Dismissing the transactionist interpretation *in general* simply because Gould wasn't specifically a *Marrist* is too hasty.

- (2) "*Niles Eldredge was not a Marrist.* Even if Gould was a Marxist the theory was primarily Niles Eldredge's, heavily influenced by Ernst Mayr, neither one of whom was a Marxist."<sup>39</sup>

This response is too hasty in the same way the last response was too hasty: that the development of punctuated equilibrium wasn't influenced by Marxism doesn't imply that it wasn't influenced by any other ethical or political practices. Further, even if Eldredge and Mayr were completely and utterly uninvolved in any other practices (I know nothing of their political activities or views), Gould still played *some* role in the development of punctuated equilibrium, and thus may still have been a joint practitioner.

- (3) "*We must separate the sociological aspects of science from its empirical nature.* It is good to know if a theory has sociopolitical origins and implications, but that is orthogonal to the matter of its validity."<sup>40</sup>

Gould himself gave this same response: "We raised this point as sociological commentary about the *origin* of ideas, not as scientific argument for the *validity* of the same ideas."<sup>41</sup> That is, both Gould and Shermer appear to be appealing to a version of the distinction between context of discovery and context of justification: while Gould's participation in some ethical and political practices may have helped him initially develop the punctuationist model of speciation and identify shortcomings of the gradualist model, they certainly didn't provide any justification or warrant to the punctuationist model — they didn't, that is, contribute to its excellence as representational knowledge.

But, as discussed in §3.2.2, the discovery/justification distinction is problematic on any view of scientific inquiry as a practice, and is rejected by many science studies scholars from a variety of disciplines. Thus, this defense is *at least* problematic, if not completely inadequate on its face.<sup>42</sup>

- (4) "*Gould's emphasis on contingency in evolution is anti-Marrist . . . .* Contingency not only subverts evolutionary determinism but negates economic determinism, the very foundation of Marxist ideology."<sup>43</sup>

<sup>39</sup> Shermer, "The Punctuated Politics of Stephen Jay Gould," 507, his emphasis.

<sup>40</sup> *Ibid.*, 507, his emphasis.

<sup>41</sup> Gould, *The Structure of Evolutionary Theory*, 1017, his emphasis.

<sup>42</sup> Indeed, I'm surprised that Shermer, a professional historian of science who earned his Ph.D. in 1991, would use it so uncritically.

<sup>43</sup> Shermer, "The Punctuated Politics of Stephen Jay Gould," 508, his emphasis.

This response has the same problem as the first two responses: that the development of punctuated equilibrium wasn't influenced by Marxism doesn't imply that it wasn't influenced by any other ethical or political practices. In addition, the content of the response itself is quite odd, as economic determinism — whatever its status or function in Marx's own work and granting that it was part of the dogma of Marxism-Leninism — was not universally accepted within the Western Marxist tradition of the late twentieth century, much less taken to be “the very foundation” of contemporary versions of Marxism. On the continental side, Ernesto Laclau and Chantal Mouffe distinguish two versions of “economism” or economic determinism, and reject both.<sup>44</sup> On the analytic side, G.A. Cohen limits his discussion of determinism in *Karl Marx's Theory of History* to a single footnote: “the issue of determinism will not be discussed in this book.”<sup>45</sup> Furthermore, influential anti-determinist Marxist views can be found in the generation prior to Gould. For example, physicist David Bohm argued for a version of non-reductive materialism in which chance and “approximate and relative” autonomy played an essential role.<sup>46</sup>

- (5) “*Metaphors of change do not necessarily translate across disciplines . . . .* Metaphors are heuristic tools for conceptual explanation, not precise descriptions of actual events in nature . . . . The fact that theories from two different disciplines vaguely resemble one another tells us little about the internal workings of the theories (or theorists) themselves.”<sup>47</sup>

I find this response quite difficult to understand. Shermer may be arguing that borrowing a metaphor from one discipline for use in another — such as borrowing metaphors of economic and social change for use in evolutionary biology — doesn't involve any commitments to the legitimacy of the metaphor in the source discipline. He gives an example of Darwin, borrowing Smith's invisible hand metaphor as a heuristic for the process of natural selection. The suggestion, I think, is that Darwin did not thereby commit himself to Smithian laissez-faire economics; nor, more generally, is anyone who accepts a Darwinian account of natural selection (gradualist or otherwise) thereby committed to Smithian laissez-faire economics. Similarly, Shermer quotes Gould, borrowing some metaphors and discussion from Marx in order to illustrate “the interaction between contingencies and necessities, and the nonrepeatability of historical systems”;<sup>48</sup> the suggestion, then, is that neither Gould nor anyone else

<sup>44</sup> Ernesto Laclau and Chantal Mouffe, *Hegemony and Socialist Strategy: Towards a Radical Democratic Politics*, second edition (1985; Verso, 2001), first edition published 1985.

<sup>45</sup> G.A. Cohen, *Karl Marx's Theory of History: A Defence*, expanded edition (Princeton University Press / Oxford University Press, 2001), 147n1, ISBN: 0691070687.

<sup>46</sup> David Bohm, *Causality and Chance in Modern Physics* (1957–1961; Philadelphia: University of Pennsylvania Press), ISBN: 0812210026.

<sup>47</sup> Shermer, “[The Punctuated Politics of Stephen Jay Gould](#),” 508.

<sup>48</sup> *Ibid.*, 509, not a quotation from Gould.

is thereby committed to, say, Marx’s account of interactions between the system of production and civil society.

If my reading of this response is accurate, then it (the response) seems far too simplistic. Darwin didn’t just use a somewhat convenient metaphor that, as it were, he happened to come across somewhere or another. He was an avid reader of economics and political theory, and reading Malthus’s *Essay on the Principle of Population* in 1838 played a crucial role in the development of the theory of natural selection.<sup>49</sup> Similarly, as Shermer himself concedes,

There is no denying that Gould’s leftist upbringing influenced his style of thought, as evidenced by the fact that Karl Marx makes frequent appearances in his writings. For example, I found Marx’s famous opening from *The Eighteenth Brumaire* quoted no less than seven times.<sup>50</sup>

Gould clearly studied Marx and other leftist political thinkers carefully, and not just because Marx happened to offer Gould some convenient metaphors. In Gould’s case, at least, this suggests an influence of the appropriate kind and degree to constitute joint practice and partly external progress.

### 5.3.3 Gould as transactionist

Thus far, I have only discussed what I have called the isolationist response to the charge against Gould. An alternative response is to read Gould as some sort of transactionist, and in particular an instance of partly external progress brought about by a joint practitioner. In the remainder of this section, I briefly sketch this sort of transactionist response.

Recall, from §5.2, Longino’s conception of explanatory models. As I argued there, explanatory models are best understood as standards for explanation, and thus are standards for representational knowledge. Furthermore, we saw several cases of what appear to be partly external progress in scientific inquiry brought about by feminist scientist joint practitioners levelling challenges to then-dominant androcentric or otherwise sexist explanatory models that are, at the same time, “mainstream” and “feminist.”

The challenge to gradualism made by Gould and Eldredge can likewise, I argue, be best understood in terms of challenging a then-dominant explanatory model. In

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<sup>49</sup> See, among numerous other analyses, Jonathan Hodge, “The Notebook Programmes and Projects of Darwin’s London Years,” in *The Cambridge Companion to Darwin*, second edition, ed. Jonathan Hodge and Gregory Radick (Cambridge, UK: Cambridge University Press, 2009), 64-71 and Gregory Radick, “Is the Theory of Natural Selection Independent of Its History?” In *The Cambridge Companion to Darwin*, second edition, ed. Jonathan Hodge and Gregory Radick (Cambridge, UK: Cambridge University Press, 2009), 163-4.

<sup>50</sup> Shermer, “[The Punctuated Politics of Stephen Jay Gould](#),” 509.

their abstract, Gould and Eldredge call gradualism “an a priori bias” and claim that it “has precluded any fair assessment of evolutionary tempos and modes. It could not be refuted by empirical catalogues constructed in its light because it excluded contrary information as the artificial result of an imperfect fossil record.”<sup>51</sup> Early in their paper, they develop this charge:

The empirical distribution of evolutionary tempos is as fundamental a datum to our profession [paleontology] as amounts of variability are to genetics . . . . Paleontologists have worn blinders that permit them to accumulate cases in one category only: they have sought evidence of slow, steady and gradual change as the only true representation of evolution in the fossil record . . . . Two other classes of information were explained away or simply ignored . . . . Paleontologists allowed a potent, historical bias to direct their inquiry along a single path, though they could have accumulated other data at any time.<sup>52</sup>

Similarly, later, they write

An a priori bias toward gradualism as the only ‘true’ evolutionary event continues to preclude any fair test for relative frequency among the possible tempos of evolution. At worst, it dictates an erroneous interpretation of major evolutionary events.<sup>53</sup>

Bracketing the hyperbole,<sup>54</sup> it is clear that Gould and Eldredge see themselves as challengers of a background assumption of gradualism that — much like the background assumptions Longino and other feminists identified in the human sciences — had been used uncritically by evolutionary biologists in the interpretation of observations (paleontological finds, for example) and the development of representational knowledge. In claiming that gradualism is “a priori” and “could not be refuted by empirical catalogues,” Gould and Eldredge assert that gradualism is genuinely an assumption, not any sort of inductive or otherwise empirically-grounded generalization. Further, and again much like the background assumptions identified by feminists, gradualism serves as an explanatory model — a set of restrictions and rules for constructing explanations, in this case explanations of phyletic change. Gradualism, as an explanatory model, requires that the evolutionary relations posited between species be, roughly, a smooth transition over geologic timescales. Gould and Eldredge’s punctationism introduces a rival explanatory model, allowing transitions between species to be relatively abrupt over geologic timescales.

<sup>51</sup> Gould and Eldredge, “Punctuated Equilibria,” 115.

<sup>52</sup> *Ibid.*, 116.

<sup>53</sup> *Ibid.*, 117.

<sup>54</sup> Note that their charges are not *just* hyperbole, despite the impressions my quotations may give. As mentioned above, most of the article is a technical — and rather dry — discussion of difficulties created by the assumption of gradualism.

Recall from the long quotation from section V that Gould and Eldredge “do not assert the ‘truth’ of this alternate metaphysic of punctuational change.”<sup>55</sup> They do not propose to replace an uncritical assumption of gradualism with an uncritical assumption of punctuationalism. Rather, they “make a simple plea for pluralism in guiding philosophies” and recognize the possibility that gradualist explanatory models are more appropriate than punctuationalist ones for “some hierarchical levels.” Thus, in the same way that the adoption of nonsexist alternatives to androcentric explanatory models constituted changes — and indeed progress — with respect to standards of representational knowledge, the adoption of punctuationalism (especially the moderate pluralist version) in place of (dogmatic) gradualism constituted a change — and indeed progress — with respect to standards of representational knowledge. In short, Gould and Eldredge’s work at least arguably constituted progress with respect to the standards for representational knowledge in evolutionary biology.

The final step in this transactionist interpretation of Gould’s work is to show that this case of progress was in fact *partly external* progress, the product of Gould’s joint practice. Given Gould’s political activities and the similarities between his challenge to gradualism and the feminist challenges to androcentric explanatory models, this final step is at least plausible. But I have already identified at least two serious problems with it. First, he himself explicitly endorses isolationism, albeit a moderate version that, insofar as it presupposes a context of discovery/context of justification distinction, is philosophically untenable. Second, it’s not clear in just which ethical and political practices, if any, he participated, and thus, third, it’s especially unclear how his participation in ethical and political practices might have contributed to the identification of, and challenge to, the gradualist background assumption.

The most straightforward response to the first problem is a simple error theory: while Gould was extremely interested in the history of philosophy of science,<sup>56</sup> he was not a trained philosopher, and he simply failed to understand accurately his own joint practice. This error theory also explains his failure to recognize the problems with the simplistic context of discovery/context of justification response to the charge of bias. This presupposes, however, that his work actually was a case of joint practice, and thus presupposes a response to the second and third problem. But the problem here is a lack of historical evidence, not the existence of contradictory evidence; as I have already said, developing this transactionist interpretation of Gould’s work requires further historical research.

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<sup>55</sup> See above, quotation at n. 35.

<sup>56</sup> Shermer, “[The Punctuated Politics of Stephen Jay Gould](#),” 511.



## 5.4 Otto Neurath: Physicalism, socialism, and the foundations of economics

In this section, I consider the work of Otto Neurath. In the first half of the twentieth century, Neurath was an economist, public educator, socialist activist, and logical empiricist. Over the last 15 years or so Neurath has received considerable attention by historians of philosophy of science as a member of the previously-neglected “Left Vienna Circle,” a precursor to contemporary transactionism, and a pivotal figure in the Steel Belt narrative of the history of twentieth-century philosophy of science. In the first subsection, I consider an objection to this recent work on Neurath, from feminist historian of science Sarah Richardson. While I find Richardson’s objection unsupported by the evidence that she offers, I believe she does legitimately raise the question of the value of Neurath for contemporary transactionists. Then, in the second subsection, I offer my answer to this question by examining Neurath’s early work on the foundations of economics. I argue that Neurath was a joint practitioner in three practices — logical empiricism, movement socialism, and economics — and that his view of the aims of economics and its relationship to socialism are similar to the “basic” version of transactionism that I present in §6.3. While I do not believe that Neurath offers anything like a philosophical theory of transactionism, he does offer contemporary transactionists some important examples and models.

### 5.4.1 Neurath as Neutral Marxist?

In recent work, feminist historian of science Sarah Richardson has argued that Neurath did not develop a political philosophy of science, and the attention devoted to “recovering” Neurath marginalizes the more significant contributions of feminism — the kinds of contributions discussed in §5.2 — to scientific inquiry and philosophy of science.<sup>57</sup> Since the argument for the second conjunct depends on the first conjunct, I will deal with only the first here.

Richardson’s critical discussion of Neurath can be divided into two parts. In the first, she argues that, while Neurath was a Marxist, he was a politically-disengaged, “Neutral Marxist.”<sup>58</sup> Richardson presents Max Weber’s isolationist conception of “value-neutrality”; on this conception, “the social scientist may legitimately cite ‘value-relevance’ in the selection of research problems” but must remain “value-neutral” in her methods and empirical data.<sup>59</sup> Neutral Marxism provided some basic concepts and motivation for social scientific inquiry, but was otherwise thoroughly iso-

<sup>57</sup> Sarah Richardson, “The Left Vienna Circle, Part 1: Carnap, Neurath, and the Left Vienna Circle Thesis,” *Studies in the History and Philosophy of Science Part A* 40, no. 1 (March 2009): 14–24; Richardson, “The Left Vienna Circle, Part 2.”

<sup>58</sup> Richardson, “The Left Vienna Circle, Part 1,” 19–22.

<sup>59</sup> *Ibid.*, 20.



lationalist and politically-disengaged. Following historian Robert Proctor,<sup>60</sup> Richardson attributes this conception of value-neutrality to Neurath: “More likely, Neurath conceived of Marxism as a theoretical source for social analysis fully consistent with Weberian value-neutrality.”<sup>61</sup> Call this claim *the attribution*.

Richardson provides very little evidence to support the attribution; indeed, most of her citations seem to be to evidence that challenges the attribution. She does, however, cite two pieces of evidence that seem to support her claim. First there is a discussion by Thomas Uebel of Neurath’s views prior to the First World War: “In particular Neurath applauded [Tönnies’s, Simmel’s, and Weber’s] conception of the value-neutrality of science.”<sup>62</sup> However, this is the only sentence that I can find at this citation that supports the attribution. The surrounding several pages say nothing else about science and values. Furthermore, this sentence itself does not give any supporting citations. As we shall see in the next subsection, even if Neurath did accept this sort of view prior to the First World War, he rejected it very shortly afterwards.

Second, there is Robert Proctor’s discussion of Neutral Marxism; Proctor gives Neurath as his prime example of a Neutral Marxist.<sup>63</sup> However, Proctor cites only two works by Neurath, *Empirical Sociology* and “Sociology in the Framework of Physicalism,” both of which deal more-or-less exclusively with methodology in social scientific inquiry, not the relationship between social scientific inquiry and, for example, socialism.<sup>64</sup> Notably, neither Richardson nor Proctor show any familiarity at all with Neurath’s work on economic planning or the foundations of economics, or for that matter with his polemical, Marxist pamphlets — all of which will be my evidence in the next subsection. While the methodological pieces cited by Richardson and Proctor — when read out of context — can be taken to be consistent with Weberian value-neutrality, I show in the next subsection that this is not the case when these pieces are put into the context of Neurath’s earlier works. So, when this is done, the attribution does not seem to be supported by the evidence that Richardson adduces for it.

In the second part of her criticism of Neurath,<sup>65</sup> Richardson analyses the exchange with Horace Kallen published in *Philosophy and Phenomenological Research* shortly

<sup>60</sup> Robert Proctor, *Value-Free Science?: Purity and Power in Modern Knowledge* (Cambridge, MA and London: Harvard University Press, 1991).

<sup>61</sup> Richardson, “[The Left Vienna Circle, Part 1](#),” 21.

<sup>62</sup> Nancy Cartwright et al., *Otto Neurath: Philosophy between Science and Politics* (Cambridge, UK: Cambridge University Press, 1996), 98.

<sup>63</sup> Proctor, *Value-Free Science?* 199, 122, and 169.

<sup>64</sup> Otto Neurath, “Empirical Sociology: The Scientific Content of History and Political Economy,” chap. 10 in *Empiricism and Sociology*, ed. Marie Neurath and Robert Cohen (1931; Dordrecht: D. Reidel, 1973), 319–421, ISBN: 9027702586; Otto Neurath, “Sociology in the Framework of Physicalism,” in *Philosophical Papers 1913-1946*, ed. Robert Cohen and Marie Neurath (1931; Dordrecht: D. Reidel, 1983).

<sup>65</sup> Richardson, “[The Left Vienna Circle, Part 1](#),” 22-3.

after Neurath's death,<sup>66</sup> calling it “the most widely-cited, clearest, and extensive development of Neurath's position” on science and values. Richardson does not cite any LVC historiographers citing this exchange, and it does not appear to be cited much in the pieces by LVC historiographers that she cites,<sup>67</sup> but I will suppose that it is as important as she says. Richardson extracts two criticisms of Neurath from Kallen's contributions:

- (1) “To be successful, [Neurath's] ‘unified science’ would require the introduction of undesirably authoritarian forms of organization into science.”
- (2) “This ‘scientific totalitarianism’ emboldens forms of “political totalitarianism.”<sup>68</sup>

I grant that this is an accurate reconstruction of Kallen's position. Indeed, it seems to me that Neurath's contributions are, on the whole, responses to the first criticism.<sup>69</sup> Richardson's rejoinder, though, is that Neurath has not adequately responded to “the challenge of Nazi science raised by Kallen.”<sup>70</sup> That is, he has not provided a systematic political philosophy of science that can generate an adequate theoretical criticism of scientific inquiry as practiced under the Nazi regime.<sup>71</sup>

Richardson's rejoinder seems to me to be at least a non sequitur, and possibly an anachronism. Kallen's criticisms are not that Neurath's position (somehow) endorses Nazi science; nor does responding to these criticisms require condemning Nazi science. It is accurate to call the organization of scientific inquiry under the Nazis “totalitarian”; hence, if Kallen's first criticism were successful, it would imply that unified science, like Nazi science, involves “scientific totalitarianism.” But rejecting this comparison simply requires responding to the first criticism — which Neurath does — and not criticizing Nazi science as such.

So then why does Richardson think that Neurath should have criticized Nazi science? One possibility is that Richardson is projecting a contemporary desideratum

<sup>66</sup> “A Discussion of the Unity of Science,” *Philosophy and Phenomenological Research* 6, no. 4 (June 1946): 3 contributions by Kallen, 2 by Neurath, 1 by Charles Morris, and a memorial postscript on Neurath's death by Kallen.

<sup>67</sup> In particular, it does appear to be not cited at all in the works by Haller, Howard, Nemeth, and Stadler listed in Richardson's bibliography. It is cited by two of the pieces by Uebel, but does not seem to make a crucial contribution to his argument in either case.

<sup>68</sup> Richardson, “[The Left Vienna Circle, Part 1](#),” 22.

<sup>69</sup> Compare Neurath's own summary of his first contribution, Otto Neurath, “The Orchestration of the Sciences by the Encyclopedism of Logical Empiricism,” *Philosophy and Phenomenological Research* 6, no. 4 (June 1946): 508.

<sup>70</sup> Richardson, “[The Left Vienna Circle, Part 1](#),” 23.

<sup>71</sup> For a brief discussion of some of the important features of Nazi science — including my answer to the challenge — see §5.5. For a fuller discussion, see Robert Proctor, *Racial Hygiene: Medicine under the Nazis* (Cambridge, MA: Harvard University Press, 1988) and the citations given in §5.5.

for a political philosophy of science back onto Neurath. As we saw in chapter 4, contemporary isolationists are extremely concerned about the threat of institutional domination; comparisons with Nazi science are common in this context; and thus contemporary transactionists have a need for a political philosophy of science that can respond to “the challenge of Nazi science.” Any political philosophy of science that cannot do this is, in that respect, lacking for contemporary purposes. Finally, granting that Neurath’s political philosophy of science cannot do this, it follows that it is lacking for contemporary purposes.

This last criticism, however, is anachronistic as such. Neurath is pursuing unified science for his purposes, not ours. And his aims are clearly pragmatic: as the Vienna Circle Manifesto puts it, the aim of the logical empiricist movement is “to fashion intellectual tools for everyday life, for the daily life of the scholar but also for the daily life of all those who in some way join in working at the conscious re-shaping of life.”<sup>72</sup> His reflections on scientific methodology are meant to stimulate progress in the practice of scientific inquiry by making it more useful (and thereby to stimulate indirectly progress in society), not to be excellent as representational knowledge. For most of his life, Neurath is an activist-intellectual, not an academic, and especially not a professional philosopher. By contrast, contemporary participants in the science and values debate are, for the most part, professional academics, primarily interested in historical and philosophical accounts — representations — of the interactions of scientific inquiry and other practices and institutions. For these contemporary academics, the epistemic aims are primary; for Neurath, the pragmatic aims are primary. It is therefore no criticism of Neurath’s work — at least, with respect to *his own* aims — to point out that it is not a systematic, philosophical account of Nazi science.

On the other hand, Richardson’s criticism does lead to a legitimate question: how relevant are Neurath’s reflections on science and values to contemporary, academic issues, debates, and concerns? That is, how relevant is Neurath’s work to *our* aims? This question is best answered by presenting some of Neurath’s work and relating it to some contemporary issue. Uebel does this primarily by drawing underdetermination arguments out of Neurath’s work.<sup>73</sup> But, as I argued in §4.3, underdetermination arguments are quite limited as resources for contemporary transactionism, and this is precisely because they are unable to address the kinds of challenges that Richardson presents.<sup>74</sup> Furthermore, I believe that Uebel’s approach risks misrepresenting (in

<sup>72</sup> Otto Neurath, Rudolf Carnap, and Hans Hahn, “The Scientific Conception of the World: The Vienna Circle,” chap. 9 in *Empiricism and Sociology*, ed. Marie Neurath and Robert Cohen (1929; Dordrecht: D. Reidel, 1973), 305, ISBN: 9027702586.

<sup>73</sup> See, e.g., Thomas Uebel, “Political Philosophy of Science in Logical Empiricism: The Left Vienna Circle,” *Studies in the History and Philosophy of Science A* 36, no. 4 (2005): 758-60, doi:[10.1016/j.shpsa.2005.08.014](https://doi.org/10.1016/j.shpsa.2005.08.014).

<sup>74</sup> Don Howard correctly points out that on, Neurath’s version of underdetermination, the effects of the influence of various “auxiliary motives” — roughly, non-epistemic procedures and reasons for choosing one theory over its rivals, including pragmatic aims

light of the line of thought of the last paragraph) Neurath as a philosopher rather than a social scientist and activist with a philosophical bent.

### 5.4.2 Neurath as economist, socialist, and methodologist

In light of these problems, I suggest approaching Neurath as a social scientist and activist first and foremost, and therefore looking more carefully at his contributions to the methodology of the social sciences and movement socialism than is usually done in the literature on the Left Vienna Circle. Taking this approach, I argue that Neurath can be understood as a joint practitioner, whose work draws on the practices of both movement socialism and logical empiricism to (attempt to) stimulate progress in social scientific inquiry.

A number of Neurath's early works on the foundations of economics, economics in kind, war economics, and the economic administration of a socialist society have been translated and published in a single collection.<sup>75</sup> In the context of Neurath's life, the writing of these roughly coincide with the time including his short-lived academic career (1907-1914); the first World War; his participation in the economic administration of the Bavarian Soviet Republic (*Bayerische Räterepublik*) (1919); the collective housing movement in Red Vienna (1919-1924); and the formation of the Second Vienna Circle around Moritz Schlick (starting c.1922).

In the earliest of these pieces, Neurath criticizes the "narrow delimitation of the concept of income" as wealth or money that is accepted by economists and social scientists.<sup>76</sup> His criticism appeals to what we would call the incommensurability of

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and ethical and political values — are subject to empirical, sociological investigation. (personal communication) Such an investigation is necessary for any empiricist response to the concerns raised by Anderson and Steel about underdetermination arguments, and in this respect Neurath's version of underdetermination is superior to, for example, purely logical versions. But claims that the influence of Nazi values has such-and-such effects while the influence of socialist values has thus-and-so effects do not tell us how to evaluate and compare the two sets of effects. For this we need what Dewey called a theory of valuation, and I follow Dewey in thinking that the noncognitivist metaethics of logical empiricists — including Neurath — preclude the development of an adequate such theory. For Dewey's critique, see John Dewey, *Theory of Valuation*, in *John Dewey: The Later Works, 1925–1953*, ed. Jo Ann Boydston, vol. 13: 1938–1939 (Carbondale and Edwardsville: Southern Illinois University Press, 1988), 189–251, ISBN: 9780809314256; for Neurath's noncognitivism, see Joshua Stuchlik, "Felicology: Neurath's Naturalization of Ethics," *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 1, no. 2 (Fall 2011): 189,204, <http://www.jstor.org/stable/10.1086/659399>.

<sup>75</sup> Thomas Uebel and Robert Cohen, eds., *Economic Writings: Selections 1904-1945* (Dordrecht: Kluwer Academic Publishers, 2004), ISBN: 1402022735.

<sup>76</sup> Otto Neurath, "Interventions in Discussions of the Social Policy Association [*Verein für Sozialpolitik*]: Selections 1904-1945," chap. 8 in *Economic Writings*, ed. Thomas Uebel

different goods:

Suppose a civil servant has the choice between two places of residence, A and B. In A, he receives a larger quantity of food and accommodation, in B on the other hand a larger quantity of honour. Is it possible to have a calculus such that it summarises for us food and accommodation as one magnitude, and honour as another? Impossible! We are not able to compute such a complex, containing both pleasure and pain, by first separately establishing the magnitude of pleasure, then the magnitude of pain and finally doing the sum. On the contrary, we can only look at such a complex as a whole. Therefore the conversion into money is of no help in this case.<sup>77</sup>

That is, there are a variety of goods — in the long quotation, food, housing, and honor or status — and we cannot compare them by measuring them all in some common unit, monetary or otherwise. Put another way: there is no general way — using money, utility, socially necessary labor,<sup>78</sup> or any other one-dimensional metric — to compare the benefits of a greater quantity of food and a nicer home (in A) with the benefit of higher status (in B), and thus no way to ask whether the benefits of A are “greater than” the opportunity costs of not taking B.<sup>79</sup>

Despite similarities to arguments given by John Hicks and Nicholas Kaldor in the 1930s,<sup>80</sup> Neurath’s problem is not with *interpersonal* comparisons of well-being. Kaldor and Hicks generally assume that, for a given individual, the amount of pleasure she receives (or would receive) from taking A can be compared to the amount of pleasure she receives (or would) from taking B; their problem is with comparing the amount of pleasure individual *a* receives from the amount of pleasure another individual *b* receives, even in one and the same situation. Neurath’s point, rather, is that the *kinds* of pleasure a *given* individual receives from food and accommodation are different from the *kinds* of pleasure *the same individual* receives from status. There is no common unit in which these three (at least) kinds of pleasure can be

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and Robert Cohen (1910; Dordrecht: Kluwer Academic Publishers, 2004), 292, ISBN: 1402022735.

<sup>77</sup> [Ibid.](#), 293.

<sup>78</sup> Otto Neurath, “Economic Plan and Calculation in Kind: On the Socialist Order of Life and the Human Beings of the Future,” chap. 13 in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (1925; Dordrecht: Kluwer Academic Publishers, 2004), 435, ISBN: 1402022735; cf. G.A. Cohen, “The Labor Theory of Value and the Concept of Exploitation,” *Philosophy and Public Affairs* 8, no. 4 (1979): 338–60, <http://www.jstor.org/stable/2265068>.

<sup>79</sup> This incommensurability is an important component of Neurath’s “felicology,” a physicalist alternative or successor to philosophical ethics. For an overview, see Stuchlik, “Felicology.”

<sup>80</sup> See, for example, John Hicks, *Value and Capital*, second edition (1939; Oxford and New York: Oxford University Press, 1975), ISBN: 978-0198282693.

measured; literally, they are incommensurable. Thus, while someone may be able to say that she prefers B to A because B is a higher-status position, she will not be able to say this on the grounds that  $x$  units of status are equivalent to  $y$  units of food, even if only for her. Neurath's criticism is much more radical than Kaldor and Hicks's: his leads to criticisms of the very idea of a utility function.

Six years later, Neurath is still critical of what he calls the "victory of the pure monetary calculus," the prevailing impression "that to a higher national wealth in monetary terms there correspond[s] a higher real income."<sup>81</sup> Notably, socialists have also been impressed in this way:

Even in the arguments of many socialists, who tend to emphasise the discrepancy between nominal calculus and real income, the assumptions of a monetary economy play a rather important role. This becomes apparent when their accounts are scrutinised in detail. Marx, for instance, often stresses this distinction in general terms, but lacks a proper theoretical in-kind calculus.<sup>82</sup>

Instead of treating wealth as the single standard of well-being — and consequently studying the economy as, in the first instance, a system for the circulation and accumulation of wealth — Neurath proposes that economists develop measures and models for economics "in kind," that is, for economic systems in which different, incommensurable kinds of goods are produced, are exchanged (both for other goods of the same kind and for goods of different kinds), and circulate.

While some of the concerns of incommensurability from 1910 re-appear here, Neurath's main arguments for the turn to economics in kind deal with the empirical adequacy of the representations produced by economists. The World War has replaced the monetary, market economy of the nineteenth century with an economy with significant in-kind, command features, and conventional, monetary economics is useless for understanding the new system and possible future developments.

The contemporary order is probably as distant from the free market of money and credit as it is from a state-run macro-economy in-kind . . . . If we want to be serious about the development of economic theory, we have to consider a whole range of possibilities: monetary economies of various kinds and in-kind economies of various kinds.<sup>83</sup>

These investigations will enable us to significantly enlarge our theoretical horizon and thus be prepared to face the problems of the present. Seeds

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<sup>81</sup> Otto Neurath, "Economics in Kind, Calculation in Kind and Their Relation to War Economics: Selections 1904-1945," chap. 9 in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (1916; Dordrecht: Kluwer Academic Publishers, 2004), 301, ISBN: 1402022735.

<sup>82</sup> *Ibid.*, 303.

<sup>83</sup> *Ibid.*, 305.



of future developments could be recognised as such sooner than before and the significance of many events would be better appreciated. Then it would depend on the political standpoints of the individual whether he wished to cultivate and to care for the seeds or to destroy them.<sup>84</sup>

Similarly, a year later, Neurath writes that

Whereas the market economy has been thoroughly investigated theoretically, the phenomena of administrative economy — price controls, rationing, compulsory production, centralisation of distribution — has been neglected so far. This disregard is one of the reasons why most predictions of economists about the possible duration and the probable economic institutions of a World War have failed — a failure that underscored the crisis of economic theory that has been noted for some time now.<sup>85</sup>

By now, Neurath has developed a new foundations for economic inquiry, one designed explicitly for the investigation of economics in kind. To be clear, by “foundations” I do not mean to suggest that Neurath was ever an epistemological foundationist of any sort. Rather, “foundations” refers to a set of central concepts, some assumptions about their relationships, and a methodology for further empirical investigation. In Lakatosian terms, a foundations is the hard core of a research program; in Longinian terms, it is the set of background assumptions required to start any inquiry; or, in Gutting’s terms, it is a “picture” whose details still need to be filled in. A foundations, in this sense, can easily be adopted fallibilistically or tentatively, and as one among several possible ways of approaching a topic.

The most basic concept in Neurath’s new foundations is *quality of life*, *Lebensstimmung*, “the course of experiences of a human being, as far as their enjoyment is concerned.”<sup>86</sup> From his characterization, it is unclear whether this is to be understood as a Benthamite, subjective sensation or living well in a more objective, Aristotelean sense:

[T]he quality of life is connected with all types of experiences, with eating, drinking, reading, artistic sensibility, religious contemplation, moral speculation, loving, hating, heroic and cowardly behaviour. If a quality of life is assigned to artistic sensibility this does not mean that the latter is nothing but quality of life or could be derived from it alone.<sup>87</sup>

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<sup>84</sup> [Ibid.](#), 308.

<sup>85</sup> Otto Neurath, “The Conceptual Structure of Economic Theory and Its Foundations: Selections 1904-1945,” chap. 10 in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (1917; Dordrecht: Kluwer Academic Publishers, 2004), 312, ISBN: 1402022735.

<sup>86</sup> [Ibid.](#), 313.

<sup>87</sup> [Ibid.](#)



In any case, economic inquiry itself deals more with the “external condition” of quality of life, that is, the macroscopic physical objects that are utilized to produce the experiences that constitute quality of life. Importantly, items in the external condition are often used to produce other and more items, and economics as a line of inquiry is defined as the study of this system of production:

Parts of the external condition are of interest to us as causes of life conditions. Yet parts of the condition of life itself can also become causes of conditions of life; this is true of the bread which, just eaten, counts as condition of life due to the pleasure it provides, but as a cause of conditions of life insofar as it enables humans to produce more bread.<sup>88</sup>

When we do[, for example,] agricultural economy we consider a group of activities characterised by their field of action. Theoretical agricultural economics asks how the same fields, woods, human and animal forces can produce various components of conditions of life by applying different technical, biological, chemical, etc., methods, as when it asks what happens by changing the condition of the fields, the swamps, the woods, etc. Other economies can be delimited and their different methods compared in a similar way: the economy of hunting, of water, of robbery, etc. In a comprehensive economic theory, discussion of agricultural methods (three-fallowing, rotation of crops, etc.) finds its place as much as that of methods of overall organisation (market economy, administrative economy, etc.). Sometimes we shall disregard the technical methods and only study certain forms of economic organisation for their economic performance, at other times disregard the latter and only study the former independently.<sup>89</sup>

Calculation in kind could, for example, start by finding out which raw materials are available at a certain time and at which places, how much water power, how much and which kind of labour power, inventive power, stupidity, diligence, etc. This cluster changes into another at the next moment. Certain things are transformed, as food into human body, other things into machines, etc. If a whole people is the subject of the study, the movement of the raw materials can be followed through their various stages by taking account of the production, consumption, storage, import and export. In a similar way individual spheres, such as agriculture, industry, etc., can be distinguished and investigated for how much in terms of power and material enters into them, how many products, how much waste material, etc., leaves them to enter other spheres. What has been said before may be repeated: facts which do not lend themselves to being stated in amounts, as inventive power, etc., must not therefore

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<sup>88</sup> Neurath, “The Conceptual Structure of Economic Theory and Its Foundations,” 316.

<sup>89</sup> *Ibid.*, 318-9.

be considered to be of absolutely indifferent character. If inventive power could be measured in horse power like working power, it would be added to the other powers. The pure impossibility of such measurement must not induce us to overlook that the gift of invention can, for example, replace other powers in certain circumstances.<sup>90</sup>

Consequently, economics is a much broader field than it is understood by Neurath's contemporaries, especially the rising marginalist school:

[T]he pure theory of exchange or of the market does not belong into the theory of economy proper in so far as it investigates on what prices depend or under which market conditions all exchange stops or when, as it is sometimes put, an equilibrium has established itself. These discussions of the pure theory of exchange represent, however, extremely valuable auxiliary considerations for the theory of economy, for they serve to provide data for the detailed description of the influence of particular conditions and processes on the intensity and distribution of the qualities of life.<sup>91</sup>

Let us note two important features of economics, as Neurath understands it in 1917. First, economics is *physicalist*, in exactly the sense that Neurath will use that term during the protocol sentence debate within the Vienna Circle fifteen years later:<sup>92</sup> it investigates the relationships between ordinary, macroscopic physical objects, and not subjective experiences (as with Carnap, Bentham, and Locke); synthetic *a priori* first principles (as with Ludwig von Mises's "praxeology"); or supra-individual cultural entities (as with late nineteenth-century Idealist social scientists). While Neurath's physicalist version of empiricism is not as well-developed in 1917 as it will be by 1935, even at this point the influence of empiricism on Neurath's economics is clear, and he is already engaged in the methodological reflections that will provide his basic arguments in the protocol sentence debate. In short, Neurath's work here is the work of a joint practitioner, in the practices of economics and empiricist methodology-epistemology.

Second, the overall aim of economics, as Neurath understands it, is *practical*:

In this way we would gradually return to the endeavours which assisted the start of economic theory, when economists generally were interested in finding out what the conditions of the wealth of the people are, which institutions increase it and which decrease it. This is not the place to demonstrate the gradual replacement of stress on the lively contribution of reality to theory by a methodology producing models which distinguished themselves more by their logical coherence than by their applicability to possible cases in real life.<sup>93</sup>

<sup>90</sup> [Ibid.](#), 327.

<sup>91</sup> [Ibid.](#), 323.

<sup>92</sup> See, among others, Neurath, ["Sociology in the Framework of Physicalism."](#)

<sup>93</sup> Neurath, ["The Conceptual Structure of Economic Theory and Its Foundations,"](#) 328.

The economist — or better, political economist — aims to produce excellent representational knowledge, but because excellent representational knowledge is essential for accurately predicting the effects of various public policies. Economic representations are evaluated by their ability to support the know-how of the economic administrator.

This is the sense in which Neurath thinks of social scientists — especially economists — as “social engineers.” Richardson adduces this concept as support for attributing a Weberian value-neutrality to Neurath.<sup>94</sup> On such a reading, economists as social engineers are technocrats, identifying the effects of various public policies by applying the representational knowledge produced by pure economists and not evaluating these effects as good or bad or otherwise; the work of economists as such remains value-free or value-neutral. But — as should be obvious in light of the last several chapters — this understanding of the relationship between engineers (and other “applied scientists”) and pure scientists presupposes an narrow view isolationist version of the pure/applied distinction. This interpretation of “social engineers” thus begs the question: Richardson (and Proctor before her) must assume that Neurath is a narrow view isolationist if the idea of “social engineers” is to support her claim that he is a narrow view isolationist.

Furthermore, it is difficult to reconcile this narrow view isolationist reading of Neurath with the quotation of two paragraphs ago. Neurath is not arguing that some subset of economists — namely, the ones who work as “social engineers” on public policy — should switch to economics in kind, leaving pure economics or economics as such as it currently stands. Indeed, doing so would split economics into two completely different fields, since pure economists and social engineers would work with completely different kinds of representations and standards of excellence. Rather, Neurath is arguing that economics *as a whole* should switch to economics in kind. And while this would stimulate progress in the sense of making the representations of economics more empirically adequate, even this is valued primarily for its practical benefits.

My argument of the last few paragraphs has been compressed, and there are points where I have ignored some important distinctions and complications. Thus it is still possible, based on the textual evidence available in the documents that I have cited so far, that Richardson can find a way to clarify and support her reading of Neurath. However, this will not be the case when we bring in some of the pamphlets that Neurath wrote within a few years of the 1916 and 1917 essays on the foundations of economics. We will deal with three in particular:

- Otto Neurath, “A System of Socialisation: Selections 1904-1945,” chap. 11 in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (1920; Dordrecht: Kluwer Academic Publishers, 2004), 345–70, ISBN: 1402022735.

This paper was first published in a journal for academic social scientists, *Archiv für Sozialwissenschaft und Sozialpolitik*, dated 1920/1921. This same journal

<sup>94</sup> Richardson, “[The Left Vienna Circle, Part 1](#),” 19.

published Max Weber's *The Protestant Ethic and the Spirit of Capitalism* in 1904 and 1905.

- Otto Neurath, "Total Socialisation: Of the Two Stages of the Future to Come," chap. 12 in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (1920; Dordrecht: Kluwer Academic Publishers, 2004), 371–404, ISBN: 1402022735.

Thomas Uebel notes that this "originally was a small brochure addressed to the public at large as the fifteenth in the series *Deutsche Gemeinwirtschaft* (German Communal Economy), published by the well-known Eugen Diederichs Verlag, whose 18 volumes included the Wissell-Moellendorff plan and sold a total of more than 40.000 copies."<sup>95</sup>

- Otto Neurath, "Economic Plan and Calculation in Kind: On the Socialist Order of Life and the Human Beings of the Future," chap. 13 in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (1925; Dordrecht: Kluwer Academic Publishers, 2004), 405–65, ISBN: 1402022735.

According to Uebel, this was "originally a small book put out by a Berlin publisher of Austro-Marxist literature, . . . clearly and directly addressed the workers' movement and [seeking] to respond both to Marxist critics and to [Austrian economist and proponent of laissez-faire capitalism Ludwig von] Mises."<sup>96</sup>

As discussed below, these pamphlets make it clear that Neurath thought economics in kind was especially useful for — not economic administration *in general* but — the economic administration of a socialist society. More specifically, the representations of economics in kind are especially useful for promoting two internal goods of the practice of movement socialism, which I will call *economic inclusiveness* and *economic democracy*.<sup>97</sup> Neurath is also clearly committed to the socialist project of organizing the working class so as to bring about these two internal goods. Since the pamphlets were written just a few years after the essays on the foundations of economics discussed above, I take it to be reasonable to assume that Neurath did not undergo some sort of conversion from a Neutral Marxist (writing the first essays on the foundations of economics) to a committed socialist activist-intellectual (writing the polemical pamphlets) in the intervening time. In other words, I assume that the commitment to socialist ethical and political values that Neurath displays in the later pamphlets was also held — perhaps less vehemently, but in any case not displayed — in the earlier

<sup>95</sup> Thomas Uebel, "Introduction: Neurath's Economics in Critical Context," in *Economic Writings*, ed. Thomas Uebel and Robert Cohen (Dordrecht: Kluwer Academic Publishers, 2004), 45, ISBN: 1402022735.

<sup>96</sup> [Ibid.](#)

<sup>97</sup> These terms will be defined below.

essays. It is then reasonable to interpolate Neurath's later argument for adopting economics in kind — its value for socialism — into the earlier essays — the ones in which economics in kind is first developed. Neurath's whole body of work on the foundations of economics is thus a case of joint practice — involving the three practices of economics, empiricism, and socialism — and the ethical and political values of socialism are influencing Neurath's efforts to stimulate progress in economics. And so Neurath was a transactionist, from fairly early on in his career.

It may be that the crucial interpolation — of a commitment to socialist values by Neurath in 1916-17 — is illicit. Properly supporting this claim requires access to documents that, as far as I know, are not available in English and therefore to which I do not have access. However, consideration of the pamphlets does show that Neurath had this commitment by 1920. Even if Neurath's socialist values did not lead him to *originally* develop economics in kind in the 1910s, it is clear that these values did stimulate their *further* development and Neurath's efforts to promulgate them in the 1920s. Neurath was a transactionist by the 1920s, even if he was not before. In particular, Proctor is utterly mistaken to label Neurath a Neutral Marxist based on papers published in 1931, and Neurath was a transactionist in the heyday of the Left Vienna Circle.

All three pamphlets take a similar polemical tone, display a deep-seated commitment to the aims of movement socialism (rather than a purely academic sort of socialism), present the major elements of the sketch of economics in kind, and also adopt roughly the same structure. Neurath begins with a criticism of the political and intellectual leadership of contemporary socialism:

The doctrine of historical necessity became quietism for many; what Marx said about active engagement in reconstruction was forgotten . . . . In place of creative action one pursued detailed analysis of the more accidental forms of the doctrine of surplus value and other parts of the Marxian edifice of ideas, the real impetus and force of which will only be appreciated by the future . . . . What was lacking, however, was an idea of the economic future that could have guided [the proletariat's] will. These powerful forces [roughly, of movement socialism] can only become creative if socialisation, the conscious realisation of the new order of life, is based on an intellectual analysis and if utopianism becomes effective as science, as social engineering.<sup>98</sup>

That the adherents of a dying economic order have nothing to offer is not astonishing, of course, but the same is true also for the reformers. Leading Social Democrats, under the guise of upholding the scientific attitude, have ridiculed the demand for a comprehensive picture of the future state for so long, with very few exceptions, they have killed off any

<sup>98</sup> Neurath, "A System of Socialisation," 345-6.

interest in socio-technical constructions.<sup>99</sup>

The goal of socialist organisation is pursued consciously ... only in part. Groups of the international proletariat, moved by solidarity, can be very successful in the class struggle without having very clear ideas about the socialist aim or about the historical development. It is nevertheless an essential part of socialist development to raise peoples consciousness and to continue the intellectual developments initiated in the bourgeois period.<sup>100</sup>

What is missing, of course, is an adequate understanding of what economics might look like during and after the transition from capitalism.

This understanding is supplied — or will be supplied — by work on economics of kind, and in each pamphlet the introductory section is followed by a section sketching Neurath's proposed foundations for economics, with exactly the same terminology as in the 1916 and 1917 papers.<sup>101</sup> These sections also argue that economic administration — the development and promulgation of an economic plan — is essential for remedying the defects of capitalism.

This passage is typical of the anti-capitalist arguments, in both content and style:

The traditional economic order appears less economical than a socialist economy which would replace under-utilisation by total utilisation. The lower degree of utilisation of the traditional market economy has still other facets. The free market economy wastes energy which could serve the production of utility goods, for instance, when an excess number of shops are kept open with underoccupied staff. An abundance of material and energy is used in advertising of all sorts, from poster hoardings to crowds of commercial travellers flooding the land.

In even greater measure the under-utilisation characteristic of the traditional economic order manifests itself in a multiplicity of goods that is to nobodys advantage. To win the fight of competition ever new forms of goods of restricted use are thrown on the market, pen-knives, suitcases, etc., in shocking numbers. The producers are not interested in the quality and durability of their goods but, on the contrary, in a quick change of fashions.<sup>102</sup>

More succinctly, “The critics of the traditional economic order issued two severe indictments: that it accepts mass poverty side by side with massive wealth and that

<sup>99</sup> Neurath, “[Total Socialisation](#),” 371.

<sup>100</sup> Neurath, “[Economic Plan and Calculation in Kind](#),” 405.

<sup>101</sup> Neurath, “[A System of Socialisation](#),” 346-7; Neurath, “[Total Socialisation](#),” 375-84; Neurath, “[Economic Plan and Calculation in Kind](#),” 408-30.

<sup>102</sup> Neurath, “[Total Socialisation](#),” 378-9. At this point, it should be clear that Neurath was not a Neutral Marxist.

it accepts crises, depressions, unemployment and waste of all sorts of energy.”<sup>103</sup> The root of both problems is the chaotic, disorganized capitalist system of production and distribution; socialism will bring about order and organization.

Socialism tries to replace the planlessness which springs from the disconnected activities of individual entrepreneurs by an administrative economy according to a plan, by an economic order in which central institutions survey the entire economy in order to participate in decisions on work, production and consumption.<sup>104</sup>

Later sections of each pamphlet discuss — in some detail — the various institutions (I use this in Neurath’s sense, not my as-contrasted-with-practice sense) of socialist society responsible for various aspects of developing and administering the plan. These will not concern us here; our interest is the overall aims of these institutions and the contributions the study of economics in kind makes to them.

Above, I introduced the terms *economic inclusiveness* and *economic democracy* to characterize the primary internal goods of socialism — that is, the overall aims of the institutions of socialist economic planning. By economic inclusiveness I mean the inclusion of every individual — to the best of their ability — in the productive activities of the community. For example,

There is a mentally handicapped boy in the village — what to do to give him his share of human dignity? He may look after the geese. In the capitalist order all the children would have been subjected to qualification tests to find out who is best suited to look after the geese. Let us assume that under the best child only five out of a hundred geese would be lost in a year, but the handicapped boy would lose ten. The socialists could nevertheless say: the five additional geese that the handicapped boy lost are sacrificed for his humanity; they make it possible for him to be a member of the community, and he is not pushed aside to go to the dogs. They could say the same even if it were cheaper to let the boy vegetate in a home, without occupation . . . . As mentioned before, the time to come will not have to do with villages, nor with such narrowly circumscribed problems, but with giant organisations, with a giant complex of associations in many layers. All the same, for kind-hearted people it can appear to have at least in principle something in common with this village . . . . As a counterpart to the handicapped little boy with his geese there will be settlements for epileptics, with production equipment that is especially adapted to them.<sup>105</sup>

<sup>103</sup> Neurath, “[Total Socialisation](#),” 378.

<sup>104</sup> *Ibid.*, 381.

<sup>105</sup> Neurath, “[Economic Plan and Calculation in Kind](#),” 455 and 457.



Such inclusiveness, however, cannot be understood or rationalized in terms of wealth. As measured in terms of wealth, the marginal contributions of the handicapped boy looking after the geese and the epileptics with their specially-adapted equipment are, quite probably, negative; it makes better economic sense (with this conception of “economic”) simply to provide them with a minimal income and exclude them from productive activity. By contrast, as measured by economics in kind — and its underlying notion of quality of life, which can incorporate such concepts as “human dignity” — the inclusion of these impaired individuals in productive activity can be justified, and we can even discuss the specific goods they will produce and the specific (other) goods they will require to produce those, given their specific cognitive and physical abilities. Bourgeois charity is replaced with socialist *caritas*.

Notably, economics in kind seems to bring Neurath to the brink of understanding reproduction and care work as forms of productive activity:

In an order which is based on an economic plan, mothers can be granted shorter working time and assigned to special tasks; in the economy of free competition women had, as far as possible, to be on an equal footing with men to avoid their total enslavement. A purely superficial equality, which has nothing to do with the emotional harmony between people who share feelings, is one of the notable effects of an individualistically minded age.<sup>106</sup>

In this respect, he seems to have been decades ahead of many of his socialist contemporaries.<sup>107</sup>

By economic democracy I mean that decisions about economic public policy — about the economic plan — are substantively influenced by public deliberation and opinion. While the Central Economic Administration contains the bureaucrats and economists responsible for the technical details and is, in some sense, at the center of the administration of the economy, the “institutions of socialisation” go well beyond this government bureaucracy. More specifically, they include an elaborate system of decentralized or semi-autonomous “non-bureaucratic Producers Associations,” co-operatives for the crafts and peasants, and “councils” (literally *Räte*, as in the *Bayerische Räterepublik*, the Bavarian Soviet Republic) of various groups of citizens. Through various mechanisms, the local-level deliberations and views in these various bodies flow up to and inform the work of the Central Economic Administration. Like the government of the Soviet Union was supposed to work, Neurath’s economic administration is run from the bottom up, not the top down.

Furthermore, economic democracy as Neurath understands it also includes a significant degree of “economic tolerance” or, in language closer to my own, significant autonomy for different practices. After presenting an elaborate taxonomy of versions

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<sup>106</sup> *Ibid.*, 461.

<sup>107</sup> Cf. Jaggard, *Feminist Politics and Human Nature*, chs. 4 and 8.

of socialism,<sup>108</sup> Neurath points out that

The programme of socialisation here discussed makes an attempt at a simultaneous realisation of socialism, solidarism and communism . . . . It provides for co-operatives for peasants and craftsmen, for collectivist settlements on a communist basis and for large-scale socialist production in agriculture and industry to exist side by side, in order to do justice to their different aspirations to realise a collective economy in their own way . . . .

That the flows of raw materials and resources occur according to a plan does not mean that that plan is known to everyone! It is conceivable that peasants, sitting on their plot of land, continue to produce in peace for their own needs, following the old custom, because their actions correspond to the new plan of the collective economy. The authorities do not have to pursue a centralised formation of the will, it is enough if deviations from the economic plan are avoided and deficiencies are compensated for.<sup>109</sup>

Similarly,

The economic plan requires the economy to be unified. This does not mean that all decision-making is centralised, as many Social Democrats wish. It is enough if a central body ensures that independent decisions by various economic groups will fit into the general plan and that certain changes are brought about in the interest of the general plan. For example, a cooperative of craftsmen can be allowed to continue alongside an industrial association; the only requirement is that each single craftsman belongs to an association that is concerned with the implementation of the economic plan. That association will control, e.g., whether the craftsmens cooperative returns the products made under their own management from assigned raw materials to the people as a whole in accordance with the economic plan.<sup>110</sup>

The next future but one [sic] will perhaps not be characterised by the rule of the Leviathan world organisation but much more by the enlivening activity of smaller groups and associations, combined into a unit on a higher level which, however, would be conciliatory and not despotic.<sup>111</sup>

Neurath's conception of economic democracy offers the possibility of *détente* — and optimistically even common ground and collaboration — between several of the rival socialist factions of his day and between the urban proletarian and rural peasantry. In

<sup>108</sup> Neurath, "[A System of Socialisation](#)," 351.

<sup>109</sup> [Ibid.](#), 354.

<sup>110</sup> Neurath, "[Total Socialisation](#)," 388.

<sup>111</sup> [Ibid.](#), 401.

my terminology, each practice is able to organize itself in line with its own best current understanding of what internal goods are and how they are produced; the function of the Central Economic Administration is to coordinate these various activities, not force them into a rigid and micromanaged hierarchy.

As discussed in §2.5, in a society whose productive system is dominated by the market, a practice requires money for the provision the concrete, specific resources it requires for the production of its own internal goods, but money as such is not useful. So, for members of a practice, answering the question “What can we produce, and what do we require to produce it?” requires, in a primary sense, reference to concrete, specific goods. Reference to money is required, if at all, only and insofar as these concrete, specific goods are to be obtained by way of the market. That is, the question is answered primarily in terms of economics in kind, and wealth is only relevant to the extent that the market dominates the production and provision of concrete, specific goods. In a socialist society, where the market no longer dominates the production and provision of goods, we may not even need to consider wealth at all. All together, the primary language in which the deliberations of economic democracy are conducted is the (physicalist) language of economics in kind rather than the (idealist) language of monetary economics. As Neurath puts it,

What is the basis for such decisions [i.e., public policy] in an administrative economy? Certainly not considerations of creating or changing net profit, but considerations of the consequences of the production of coal and of agriculture. The administrative economy investigates what the production and distribution in the various possible cases are and which of the outcomes correspond best to the desired goal, be it a master-oriented or a community-oriented economy. In short, it bases its decisions on an economic plan.<sup>112</sup>

At its worst, the language of monetary economics leads us to evaluate production in terms of profitability, which in turns leads to the neglect of non-financial aspects of the quality of life:

In the socialist order the economic efficiency of a single factory cannot be deduced from the accounts of the factory alone. This was also impossible in the capitalist order, but then the profitability of a factory was confused with its economic efficiency, because the question of how the operation of a factory may improve the totality of conditions of life of a society was not raised at all. The profitability of a factory can of course be assessed in isolation. It is only necessary to juxtapose expenditure and income in money terms, taking into account the definite situations at the beginning and at the end. But whether a certain factory is more

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<sup>112</sup> [Ibid.](#), 382.

efficient, is better for society under one kind of operation than under another, can only be decided by finding out which of the two fits into the better economic plan. For instance, a factory might be managed in the best technical way and increase its production, yet could still be uneconomical because the product is of no use, for example if the factory that produces necessary complementary goods cannot produce enough.<sup>113</sup>

We have now seen how Neurath argues for the adoption of economics in kind — for economists in general, not just the social engineers in the Central Economic Administration — based on the contributions it can make to the two socialist internal goods of economic inclusiveness and economic democracy. While Neurath does occasionally repeat the methodological criticisms of mainstream economics — the ones we saw in the essays from 1916 and 1917 — his primary argument, in the pamphlets from the 1920s, is the transactionist argument that the representations of economics should be evaluated by their contributions to the internal goods of socialism. And lest it is not crystal clear that this transactionist view of the relationship between the movement of socialism and the intellectual discipline of economics is Neurath's, I close this discussion with his own words:

This book aims at being a constituent part of the structure of proletarian socialism — that socialism which the proletariat consciously brings into reality — and is to be regarded as a building block which is useful only if it fits into the acting and thinking of the socialistically minded proletariat.

Here we will attempt to make the theory of conditions of life the centre of our considerations so as to be able to improve economic theory.<sup>114</sup>

## 5.5 Pascual Jordan: Nazi joint practice in physics?

Nazi science — the domination of German scientific inquiry by the NSDAP in the 1930s and '40s — is important for any advocate of transactionism for at least three reasons. First, Nazi science is perhaps the single most common example of the harmful effects of politicization, that is, it is *the* standard example of the institutional domination of scientific inquiry. Thus transactionism should be able to articulate this harm. In short, transactionism should be able to articulate the thought that Nazi science was bad science. Second, some of the details of Nazi science bear resemblance to some transactionist proposals. This lends at least some rhetorical strength to isolationist worries over the threat of domination. Thus transactionists must be able to distinguish their proposals from Nazi science, either by clarifying their own views, by giving a better account of Nazi science than isolationists, or both. Third,

<sup>113</sup> Neurath, “[Economic Plan and Calculation in Kind](#),” 426.

<sup>114</sup> *Ibid.*, 408 and 410.

the domination of German scientific inquiry by the NSDAP was so complete and yet so quick that it provides an important case study for the empirical investigation of the processes involved in institutional domination.

I will not, however, give anything like a complete treatment of Nazi science here.<sup>115</sup> Instead, I focus on the particular example of Pascual Jordan (1902-80), a prominent and influential quantum physicist, Weimar culture warrior, and member of the NSDAP. Jordan's most important scientific contributions were to the development of quantum mechanics, especially the formalization of matrix mechanics he wrote in 1925-6 with Heisenberg and Born.<sup>116</sup> For Jordan, quantum mechanics posed a profound challenge to the determinism, materialism, mechanism, and objectivity that had been characteristic of natural science — including both physics and biology — since the Enlightenment.<sup>117</sup> As Norton Wise puts it,

he intended the development [of quantum mechanics] to show that the “dogmatic materialistic conception” of classical physics was completely inadequate even for the physical world . . . . Quantum mechanics would point the way toward an “organic conception,” a rigorous conceptual foundation for previously fuzzy ideas like “finality” and “wholeness.” It would ground in physics itself, in its strictest mathematical form, a holistic, teleological viewpoint on all aspects of nature.<sup>118</sup>

It thereby also had immense cultural significance. As Richard Beyler puts it,

Throughout his life, and in a variety of ways, Jordan linked scientific and cultural-political concerns. The common factor in these endeavors was his professed antagonism to materialism in all its forms and a consequent series of affiliations with perceived opponents of materialism in the social sphere, ranged on the right wing of the political spectrum, but varying in their specific identity [from proto-fascism to National Socialism to Cold War anti-Communism] with the turbulence of twentieth-century German history.<sup>119</sup>

<sup>115</sup> For a thorough discussion of Nazi science, see Proctor, *Racial Hygiene*. For a discussion of some important elements of Nazi science in the context of the contemporary science and values debate, see Daniel Hicks, “Is Longino’s Conception of Objectivity Feminist?” *Hypatia* 26, no. 2 (Spring 2011): 333–51, doi:[10.1111/j.1527-2001.2010.01160.x](https://doi.org/10.1111/j.1527-2001.2010.01160.x).

<sup>116</sup> M. Norton Wise, “Pascual Jordan: Quantum Mechanics, Psychology, National Socialism,” in *Scientists, Engineers, and National Socialism*, ed. Monka Renneberg and Mark Walker (Cambridge, UK: Cambridge University Press, 1994), 224.

<sup>117</sup> Wise, “Pascual Jordan,” 225-6; Richard Beyler, “Exporting the Quantum Revolution: Pascual Jordan’s Biophysical Initiatives,” in *Pascual Jordan (1902-1980). Mainzer Symposium zum 100. Geburtstag*, ed. Jürgen Ehlers, Dieter Hoffmann, and Jürgen Renn (Max-Planck-Institut für Wissenschaftsgeschichte, 2007), 69.

<sup>118</sup> Wise, “Pascual Jordan,” 229.

<sup>119</sup> Beyler, “Exporting the Quantum Revolution,” 71.

That is, Jordan associated determinism, materialism, mechanism, and objectivity with Enlightenment liberalism (or, after the war, Communism), and attacked the latter through and along with attacks on its scientific “foundations.” I want to examine three aspects of Jordan’s work in particular: his rejection of objectivity, his interpretation of quantum mechanics, and his quantum biology. Throughout, I show how Jordan believed that his work held prospects for significant progress in scientific inquiry, and in ways that drew directly on his Nazi views. This raises the possibility that Jordan’s work is an example of transactionist joint practice. I use my discussion as an occasion to sketch a transactionist response to this possibility.

### 5.5.1 Objectivity

The first aspect of Jordan’s work is his rejection of objectivity, in the sense of an ideal of value-neutral, universal scientific inquiry. While the Nazi movement did not have, as it were, an official epistemology, Dick Pels has shown that philosophers associated with the movement defended a version of standpoint epistemology and connected it to the movement and its political views. He gives, for example, the following summary of the epistemology Martin Heidegger articulated in his inaugural address as the Nazi rector at Freiburg:

[Heidegger] wedded explicit criticisms of scientific neutrality . . . to a clear conviction about the identity of the mission of science and the mission of the German people in its present spiritual-political fate . . . . The essence of science was not found in its “liberal” independence and freedom from presuppositions (*Voraussetzungslosigkeit*), but in its submission to necessity (i.e. the *political* necessity of an alienated and threatened German identity), and in its immediate coalition with practice (i.e. the *political practice* of “knowledge service” to the spiritual mission of the German people). It was this standpoint, that of “the German *Volk* in the extreme questionableness of its existence,” which also defined the true nature of objectivity (*Sachlichkeit*); it was only from this standpoint that objectivity could establish itself, i.e. find its character and limits . . . .<sup>120</sup>

Standpoint epistemologists, on both the right and the left, can be characterized by two claims: that all knowledge is socially located — that is, that the class or group identity of the producers of a given representation is epistemically significant — and that some small set of social locations is epistemically privileged — that is, that a small set of classes or groups produce better representations (in some sense) than others. Leftist standpoint epistemologists, for example, claim that the representations produced by members of the poor and working class, by women or feminists, or by historically oppressed racial and ethnic groups are better (for certain purposes or in

<sup>120</sup> Dick Pels, *The Intellectual as Stranger: Studies in Spokespersonship* (London and New York: Routledge, 2000), 105; his emphasis and parentheses.

certain particular fields) than those produced by members of privileged and powerful classes. Nazi standpoint epistemologists made similar sorts of claims about the representations produced by the German *Volk*. Often, standpoint epistemologists connect epistemic privilege to ethical and political practices: feminist and working-class standpoint epistemologists claim that involvement in the feminist or labor movement is the basis for the superior representational knowledge produced by feminists and labor activists, for example. This is therefore a version of transactionism, and like the feminist transactionism discussed in §5.2, it is related to partly external progress with respect to the standards of representational knowledge: as standpoint epistemologists for practice *X* see it, the influence of *X* on scientific inquiry leads to partly external progress. Nazi standpoint epistemologists can be understood as making the same sort of claim *so long as the NSDAP or its associated movement was a practice*.

This last clause is extremely important for a transactionist analysis of Nazi scientific inquiry. If the Nazi movement was a practice then Nazi standpoint epistemology should be understood straightforwardly as a version of transactionism: they claimed, after all, that the Nazi movement may legitimately influence all aspects of scientific inquiry, including its standards. To return to Jordan, he argues that the liberal, Enlightenment, value-neutral, reason-based, etc., conception of objectivity must be replaced with a militaristic conception of objectivity that Norton Wise calls “the machine gun principle of objectivity”:

“War is the chief means for the creation of objective historical facts — meaning such facts whose actuality must be recognized also by opposing nations. And war forms the objective probe for comparison of the forces and weapons on both sides.” Despite differences of style, if weapons work for and against the enemy, they possess transnational validity. Thus [for example] western mathematics possessed “universal meaning,” extending even to Japan, “after the Japanese have adopted European technology and European conduct of war.”<sup>121</sup>

According to Jordan, the ultimate *epistemic* test of quantum mechanics is its ability to produce new weapons — atomic bombs — and thereby to extend and maintain a socio-political order over a larger geographical and temporal region than is possible with the weapons produced using classical physics and chemistry.<sup>122</sup> Granting that militarism, the Third Reich, and the security of the German *Volk* are internal goods of the practice of the Nazi movement, it seems that Jordan can claim that his new conception of objectivity is an improvement over Enlightenment conceptions and that this improvement is a result of the influence of the practice of the NSDAP by way of Nazi standpoint epistemology. In short, it appears to be a case of partly external progress.

<sup>121</sup> Wise, “Pascual Jordan,” 226, glossing and with quotations from Jordan.

<sup>122</sup> *Ibid.*, 226, 234.



This conclusion, while not a complete *reductio ad absurdum* of transactionism, should be considered unacceptable, especially for any liberal or leftist transactionist. The most obvious response — which is ultimately my response — is to deny that the Nazi movement was a practice. Perhaps it never was, and never could have been, a practice; perhaps it started off as a practice and rapidly came to suffer institutional domination. Both lines of response can emphasize the importance of, for example, military power and racial hierarchy to the Nazi movement, and argue that these were forms of power and thus external goods. Jordan’s militarism, for example, did not seem to have any room for the traditional heroic or warrior virtues of honor, courage, and sacrifice of one’s life to protect one’s community or the virtues of conducting war justly and with respect for all soldiers as “comrades in arms” that might be thought constitutive of *warrior practice*;<sup>123</sup> he is focused entirely on the power of new weapons to kill, maim, and terrorize massive numbers of people, which (beyond not counting as an “excellence” in any but the most perverse sense) does not seem to be attached to any specific kind of activity in the way that internal goods are. This view is supported by the rampant infighting and doctrinal incoherence that, Beyler argues, characterized the Nazi regime, including Nazi science:

The power structure of the Nazi state, despite the propaganda touting totalitarian unity, was a “polycracy” of different power blocs, often with ill-defined and overlapping spheres of authority and sometimes with mutually antagonistic interests. This was especially true in science policy. Here there were long-standing conflicts between authorities who pushed for instrumental efficiency and those whose main interest was ideological purity. The struggle between modern physics (relativity and quantum theory) and the *deutsche Physik* movement was an example of this phenomenon.<sup>124</sup>

I believe that this line of response — that the Nazi movement was either an institution or a nationalist practice that suffered massive institutional domination — is correct. However, the recognition claim gives us reason to pause. As the connection argument shows in the context of the science and values debate, those who are not familiar with a practice are liable to mistake its internal goods for external goods — to mistake a practice for an institution. We, as non-participants in the Nazi movement,

<sup>123</sup> Cf. MacIntyre, *After Virtue*, ch. 10; Jonathan Lear, *Radical Hope: Ethics in the Face of Cultural Devastation* (Cambridge, MA and London: Harvard University Press, 2006), ch. 1; Michael Walzer, *Just and Unjust Wars* (BasicBooks, 1977), ch. 3. To be clear, I have strong pacifist leanings, and am therefore skeptical of any claim that any such intrinsically violent activity as warfare (and, for that matter, boxing and football) is a practice. My suggestion here is just that, if warfare is a practice, the virtues of heroism and just war are the best candidates for its internal goods.

<sup>124</sup> Richard Beyler, “Targeting the Organism: The Scientific and Cultural Context of Pascual Jordan’s Quantum Biology, 1932-1947,” *Isis* 87, no. 2 (June 1996): 250.

may be making a similar mistake if we conclude too quickly that this movement was an institution. So making this response to the challenge of Nazi science requires significant and careful empirical support. I believe that the ample literature on the Nazi movement, and Nazi science in particular, provides more than adequate support for the response, but I will not attempt to give it here. Instead, I will show how the basic response can be deployed with respect to the other two aspects of Jordan's work.

### 5.5.2 Quantum physics

The second aspect is Jordan's interpretation of quantum mechanics. His monograph *Anschauliche Quantentheorie* was an attempt to develop an "intuitive" interpretation of quantum mechanics; Jordan drew heavily and explicitly on Machian epistemology, the so-called "Copenhagen" interpretation of quantum mechanics of Bohr, and Nazi political theory.<sup>125</sup> Quantum indeterminacy, for example, was interpreted by Jordan as a sort of radically spontaneous, non-rational "choice" by the electron (say). Two years earlier, the prominent Nazi political theorist and jurist Carl Schmitt had published the second edition of his defense of dictatorship.<sup>126</sup> Schmitt argued that the need to take decisive action in a state of emergency required a dictator: where parliamentary deliberation is slow, rational, and constrained by the rule of law, the decisive action of a dictator is immediate, supra-rational, and supercedes legal constraints — like the "choice" of Jordan's indeterministic quantum.

Similarly, Jordan presented the work of physicists as a movement from the brutal, "vitalizing primitivity" of experience to the "elegant simplicity" of "leading," *föhrende*, ideas and principles under the guidance of a leader, *Föhrer*.<sup>127</sup> As Wise points out, "this juxtaposition of beauty with brutality was . . . common in romantic ideology";<sup>128</sup> for example, Heidegger's interpretation of Nietzsche opens with a discussion of "The will to power as art."<sup>129</sup> This use of the language of the *Föhrer* was interpreted by Hugo Dingler — another member of the NSDAP and a leader of the *deutsche Physik* movement — as an attempt to, as Wise puts it, "transplant the symbol of the *Föhrer* onto Bohr and his elite school (as well as Einstein)."<sup>130</sup> In the eyes of his contemporaries, Jordan was attempting to associate modern physics (even

<sup>125</sup> Pascual Jordan, *Anschauliche Quantentheorie. Eine Einführung in die modern Auffassung der Quantenerscheinungen* (Berlin: Julius Springer, 1936). On the "Copenhagen" interpretation, see Don Howard, "Who Invented the "Copenhagen Interpretation"? A Study in Mythology," *Philosophy of Science* 71, no. 5 (December 2004): 669–82.

<sup>126</sup> Carl Schmitt, *Political Theology: Four Chapters on the Concept of Sovereignty*, ed. George Schwab (Chicago: University of Chicago Press, 2005).

<sup>127</sup> Wise, "Pascual Jordan," 233–234.

<sup>128</sup> *Ibid.*, 234.

<sup>129</sup> Martin Heidegger, *Nietzsche*, ed. David Farrell Krell (1936–1940; San Francisco: Harper-Collins, 1991), vol. 1.

<sup>130</sup> Wise, "Pascual Jordan," 233, his parentheses.

if it was created by socialists and Jews) with the Nazi movement. In particular, in context, the hierarchical organization of disciplines under a single leader was known as the *Führerprinzip*, and this principle was the basis for the consolidation and reorganization of many areas of German life — including science and medicine — under the NSDAP.<sup>131</sup>

The title of Jordan’s monograph is a clear reference to Hilbert and Cohn-Vossen’s famous semi-popular introduction to geometry, *Anschauliche Geometrie*.<sup>132</sup> Hilbert and Cohn-Vossen aimed at an “intuitive understanding” of geometry that “fosters a more immediate grasp of the objects [of geometry] . . . , a live rapport with them, so to speak, which stresses the concrete meaning of their relations.”<sup>133</sup> Jordan’s effort to give a similarly “intuitive” presentation of quantum theory can be construed as an effort to deal with a pressing problem in early quantum mechanics: “the widespread charge that quantum mechanics, because it denied the possibility of unique causal description in space and time, was *unanschaulich* — unvisualizable and nonintuitive — therefore degenerate . . . and ultimately false.”<sup>134</sup> Granting that the Nazi movement — including Schmitt’s justification of dictatorship and Heidegger’s interpretation of the will to power — was a practice, Jordan’s proposal, as with objectivity, appears to have been a case of joint practice leading to partly external progress: quantum mechanics becomes “intuitive” — understood immediately, vividly, and concretely — when it is interpreted using concepts and metaphors central to Nazi practice.

### 5.5.3 Quantum biology

The third aspect of Jordan’s work — his “amplification theory” of quantum biology — can likewise be construed as an effort to deal with a pressing problem, in this case the centuries of interminable disagreements between, on the one hand, reductionist, mechanist, and materialist views of biology and, on the other hand, anti-reductionist, organicist, and vitalist views. Jordan’s proposal was, viewed strictly in this context, a rather clever attempt to find a golden mean using the new tools of quantum mechanics. As Beyler puts it, “his aim was nothing less than a conceptual synthesis between physics and organicism.”<sup>135</sup> Based on some early work in genetics and the effects of radiation exposure on living organisms — the latter done largely by physicists working within mechanist views of biology<sup>136</sup> — Jordan proposed that “living beings, although macrophysical in size, were ‘directed’ or ‘steered’ by acausal [indeterministic] quantum events, whose effects were then somehow amplified by organic

<sup>131</sup> Proctor, *Racial Hygiene*, ch. 3.

<sup>132</sup> David Hilbert and S. Cohn-Vossen, *Geometry and the Imagination*, trans. from the German by P. Nemenyi (1932; Chelsea Publishing Company, 1990), ISBN: 0828410879.

<sup>133</sup> *Ibid.*, iii.

<sup>134</sup> Wise, “Pascual Jordan,” 232.

<sup>135</sup> Beyler, “Targeting the Organism,” 249.

<sup>136</sup> *Ibid.*, 255.

structures.”<sup>137</sup> On this “amplification theory,” organisms must be understood organically in some sense, as complex systems that transform the indeterministic statistical regularities of quantum mechanics into the indeterministic statistical regularities of biology, ecology, and psychology. It is reductionist and not vitalist, yet also indeterminist and organicist. In a 1941 book developing the amplification theory, Jordan identified the gene as the “microphysical steering center ruling the macrophysical cell” and, unsurprisingly given his interpretation of quantum indeterminacy, linked it to authoritarianism, calling it “perhaps the most extreme realization that the principle of authoritative leadership has received in all of nature.”<sup>138</sup>

Jordan’s proposal was, to put it mildly, controversial and, while it enjoyed some initial experimental successes, he was never able to gather sufficient institutional support (in my sense) from either the Nazi state, the Soviets, or the Americans before its associated project in radiation biology fell apart.<sup>139</sup> But it is charitable to think that some of its claims were at least plausible in the earliest days of molecular biology and it is, again on its own terms, quite a clever attempt to reconcile organicists and reductionists. At the time, therefore, or if its empirical research had gone in its favor, it could have been rightly thought a plausible case of partly external progress.

Again, the most straightforward response to claims that Jordan was a joint practitioner and that Nazi science could have stimulated partly external progress is to claim that these ideas were not contributions to practices, whatever the value of the rest of their work. I’ll focus here on the aspects of Schmitt’s and Heidegger’s work mentioned above. For example, Schmitt’s “justification” of dictatorship might be better understood as a rationalization of authoritarian power and such systems of social organization as the *Führerprinzip*. Heidegger’s aesthetic interpretation of Nietzsche’s will to power, similarly, might be interpreted as an attempt to link the external goods of glory and power and to subordinate aesthetic values to these external goods — the appropriation of art as a means of glorifying nationalist power. This line of response would argue, more generally, that these aspects of Schmitt’s and Heidegger’s work effectively disguised external goods and institutional domination as the internal goods of a purported practice, in what Wise calls the Nazi “celebration of power for its own sake”;<sup>140</sup> these works themselves were thereby forms of institutional domination.<sup>141</sup>

<sup>137</sup> *Ibid.*, 261.

<sup>138</sup> *ibid.*, 269, quoting Jordan in the first quotation. For egalitarians, on the other hand, the authoritarianism of this “master molecule” view of DNA and genes is a bug, not a feature. For a classic feminist critique of this view of DNA and genes and “master molecule” views in general, see Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven: Yale, 1985), chs. 8 and 9, esp. pp. 169-72.

<sup>139</sup> Beyler, “Targeting the Organism,” 262ff, 267, 271.

<sup>140</sup> Wise, “Pascual Jordan,” 254.

<sup>141</sup> The text may suggest that important aspects of Jordan’s work can be directly attributed to Schmitt and Heidegger. This is not the case. For one, Nietzsche’s lectures on Heidegger started in 1936, the same year Jordan’s book was published. For another, I have neither given nor found any evidence that Jordan was familiar with the work of

While this response may be satisfactory with respect to Schmitt's account of dictatorship on its own, it is much less so with respect to Heidegger's aesthetics. The latter, I understand from my continental colleagues, is considered a mainstream and legitimate treatment of aesthetics in continental circles. A transactionist analysis may therefore focus on Jordan himself. Postwar writings and editions of his books carefully omitted any reference to politics and he appeared more than willing to convert his militaristic conception of objectivity "from an offensive to a defensive posture and deploying it in the interests of democratic rather than dictatorial government"; Jordan "now defended freedom with the same imagination and intensity that he had formerly lavished on authority."<sup>142</sup> That is, Jordan's own scientific practice appears to have suffered institutional domination after the war. This may also have been the case before the war.

All together, I want to draw the following moral from the analysis of Jordan's work: Distinguishing between practices, institutionally dominated practices, and institutions is hard, empirical work that should be undertaken with a fallibilist attitude and attention to details. By outsiders, Nazi science can easily be described as the war-like and racist pursuit of power; but, by insiders, it can just as easily be described as the pursuit of nationalistic pride and recovery, and as offering prospects for progress in scientific inquiry. This is not to say that we should automatically defer to anyone who claims their activities constitute a practice. Indeed, in the case of Nazi scientists, I think such claims would amount to self-delusion. We must resist the temptation to slide from the undesirable aspects of unfamiliar activities to a hasty conclusion that these activities are pursued only for the sake of external goods. We must engage with those who pursue these activities — engage with those who activities we may find unappealing, or perhaps even repugnant, at least some times and in some cases — in order to judge adequately whether they pursue any internal goods.<sup>143</sup>

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either theorist in any way. Instead, I suggest that the Schmitt and Heidegger can be treated as representatives of and contributors to Nazi philosophy — individuals whose claims greatly resemble those of many of their contemporaries, at least in part because their contemporaries' claims were clearly influenced by these individuals — without any claim of particular influence on Jordan.

<sup>142</sup> Wise, "Pascual Jordan," 252.

<sup>143</sup> The clause "some times and in some cases" is meant to suggest a middle ground between the two extreme positions identified in Hicks, "Is Longino's Conception of Objectivity Feminist?" The sort of engagement that I have in mind is similar to an approach to philosophical inquiry that MacIntyre calls "the ethics of enquiry." Alasdair MacIntyre, "Moral Pluralism without Moral Relativism," in *The Proceedings of the Twentieth World Congress of Philosophy*, ed. Klaus Brinkman, vol. 1: Ethics (Bowling Green, 1999)

## 5.6 John Dewey: The transaction of science and common sense

In the first half of the twentieth century, John Dewey argued for one of two prominent versions of transactionism. Like the logical empiricists who defended the other prominent version,<sup>144</sup> Dewey's reputation languished for much of the second half of the twentieth century. If the Steel Belt narrative of postwar philosophy is accurate, this is in part due to his transactionism: prior to World War II, transactionism was associated with socialism; at the height of the Cold War, transactionism was politically dangerous, and hence so was defending too many of Dewey's views.<sup>145</sup> In addition, Dewey's philosophical method and informal, rather literary style was in tension with the new emphasis on logical rigor and formalism in Anglophone philosophy after World War II.

Over the past decade, however, a number of mainstream analytic political philosophers and philosophers of science have begun to recover Dewey's legacy, in some exemplary cases even reconstructing his most important ideas in the contemporary sociolect of analytic philosophy of science. In this section, I'll examine Matthew Brown's recent dissertation, a reconstruction of Dewey's account of transactionist inquiry in the analytic sociolect.<sup>146</sup> I argue that, while Dewey's account (as Brown reconstructs it) is powerful and contemporary transactionists should make a point of understanding it better, it is limited in two crucial respects. First, inquiry as Dewey understands it is not a practice as I understand it. Instead, it is an activity or family of activities that appear in many different practices. Second, and more importantly, it lacks the resources to give an account of institutional domination; while it offers far richer resources than the underdetermination and no distinction arguments for transactionism, it still cannot adequately respond to the worries that motivate isolationists.

### 5.6.1 Habits and problematic situations

First, a quick comparative intellectual genealogy. Like MacIntyre, Dewey began his philosophical career as a species of Hegelian; where MacIntyre was a Marxist (some

<sup>144</sup> See the discussion of Neurath, §5.4.

<sup>145</sup> Dewey himself held some combination of democratic socialism and egalitarian liberalism. For a discussion of Dewey's politics and political philosophy in relation to socialism and Marxism, see Alan Ryan, *John Dewey and the High Tide of American Liberalism* (New York: W.W. Norton and Company, 1995), ch. 8. Some of Dewey's views on social and political philosophy will be discussed briefly in §7.4.2, especially as they relate to individualism. For the effects of the Cold War on Dewey's reputation within academic philosophy, see Reisch, *How the Cold War Transformed Philosophy of Science*, chs. 4, 12-14.

<sup>146</sup> Brown, "Science and Experience."



sources characterize him as a Trotskyite, but I have not been able to confirm this), Dewey was part of the American school of objective idealists.<sup>147</sup> And, like MacIntyre, Dewey gradually abandoned his Hegelian commitments for more Aristotle-influenced views of human nature and practical reason. Where, for MacIntyre, his Catholicism led him to Aquinas and thereby Aristotle, for Dewey the story is more complicated. Dewey abandoned his idealism primarily under the impetus of Darwinian natural selection and late nineteenth century work in experimental physiology and psychology. However, one of his academic colleagues at Columbia — F.J.E. Woodbridge — encouraged him to read Aristotle.<sup>148</sup>

We can therefore see Dewey's conception of *habits* as a second cousin of MacIntyre's practices. In his most perspicuous characterization, a habit is

that kind of human activity which is influenced by prior activity and in that sense acquired; which contains within itself a certain ordering or systematization of minor elements of action; which is projective, dynamic in quality, ready for overt manifestation; and which is operative in some subdued subordinate form even when not obviously dominating activity.<sup>149</sup>

Like a practice, a habit is a goal-directed or purposive activity. Just as the purpose of a practice is to realize its internal goods, the purpose of a habit is to realize certain ends.<sup>150</sup> Habits are social in two senses. First, they must be learned or cultivated in any given individual — habits are not innate — and this is usually done by or under the influence of other people. Second, many habits are collaborative, performed or done by a group of individuals working together. As Dewey characterizes them in the first few pages of *Human Nature and Conduct*, habits are “social functions.”<sup>151</sup> Habits are social, in this sense, in exactly the same way as practices.

There are some important differences between habits and practices. First, habits need not be as complex as practices. Examples of habits include walking, and while Dewey's discussion of walking goes beyond mere mechanical locomotion from point *A* to point *B*, it is still not as complex as, for example, scientific inquiry.<sup>152</sup> Second, Dewey has nothing like MacIntyre's distinction between practices and institutions, or internal and external goods. All of the ends of various habits are on a par, at least

<sup>147</sup> Robert Westbrook, *John Dewey and American Democracy* (Ithaca: Cornell University Press, 1991), part one, ISBN: 0801425603.

<sup>148</sup> *Ibid.*, 119.

<sup>149</sup> John Dewey, *Human Nature and Conduct*, in *John Dewey: The Middle Works, 1899–1924*, ed. Jo Ann Boydston, vol. 14: 1922 (1922; Carbondale and Edwardsville: Southern Illinois University Press, 1983), 31, ISBN: 0809310848.

<sup>150</sup> Dewey's view that ends and means “interpenetrate” causes trouble for this simple characterization, but it is adequate for our purposes. See Dewey, *Human Nature and Conduct*, 28ff; Dewey, *Theory of Valuation*.

<sup>151</sup> Dewey, *Human Nature and Conduct*, 15ff.

<sup>152</sup> *Ibid.*, 29-30.



as such. Empirical ethical inquiry may lead us to downplay or reject certain habits for the disruptive effects they have on other habits — for example, people who are addicted to drugs or money are liable to lose almost all opportunities to realize the goods of a rich social life — but there is no general or principled distinction between different kinds of ends built into the account at a high level.

Dewey's usage of "habit" differs from common usage in some important respects. Habits are not mindless routines, and they are not fixed and unchanging. Indeed, they are changeable precisely because they are mindful. First, habits include habits of thinking. The writing style characteristic of analytic philosophy or a particular analytic philosopher, for example, is a habit. Second, as a self-conscious being, I am capable of scrutinizing my habits and considering the various interactions between them. Using the same example, the writing habits that I have cultivated preclude creative, aesthetically stimulating writing habits. For this reason, I may decide to replace these writing habits, cultivating instead the more creative writing style characteristic of many continental philosophers.

This change cannot be achieved all at once, and in general I cannot expect to completely and utterly replace the old habits with the new ones. I cannot simply switch from the analytic to the continental writing style. My habits are voluntary, but not in the way that many philosophers seem to understand voluntary.<sup>153</sup> Indeed, Dewey says, if I try to change my habits through "will power" alone, I am almost certain to fail. His example is of a hard-drinker who wants to become sober:

The hard-drinker who keeps thinking of not drinking is doing what he can to initiate the acts which lead to drinking. He is tarting with the stimulus to his habit. To succeed he must find some positive interest or line of action which will inhibit the drinking series and which by instituting another course of action will bring him to his desired end.<sup>154</sup>

So habits can be changed, but typically not easily. One role for reason in changing habits is to identify those lines of action that lead, indirectly, from the old habits to the new ones.

However, habits do have a certain inertia or momentum: we tend to keep doing things the same ways. Understanding the reasons why we change our habits will lead us to Dewey's account of inquiry, but first we need the concept of the situation, the environment in which the organism's habits are done. For this, I will turn to Matt Brown's perspicuous account.

A *situation*, for Dewey, involves some organisms, their environment, and the synchronic and diachronic relations between them. Situations are dynamic and do not have well-defined boundaries; new elements may enter into or come to exist within

<sup>153</sup> See, for example Alvin Plantinga, *Warrant: The Current Debate* (New York and London: Oxford University Press, 1993), 23-4, ISBN: 0195078624.

<sup>154</sup> Dewey, *Human Nature and Conduct*, 28.

the environment, old elements may leave the environment or cease to be, and the relations between elements may change; parts of the environment may become distinct as elements and elements may recede into the background of the environment. Situations may be nested one inside the other, and there may even be complex relations between situations. Brown illustrates with the situation in which he finds himself as he writes:

Consider another example: as I sit attempting to characterize John Dewey's philosophy of science, the foreground of my situation includes my computer, the texts I'm looking at, my notes and my thoughts about how best to explain his views. The focus of the situation is the very text I'm composing. In the background, there are a variety of other texts I'm not at the moment thinking of, a community of Dewey scholars, philosophers of science, and others who are the potential audience of the text, but whom I am not attending to directly at the moment . . . .

The situation, the stage on which my dissertation is constructed, is one that I've been in for almost two years now, and I will be in it until the project is complete. But I'm also in many other situations during that time: when I'm teaching a class or fixing Pat's computer, there are other situations for those activities. The plight of student workers at the University of California, the difficulties of the southern California housing market, and my developing romantic relationship are all situations that I have had to navigate in these times.<sup>155</sup>

Habits are cultivated in situations; or, in an evolutionary idiom, habits are adaptations to situations. But situations can change, in endless ways, and sometimes an organism will find itself in a situation where one or many of its habits no longer work. That is, they (read as either the organism or its habits) no longer achieve their ends. An organism in such a situation is said to be in *doubt*; or the situation is said to be *indeterminate* or *problematic*.

This language of "doubt" suggests something subjective, such as an internal feeling on the part of the organism. But this would be a profound misunderstanding of Dewey's view. Brown points out that Dewey's view is a development of C.S. Peirce's, which Brown calls the *doubt-belief model*. Peirce distinguishes between *paper doubt* and *genuine doubt*. Paper doubt is exemplified by the largely unmotivated and sweepingly skeptical method of Descartes's first meditation; as Brown emphasizes, paper doubt is doubt that is not itself based on "good reason."<sup>156</sup> Genuine doubt, by contrast, is a response to a subjective experience of "unease, surprise, or novelty,"<sup>157</sup> and hence is "reasonable doubt." In other words, genuine doubt is motivated by features of the agent's experience (it is not unmotivated) and the doubt concerns

<sup>155</sup> Brown, "Science and Experience," 46.

<sup>156</sup> *Ibid.*, 130, n. 12.

<sup>157</sup> *Ibid.*, 130.

particular aspects or details of that situation (it is not sweeping or general). According to Peirce, only genuine doubt can lead to successful inquiry; paper doubt is philosophically pernicious.

On Brown's reconstruction, Dewey takes up Peirce's distinction between paper and genuine doubt, but crucially replaces the *subjective* experience with *objective or intersubjective* situations. Or, Dewey *naturalizes* Peirce's account:

A situation is not merely personal and subjective; it includes the whole person or group of persons and the constituents of their environment relevant to the inquiry or practice at hand. Problems do not arise as purely intellectual matters, but rather due to 'incidents occasioning an interruption of the smooth, straightforward course of behavior . . . . *An indeterminate or problematic situation for Dewey is a "breakdown" of practice, as it is for Heidegger . . . .*<sup>158</sup>

A situation is not indeterminate because (subjectively) the organism in it has a certain feeling of indeterminacy or uncertainty. It is indeterminate because (objectively) the organism's habits no longer realize their aims.

To stress the non-subjectivity and naturalism of the account, consider a plant seed in an area undergoing climate change. Suppose, among other changes, both the mean and variance in daily low temperature have increased significantly. Where, in recent evolutionary history, daily low temperatures have generally stayed between -10 and 0°C until late in the season, it is now common for daily low temperatures to swing between -12 and 7°C. A plant adapted to the old local climate would stay dormant throughout the season, only germinating late in the season when daily low temperatures began to move consistently above freezing and shortly before pollinators (who, as I understand it, respond more to daylength than temperature) began to become active. But now these plants begin to germinate midway through the season. Fragile sprouts are much more likely to be killed by a sudden (and severe) cold snap, and there will be no pollinators for those plants that survive long enough to bloom. *The germination habits of the plants no longer work, and the plants are faced with an indeterminate situation, in exactly the same sense that Dewey uses this term.* If they are to avoid extinction, they must adopt new habits, such as by moving to a new location with a climate similar to that of the old climate or by evolving new habits that are better adapted to the new climate.<sup>159</sup>

<sup>158</sup> *Ibid.*, 131-2, quoting Dewey, my emphasis.

<sup>159</sup> To put the reason for using the plant example another way: Assume that a plant seed does not have what contemporary philosophers of mind call "phenomenal consciousness," such as the internal, mentalistic "feel" of uncertainty or doubt about climate change. Then the example shows that "phenomenal consciousness" is not part of Dewey's account of genuine doubt or problematic situations. I suggest that this is because the notion of "phenomenal consciousness," as a successor to the notion of "sense-data," is one side of a false dichotomy that Dewey's account of experience re-

The important difference between plants and animals, on Dewey's account, is that it is much, much easier for animals — especially vertebrates — to modify their habits. Generally, the habits of plants can only be modified over the timescales of natural selection; animal habits can be modified over the lifetime of a single individual. And the important difference between human and (most) non-human animals is the set of abilities that Dewey calls rationality or *inquiry*: our abilities, for example, to anticipate problematic situations in advance, anticipate the various effects of new habits in various ways, and deliberate together over which new habits to actually adopt. As Brown puts it,

The activity of organisms normally follows established patterns — habits. Sometimes, however, habits are unsuccessful in producing their normal outcomes. In response to the failure of habit, rational organisms engage in the activity or family of activities called inquiry, in which they endeavor to establish the available courses of action and pursue one. The success of inquiry is measured by the establishment of a successful course of action and new, successful habits.<sup>160</sup>

### 5.6.2 Dewey's account of inquiry

We can gloss the sketch above as the view that *inquiry is a response to the disruption of habits*. The primary aim of inquiry is not to produce new representational knowledge, though in some situations representational knowledge will be extremely valuable. Instead, the primary aim of inquiry is to establish habits that work in the new environment or modify the environment to accommodate old habits. Further, the standards of success for inquiry depend on the standards of success for the courses of action and habits that it establishes, that is, whether the habits work. These are, of course, *pragmatic* aims and standards — the technology to modify the environment and the practical knowledge, whether new or old, of how to act in these sorts of situations. Hence, if we attempt to recast Dewey's account of inquiry as an account of scientific inquiry as practice, the primary internal goods of scientific inquiry are technology and practical knowledge.

Again, this does not mean that representational knowledge is completely without value; it is simply secondary to or a part of the primary pragmatic aims. Consider a (“pure”) scientist, conducting a series of experiments. Within the series, conducting each experiment will be a matter of habit: first cell samples are taken, next they are

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jects as at best inadequate and probably philosophical nonsense. See, among others, John Dewey, *The Quest for Certainty*, in *John Dewey: The Later Works, 1925–1953*, ed. Jo Ann Boydston, vol. 4: 1929 (1929; Carbondale and Edwardsville: Southern Illinois University Press, 1981), ch. 7. On the other hand, since I personally find talk of “phenomenal consciousness” utterly incomprehensible, it's quite possible that I am completely wrong about this.

<sup>160</sup> Brown, “[Science and Experience](#),” 62.

cultured (itself a relatively complex series of steps), then the cultures are examined under a microscope, and so on. Analyzing the results of the series of experiments will also follow a relatively established pattern, as observations are used to generate a dataset, which in turn is run through a series of statistical tests, and so on. But this habit can break down, for example, by such anomalous results as test statistics that are significantly different from expected values or by observations under the microscope that cannot be straightforwardly accommodated within the dataset format. (Pure) scientific inquiry is the scientist's response to this sort of disruption of her habits. This situation might be made more determinate in part by the development of new representational knowledge — a new account of the chemistry of these sorts of cell, say. With accompanying technology and practical knowledge, this representational knowledge can be used to establish new patterns of activity: new experimental steps that reliably produce expected results. However, even here, representational knowledge is still secondary to the pragmatic aim of establishing new patterns of activity.

On this strong interpretation, Dewey's account of inquiry seems strictly incompatible with the broad view. Where the narrow view takes epistemic aims to be more important than pragmatic aims and the broad view takes both sets of aims to be equally important, Dewey takes pragmatic aims to be more important than epistemic aims. To the extent that we can arrange the narrow view, the broad view, and Dewey's view on a one-dimensional spectrum, then, Dewey's view is on the opposite end from the narrow view and in that sense is closer to the broad view. Further, more moderate accounts of inquiry — either rival interpretations of Dewey's account or accounts that are inspired by Dewey but not attributed to him — would give representational knowledge greater or more independent status, and thus also be closer to the broad view than the narrow view.

Nothing in Dewey's account specifies which sort of established patterns of activity, when disrupted, stimulate inquiry. The example of the (pure) scientist is an example of what we might call *epistemic inquiry*, inquiry in response to the disruption of activities closely related in some way to representational knowledge. But inquiry might also be a response to the disruption of non-epistemic activities, such as the activities of ethical and political practices; call this *ethical and political inquiry*. Since the standards of successful inquiry depend on the standards of success for the patterns of activity that it establishes, it follows that the standards of successful inquiry depend, at least in ethical and political inquiry, on the standards for the internal goods of ethical and political practices. Or, as Dewey puts it,

- (1) Scientific subject-matter and procedures grow out of the direct problems and methods of common sense, of practical uses and enjoyments,
- and (2) react into the latter in a way that enormously refines, expands and librates the contents and the agencies at the disposal of common sense . . . . Scientific subject-matter is intermediate, not final and complete in

itself.<sup>161</sup>

This, of course, is a version of transactionism.

Consider the examples of feminist science in §5.2. In that section, I reconstructed the contributions of feminist scientists in terms of Longino's contextual empiricism. They can also be reconstructed in terms of Dewey's account of inquiry. By the late 1960s, the burgeoning feminist movement had highlighted and exacerbated a complex array of indeterministic situations in the lives of American women.<sup>162</sup> As this movement expanded and feminists entered various areas of scientific inquiry through the 1970s and 1980s, indeterministic situations in the sciences were identified and related to the indeterministic situations of American women. The explanatory model of male agency, for example, was involved in indeterministic situations in archeology and primatology but also — insofar as it supported representational knowledge that was used to rationalize male dominance in the contemporary US — indeterministic situations in the lives of American women in general. The success of feminist inquiry in primatology, then, depended (and, presumably, depends) on its ability to deal with both the indeterministic situation in primatology and the indeterministic situation of American women in general. And arguably it was successful in both respects: the new explanatory models enable primatologists to produce representational knowledge that is both explanatorily powerful and empirically adequate and this representational knowledge suggests — in a much more sophisticated way than previous accounts — more egalitarian ways of organizing families, for example.

### 5.6.3 Some limitations of Dewey's account

With the relevant elements of Dewey's account of inquiry laid out, I turn now to some limitations. My overall claim is not that Dewey's transactionism is of no importance or relevance to the contemporary science and values debate. Nor do I claim that Dewey's transactionism could not be modified or developed in ways that address some of these limitations; indeed, I think that it may be modifiable in this way, and hence it is valuable as a resource for contemporary transactionists. Instead, I argue that Dewey's transactionism as Brown has reconstructed it is inadequate to address some of the most important issues in the contemporary debate. I suggest, then, that mere reconstruction of Dewey is at best the first step; contemporary fans of Dewey should also engage in extending and revising his transactionism. I take it that Dewey himself would advocate exactly this sort of project.

<sup>161</sup> Dewey, *Logic, the Theory of Inquiry*, 71-2

<sup>162</sup> The thought that I am trying to express in Deweyan terms might be put better in Marxian or Maoist terms: Theorizing the contradictions of the social order can make them both more obvious and more dire. More obvious because what the oppressed suffered silently and individually before, and may not have even experienced as oppression, now becomes a serious problem. More dire because the organization against oppression stimulates a backlash movement that makes the contradictions even worse.

I discuss two limitations in particular. First, on Dewey's account, inquiry is not a practice but a feature of many different kinds of activities, including both practices and institutions. Second, Dewey's account lacks the resources to give an account of institutional domination; it therefore cannot respond to the threat of domination.

Again, in a slogan, according to Dewey inquiry is a response to the disruption of established patterns of activity. In showing that Dewey is a transactionist, I pointed out above that Dewey is not particular about what sorts of activities are disrupted; the disruption of both scientific inquiry and ethical and political practices can lead to inquiry in Dewey's sense. But this also means the activities of institutions, if disrupted, can lead to inquiry.

Consider the financial crisis of the fall of 2008. The giant financial organizations that were threatened — or, in some cases, dissolved — by the crisis were institutions if anything is. The crisis itself was a massive disruption to the established patterns of activity for the members of these institutions. As the crisis spread, much of the economy of the world was rendered indeterminate. This massive indeterminate situation led to a massive inquiry: How shall we deal with this situation? Should we let the giant financial institutions fail? Should we prevent them from failing? If so, all of them, or only some? What about the homeowners whose defaulting mortgages are undermining the balance sheets of these institutions? What about the rest of the economy? Meanwhile, there's an extremely unpopular lame duck President and the final weeks of a fiercely contested election; certainly politicians were thinking about how their policy decisions would influence their institutional positions. Their campaign managers no doubt paid extremely close attention to polls and other public opinion data in trying to determine what their candidates should say and do — a sort of electoral inquiry.

Just as certainly, all these intersecting lines of inquiry in the fall of 2008 were not examples of what we would call science or scientific inquiry. Anyone familiar with political pundits in the popular media knows that their pronouncements are typically based exclusively on speculation, anecdote, and rhetorical vigor rather than any actual data, analysis, or thoughtful argument.<sup>163</sup> It might be argued that any such speculation-driven, evidence-free activity cannot count as inquiry, and (for this reason) it is not scientific inquiry. I am happy to grant that. My point is that even a version of this activity that does rise of the level of inquiry would still not be *scientific* inquiry.

It might be proposed in response that scientific inquiry can be distinguished from the inquiry in general (and inquiry-like features of other activities) by suitably speci-

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<sup>163</sup> The work of statistician Nate Silver and his colleagues at the blog *FiveThirtyEight* is a notable exception to this generalization, and indeed Silver's blog has often explicitly criticized the groundless speculations of other pundits. See, for example, John Sides, "The Moneyball of Campaign Advertising (Part 1)," *FiveThirtyEight*, October 5, 2011, <http://fivethirtyeight.blogs.nytimes.com/2011/10/05/the-moneyball-of-campaign-advertising-part-1/> (accessed October 26, 2011).



fyng its subject matter: scientific inquiry deals with either the natural world (natural scientific inquiry) or humans and their interactions (social scientific inquiry), or something like this. Indeed, my objection might be thought quite close to the difficulty that I called the description problem, and I gave exactly this response to that problem. Or, to take a slightly different tack, scientific inquiry might be said to be a response only to disruptions of epistemic activities, as in the example of the (pure) scientist.

There are two problems with either version of this proposal. First, Dewey's account of inquiry is clearly meant to be general and applicable to any subject matter or any established activity. This more specific kind of inquiry is exactly that: one specific kind of inquiry, not inquiry as such or inquiry in general. While Dewey's account of *scientific* inquiry may be a practice, his account of inquiry *in general* cannot be, exactly because it is meant to be a *general* feature of established activities. Second, when restricted in either way, the obvious transactionism of the account is lost. If inquiry proper only deals with the subject matter or activities of (pure) scientific inquiry, for that very reason it does not obviously deal with the subject matter or activities of ethical and political practices. This emendation is thus incompatible with Dewey's clear transactionism.

Next, since Dewey's account of inquiry is not sensitive to the distinction between practices and institutions, it cannot make any distinction between the distinct goods of these two kinds of activities. It therefore cannot give an adequate general account of institutional domination.

This is not to say that Dewey cannot distinguish between progress and some sort of harm or corruption within some diachronic habit; or between activities that are extremely successful and promote (in one way or another) the success of other activities, on the one hand, and activities that disrupt or prevent (in one way or another) the success of other activities, on the other hand. The entire point of *Logic, the Theory of Inquiry*, after all, is to give an account of inquiry as an activity that is especially constructive and success-promoting rather than destructive and success-preventing.

But in cases of institutional conflict, promoting the success of any practice is at least limiting the success of its institutions and promoting the success of any institution is at least limiting the success of its practices. As far as Dewey's account of inquiry is concerned, these are simply two activities that are in tension and there is no general reason, within the account, to think that the aims or goods of one are more important than the aims or goods of the other. In the broader situation — the social context of these two activities, for example — one may be more important, but that depends entirely on particular features of the particular situation.<sup>164</sup>

<sup>164</sup> Compare this argument to one of Rawls's major arguments against utilitarianism: Utilitarianism can provide an argument for protecting basic individual liberties, but only on the very contingent grounds that doing so happens to promote maximal utility in this place and time. Rawls's justice as fairness, by contrast, recognizes the distinct im-

Consider a case of commercialization; say it has recently come to light that pharmaceutical companies have ghost-written a large number of low-quality research papers, published under the name of prominent and highly-respect pharmacologists in prominent and highly-respected journals.<sup>165</sup> Obviously this produces a disruption in both the activity of the pharmaceutical companies (the institution) and the activity of the pharmacologists (scientific inquiry). Deweyan inquiry could take into account a wide range of important features of the situation, including the value of pharmaceutical innovation, the need for pharmaceuticals to be reliable and safe (as well as the ambiguities surrounding “safe”), and the complex government-university-market arrangements that, at present, provide all of the institutional support to pharmaceutical research. It may even take into account the specific judgment that reliable and safe pharmaceuticals are a more important good than the accumulation of greater wealth by pharmaceutical companies, their stockholders, and executive management. But all of these are treated as particular facts about this particular situation.<sup>166</sup> If inquiry into this particular situation does include some more general judgment that the specific family of goods produced by the specific family of activities of scientific inquiry (including pharmaceutical research) are more important than wealth accumulation, it is not provided by the account of inquiry itself. Dewey’s account, even while it may be able to deal with particular cases of institutional domination as part of particular indeterminate situations, does not recognize institutional conflict and institutional domination as general or recurrent features of scientific inquiry.

In response, it might be pointed out that there is no incompatibility between my conception of practice and Dewey’s account of inquiry. His account could simply be supplemented with the conceptual tools needed to give an account of institutional domination.

But this response concedes my point. If Dewey’s account of inquiry needs to be supplemented with a general account of institutional domination this is because it cannot, by itself, give such an account.

Again, my aim here is not to dismiss entirely Dewey’s account of inquiry. Especially in Brown’s reconstruction, Dewey’s work provides an important alternative to the narrow view and isolationism. My aim here is simply to show that a reconstruction of Dewey, by itself, is inadequate to grapple with some of the most important issues in the contemporary science and values debate.

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portance of basic individual liberties, and hence separates them — literally by principle — from, for example, economic considerations. See, among other instances, Rawls, *A Theory of Justice*, 24-5.

<sup>165</sup> Ewen Callaway, “Questions over Ghoswriting in Drug Industry,” Nature News Blog, September 7, 2010, doi:10.1038/news.2010.453, <http://www.nature.com/news/2010/100907/full/news.2010.453.html> (accessed August 22, 2011).

<sup>166</sup> “Fact” is used here in a particular technical sense; see Brown, “Science and Experience,” 50ff.

### 5.6.4 A note on “transactionism”

In this appendix to my discussion of Dewey, I explain the meaning of one of the key terms of this dissertation.

I chose the term “transactionism” in a double nod to Dewey. First, of course, his own view is transactionist. Second, the terminology has some specific implications. In a chapter of a book co-written with Arthur Bentley, Dewey distinguishes “self-action,” interaction, and transaction:

**self-action:** where things are viewed as acting under their own powers.

**inter-action:** where thing is balanced against thing in causal interconnection.

**trans-action:** where systems of description and naming are employed to deal with aspects and phases of action, without final attribution to “elements” or other presumptively detachable or independent “entities,” “essences,” or “realities,” and without isolation of presumptively detachable “relations” from such detachable ‘elements.’<sup>167</sup>

Dewey’s definitions are, as usual, rather difficult to parse in the contemporary sociolect. The examples he uses to illustrate the distinction are helpful.<sup>168</sup> Aristotelean substances, whose actions are typically expressions of their essences, are examples of self-action. Leibnizian monads — which only appear to interact with other substances — would also be examples of self-action. Early Modern mechanist atomist representations are paradigmatic examples of interaction: atoms still have fixed and unchanging essences (their shapes and masses), but these are far sparser than the Aristotelean forms; the bulk of the explanatory work is done by appeal to the dynamic properties of the atoms (their motions and collisions), though ultimately these are based on their fixed essences.

Dewey illustrates transactions with Maxwell’s electrodynamics, general relativity, and quantum physics. Since I think I understand his point better with respect to general relativity, I’ll stick to that example. In general relativity, space, time, gravity, mass and motion cannot be understood independently of each other. A body does not have monadic (in the logician’s sense) properties of mass or motion (velocity, momentum, and so on); still less does it have any of these essentially or as part of its essence. Instead, a body’s mass and motion is (or depends on) its relations to other bodies. Or, more radically still, perhaps massive bodies and their motions through space and time are nothing more than the relation between the mass-energy and gravitational tensor fields; and finally (to gesture wildly in the direction of structural realism) these two fields don’t have (monadic) properties, but instead are nothing

<sup>167</sup> Quoting freely from John Dewey and Arthur Bentley, *Knowing and the Known*, in *John Dewey: The Later Works, 1925-1953*, ed. Jo Ann Boydston, vol. 16: 1949-1952 (1949; Carbondale and Edwardsville: Southern Illinois University Press, 1989), 101-2.

<sup>168</sup> *Ibid.*, 104-9.

more than placeholders in the relation. According to transactionism in Dewey's sense, then, there are no entities with properties, much less essential properties; rather, entities and their behavior can only be understood, in contextualist or holistic fashion, by their relations to other entities. More recent examples could come from ecology.<sup>169</sup>

To return to transactionism in my sense, we get a picture somewhat like the following. There are no essential or natural aims or standards for scientific inquiry. While they may appear to be fixed over some period of time or under some sufficiently general description, they are dynamic. Furthermore, they are not adequately understood as properties of scientific inquiry, and scientific inquiry cannot be understood in isolation from other human activities. Rather, scientific inquiry must be understood holistically or in context, as it is related to, for example, ethical and political practices. In particular, its aims and standards must be understood in relation to the aims and standards of these other practices. And this goes both ways: ethical and political practices and their aims and standards can only be adequately understood in relation to scientific inquiry. Both kinds of practices influence each other. As in Dewey's account of inquiry, the disruptions in ethical and political practices initiate at least some lines of scientific inquiry and scientific inquiry provides some tools for re-establishing patterns of activity in ethical and political practices.

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<sup>169</sup> See, for example, J. Baird Callicott, "The Metaphysical Transition in Farming: From the Newtonian-Mechanical to the Eltonian Ecological," *Journal of Agricultural and Environmental Ethics* 3, no. 1 (March 1, 1990): 36–49, doi:[10.1007/BF02014479](https://doi.org/10.1007/BF02014479).



# Chapter 6

## Science and values from the perspective of practice: Transactionism and the broad view

### 6.1 Introduction

In chapters 3 and 4, I developed and argued for the connection hypothesis, the claim that the narrow view of scientific practice leads to isolationism. In the last chapter, I began our discussion of transactionism with a series of case studies. These case studies each illustrate the importance of progress, and progress that is produced by interactions with ethical and political practices, for various versions of transactionism. In several of these cases, we also saw transactionists taking the broad view of scientific inquiry. Reflecting on these case studies and the connection hypothesis suggests the *inverse connection hypothesis* that the broad view leads to transactionism.<sup>1</sup>

In §§6.2 and 6.3, I give two arguments to support the inverse connection hypothesis. The first turns on the notion of progress, and develops the argument-sketch for transactionism presented in §4.1.2. In light of certain similarities to the connection argument, I call this argument the inverse connection argument. The second argument turns on the broad view's interdependence of epistemic and pragmatic aims.

In light of the two connection hypotheses, I conclude that there are two stable positions within the science and values debate: *narrow view isolationism* and *broad view transactionism*. This does not mean that other positions are *logically* untenable. The claims of the two connection hypotheses are causal claims, and include *ceteris paribus*

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<sup>1</sup> The terminology is meant to suggest a roughly logical relation between the two hypotheses. If the connection hypothesis is “if broad view then isolationism,” the narrow view is “not broad view,” and transactionism is “not isolationism,” then the inverse connection hypothesis is the logical inverse of the connection hypothesis: “if not broad view then not isolationism.” Of course, the conditionals here are causal, not formal or truth-functional.

clauses, or describe causal tendencies rather than necessary conditions. So we expect that someone who holds, say, both the narrow view and transactionism will tend to abandon one part of their views, and thereby move towards one of the two stable positions. Section 6.3 includes a simple account of a “basic” version of transactionism, one that I believe will be acceptable as far as it goes to all transactionists.

In §6.4 I examine the pure/applied distinction from the perspective of the broad view. As we saw in §4.2, isolationists characteristically appeal to versions of this distinction that assume the narrow view. I offered this observation as a premise in an abductive argument for the connection hypothesis: since the broad view tends lead toward transactionism, isolationists will not appeal to broad view versions of the distinction. However, this assumes that there are indeed broad view versions of the distinction. So in this chapter I offer those versions.

In §6.5, I take up the threat of domination. I argue that, in judging the legitimacy of a given ethical and political practice influencing a given line of scientific inquiry, the threat of domination must be weighed against the prospects of partly external progress, and that this can only adequately be done empirically and on a case-by-case basis. In some cases the threat of domination will outweigh the prospects of progress; in other cases the prospects of progress will outweigh the threat of domination. This suggests that the development and defense of both transactionism and isolationism should be a collaborative, interdisciplinary project, involving philosophers, social scientists, historians, and practicing scientists themselves. The science and values debate cannot be settled by strictly philosophical, epistemological considerations.

It seems clear that the narrow view is the predominant view among philosophers of science, and to a lesser extent among other academics in science studies and scientists themselves. This explains, among other things, why transactionists often articulate and argue for their views in the idiom of the narrow view. In §6.6, I argue that this produces characteristic misunderstandings of transactionist points and seems to make transactionism vulnerable to simple objections.

Finally, in §6.7, I consider the implications of my analysis for the science and values debate. While this debate is usually construed epistemologically — that is, primarily as a debate over the influence of values on normative epistemology — I argue that it must be construed “sociologically” — that is, primarily as a debate over the interactions between certain kinds of social activities. To elaborate on this reconstrual, I offer three pieces of advice to my fellow transactionists.

## 6.2 The possibility of progress

Recall the argument-sketch for transactionism presented in §4.1.2:

- (T-1) Progress is an internal good of scientific inquiry.
- (T-2) Progress can best be achieved by letting ethical and political practices influence the standards for the primary internal goods of scientific inquiry.



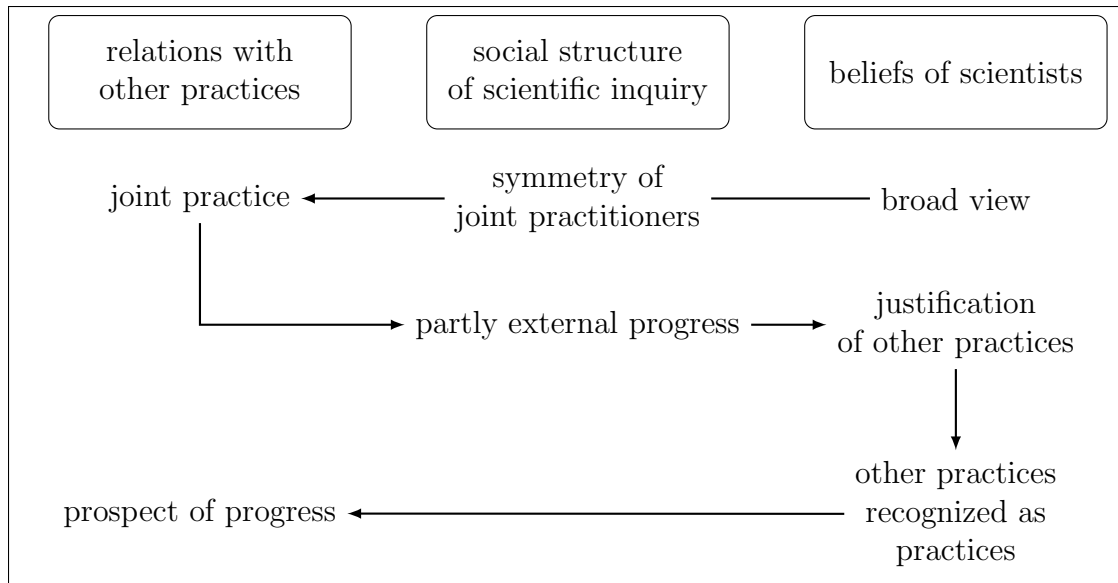


Figure 6.1: The Inverse Connection Argument

(T-3) The internal goods of scientific inquiry should be achieved.

(T-4)  $\therefore$  Ethical and political practices should influence the standards for the primary internal goods of scientific inquiry.

In that same section, I also presented an argument-sketch for isolationism and showed how the connection argument explains why someone who holds the narrow view would tend to accept the crucial premise (I-2). Here I do something similar, explaining why someone who holds the broad view would tend to accept the crucial premise (T-2) in the argument-sketch for transactionism. Furthermore, the mechanism that this argument for (I-2) describes is somewhat similar to the mechanism described in the connection argument. For example, both mechanisms concern the social position of joint practitioners within scientific inquiry and the consequent influence of joint practice. I therefore call this argument the *inverse connection argument*. See figure 6.1, and compare it with 3.1.

Recall that, in §3.4.3, I argued that the broad view leads to a conception of eudaimonia as joint practice: the development of representational knowledge, technology, and practical knowledge that simultaneously promotes the internal goods of some other practice. In many cases, joint practice will not have any lasting influence on the standards for these internal goods of scientific inquiry, much less constitute partly external progress. For example, the use of established water analysis techniques to evaluate the safety and health of the drinking water of an impoverished community downstream from a coal power plant, while joint practice, will probably not have last-

ing effects on the standards for good water analysis.<sup>2</sup> But we have also seen several examples where joint practice apparently has led to partly external progress: in the work of cookstove designers; in feminist critiques in biology, primatology, and other fields; and arguably in Gould's development of punctuated equilibrium.

On the narrow view, the work of these joint practitioners, insofar as it can be adequately construed as both applied scientific inquiry and stimulating progress, violates the asymmetry between pure and applied scientific inquiry: only the work of pure scientists may legitimately influence epistemological standards. Insofar as they are applied scientists, joint practitioners may not legitimately influence these standards. To do so, they must be pure scientists. These moves were central to the connection argument.

By contrast, on the broad view, there is no asymmetry between pure and applied scientific inquiry; scientists producing new technology are, as such, on a par with scientists producing new representational knowledge. Since joint practitioners enjoy such symmetry, their work may legitimately influence the standards for representational knowledge and, at least in some cases, stimulate partly external progress.

Recall an example discussed for the connection argument: a psychologist who objects to the use of representations concerning (purported) race-linked innate differences in I.Q. in the formation of education policy. Her criticisms invoke both issues of racial justice and issues of normative epistemology and methodology: these policies ignore the lingering effects of white supremacy and the research ignores possible environmental influences on I.Q. As analyzed from the narrow view, her work seems to require her to move between the roles of pure and applied scientist, even within a single paper. Her methodological point may be well-received, even stimulate radical changes in the field, but the anti-racist point is (or should be) utterly irrelevant and uninfluential. Hence any progress within psychology that results from her work will be purely internal, not partly external.

On the broad view, the psychologist's work does not need to be divided into distinct roles. Her criticisms combine epistemic and pragmatic considerations, and if anything are stronger and more powerful for it. So, if her methodological point is well-received and stimulates radical changes in the field, this may be in part because it is combined with an anti-racist point. In this case, the anti-racist point is quite influential, and the progress that it produces will be purely internal, not partly external. Thus joint practice can be recognized as such, and it can effectively produce

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<sup>2</sup> Assuming, for example, that the materials, equipment, and practical knowledge required for the analysis are inexpensive and in other respects easy to obtain. If testing for lead and other heavy metals were as expensive as, for example, diagnosing lymphoma, joint practitioners might work to develop less expensive tests; if successful, this would be partly external progress with respect to the technology and practical knowledge of water analysis. Further, the new requirement that such tests be inexpensive and so on would constitute partly external progress with respect to the standards for such technology and practical knowledge.

partly external progress.

Next, as discussed in §2.6, the partly external progress produced by joint practice provides an intrinsic justification of other practices. For example, the work of the anti-racist psychologist enables other psychologists to recognize anti-racism as a practice rather than an institution. Furthermore, once this is recognized, there is the prospect that further joint practice can stimulate further partly external practice. The anti-racist critiques of I.Q. might lead other psychologists to speculate that anti-racist critiques can help identify other problematic methodological assumptions in other areas of psychological research. And this last is an instance of the thought that I have called the prospect of progress or (T-2).

In response to the argument, consider Stephen Jay Gould's response to Ruth Bleier's feminist transactionism:

I am also not convinced that many methodological improvements now slowly making their way within science are, as Dr. Bleier argues, especially feminist ways of thinking . . . but [rather] the taxonomic way[s] of thought that naturalists — most of them men — have been urging against reductionistic biology for centuries. All these methodological revisions are now making great headway in my own field of evolution, and men are doing most of the work. We desperately need more women as equal companions in this effort, not because the culture of feminism grants deeper vision, but because we need as many good scientists as we can get.<sup>3</sup>

That is, prior to Bleier's work, scientific inquiry already contained the resources needed to deal with the problems and tensions the feminist critiques pointed out; biology did not need feminist resources to make progress. Since purely internal progress is sufficient, joint practice and partly external progress are unnecessary; the problems with reductionist biology can be both identified and solved using only the resources of scientific inquiry. So the work of joint practitioners can be separated into two asymmetrical roles: the "essential" role of the scientist, whose work stimulates purely internal progress, and the "accidental" role of the feminist, who is at best motivated by her ethical and political values.

This sort of response may be given to the other examples of feminist science discussed in §5.2. These examples, I argued, are best understood as challenges to androcentric standards of explanation. But these challenges were often made by explicit appeal to other standards already accepted in the discipline in question, such as empirical adequacy or coherence. For example, androcentric explanations of the development of agriculture are inconsistent with empirical evidence that women are responsible for the related development of pottery. There was, therefore, no need for specifically feminist critiques of the androcentrism of these explanations, the response goes; the tension would have (or perhaps should have) been obvious to any responsible

<sup>3</sup> Stephen Jay Gould, "Similarities between the Sexes," *New York Times Book Review*, August 12, 1984, my brackets.

archeologist. Perhaps feminists were motivated to develop these criticisms by their values, but the values as such played no role in the logic of the criticism.

But to say that the tools of scientific inquiry are all that is *necessary* to do the job isn't to say that they are the *best* tools for the job, or even that they are more than *minimally adequate*. As Gould acknowledges, biologists had been struggling with reductionism for "centuries," evidently with fairly little success. Perhaps, given time, they would have been successful without any help from feminists. But, in the handful of decades since Gould wrote those words, significant progress appears to have been made in developing robust alternatives to reductionistic biology. Feminist scientists in particular appear to have made some important contributions to this progress. Similarly, the feminist critiques of androcentric explanatory models in archeology and primatology had a rapid and profound influence on those disciplines. Even if partly external progress was not strictly necessary, it has apparently been significantly faster or more effective than purely internal progress. Feminist practices therefore appear to have made a significant, albeit not strictly necessary, contribution to the progress of scientific inquiry.

On the broad view, joint practitioners are not marginalized and their contributions quarantined; their work constitutes one of the internal goods (though not one of the primary internal goods) of scientific inquiry and there is no good reason to think this work may not legitimately influence any other aspect of scientific inquiry, including the standards for the primary internal goods. Joint practitioners are, for these reasons, free to engage in work that stimulates partly external progress. Indeed, on the broad view, the argument for (T-4) — that is, transactionism — seems clear and unproblematic.

### 6.3 Interdependence

In this section, I develop another argument for transactionism from broad view premises. This argument emphasizes the interdependence of the primary internal goods for scientific inquiry: in general and over the long term, producing each of representational knowledge, practical knowledge, and technology requires producing the other two; or, each is a means for producing the others.<sup>4</sup> So call this the *interdependence argument*. This sort of ends-means dependence of one good on another gives reason, I claim, for thinking that the standards of one internal good depend on those of the other: one sort of excellence for *A* is its usefulness for producing *B*. More formally, we might say,

(ID-1\*) For any goods *A* and *B*, if *A* is a means to achieve end *B* then the standards of excellence for *A* are legitimately influenced by the standards of excellence for

<sup>4</sup> See the discussion of ends in §3.5. Note that, in that discussion, I explicitly reject the view that if *x* is a means to achieve end *y* then *y* cannot be a means to achieve end *x*, calling my own view weak symmetry of final ends.

*B.*

However, this formulation is vulnerable to the objection that the standards of excellence for the internal goods of scientific inquiry (and other practices) are *not* legitimately influenced by the standards of excellence for *external* goods. Technology might be utilized as a means to accumulate wealth, but that does not mean that technology should be evaluated by its profitability. This objection is easily met by restricting the scope of the quantifiers to internal goods:

- (ID-1) For any internal goods *A* and *B*, if *A* is a means to achieve end *B* then the standards of excellence for *A* are legitimately influenced by the standards of excellence for *B*.

Furthermore, this does *not* mean that the standards of excellence for *A* are *determined* or *exhausted* by the standards of excellence for *B*. Consider MRI: While it is valuable *in part* as a means to producing new representational knowledge, it is not valuable *only* as such a means. It is also valuable, for example, for its contribution to medical diagnosis.

From the definition of the broad view, we have the following:

- (ID-2) Each of the three primary internal goods is a means to achieve the other two as ends.
- (ID-3) ∴ The standards of excellence for each of the three primary internal goods are legitimately influenced by the standards of excellence for the other two. (ID-1, -2)
- (ID-4) ∴ The standards of excellence for representational knowledge are legitimately influenced by the standards of excellence for practical knowledge and technology. (Instance of ID-3)

On the broad view, representational knowledge is not the unique final end of scientific inquiry. At least sometimes, we pursue and utilize representational knowledge as a means for producing new technology and practical knowledge. For example, Neurath pursued improved representations in sociology and economics as a means for producing new “social technology,” that is, practical knowledge of how to best manage the system of production.

Next, practical knowledge and technology are often means to achieve ethical and political values as ends. To take the example of Neurath again, the managerial practical knowledge was a means to realize the internal goods of movement socialism, economic inclusiveness and economic democracy. Excellent practical knowledge in sociology, on Neurath’s view, is in part practical knowledge that is useful in exactly this way, as means to these socialist ends. Again, this does not mean that the standards for practical knowledge are *determined* by this end. This knowledge must also

be, for example, robust — reliably effective in a wide range of contexts — in a way that does not depend on the ethical and political ends to which it is being used.

Granting (ID-1) and that movement socialism is a practice rather than an institution, it follows that the influence of Neurath's socialism on his standards for practical knowledge is legitimate. More generally:

(ID-5) Practical knowledge and technology are means to achieve ethical and political values as ends.

(ID-6)  $\therefore$  The standards of excellence for practical knowledge and technology are legitimately influenced by ethical and political values. (ID-1, -5)

This application of (ID-1) might be thought problematic: as formulated, it is not clear whether the quantifiers (*any* internal goods) range over all practices or only one practice, require  $A$  and  $B$  to be internal goods of the same practice, or are restricted in some other way. However, as I argued in §2.6, in some (if not many) cases, the success of joint practice involves the influence of one practice on the standards for the internal goods of the other, and that this success makes the influence legitimate. Progress in one practice ( $A$ ) depends on the internal goods of the other practice ( $B$ ), and the upshot of the argument in §2.6 is that this influence of  $B$  on  $A$  is legitimate.

To return to the overall argument of this section, I next need a general principle of what we might call the logic of legitimate influence:

(ID-7) **transitivity:** If the standards of excellence for  $A$  are legitimately influenced by the standards of excellence for  $B$  and the standards of excellence for  $B$  are legitimately influenced by (the standards of excellence for)  $C$  then the standards of excellence for  $A$  are legitimately influenced by (the standards of excellence for)  $C$ .

That is, if influence runs from  $C$  to  $B$  and from  $B$  to  $A$ , it runs from  $C$  to  $A$ . We might say that legitimate influence can be direct or indirect. If there is a direct influence between  $A$  and  $B$  and between  $B$  and  $C$ , then there is an indirect influence between  $A$  and  $C$  by way of  $B$ . As the influence becomes more indirect — as the chain of  $B$ s between  $A$  and  $C$  grows — we would expect the influence to become more attenuated or less significant. But, insofar as there is an influence between the two endpoints, so long as it is legitimate each step of the way, it is legitimate over all.

Granting this transitivity principle, we have our transactionist conclusion:

(ID-8)  $\therefore$  The standards of excellence for representational knowledge are legitimately influenced by ethical and political values. (ID-4, -6, -7)

This conclusion does not imply that *normative epistemology* is legitimately influenced by ethical and political values. The version of transactionism that I have sketched here — which I call *basic broad view transactionism*, and which is represented visually in figure 6.2 — is at least formally compatible with Popperian falsificationism

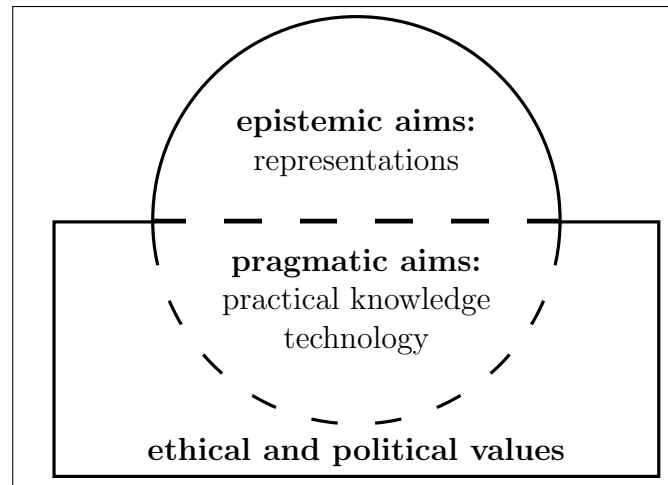


Figure 6.2: Basic Broad View Transactionism

or Bayesianism. On basic broad view transactionism, the standards for representations include *both* epistemic *and* pragmatic standards. Epistemic standards are the standards of normative epistemology — again, pretty much whichever normative epistemology you like. Pragmatic standards are standards of *usefulness* of the representation: how useful it is, for example, for building new technology, and especially new technology that will be useful for promoting some ethical and political values. On this basic version of transactionism, an excellent representation is one that is both epistemologically and pragmatically excellent.

Basic broad view transactionism, as such, is agnostic with respect to any relationship between epistemic and pragmatic standards; they may be completely and utterly independent, or epistemic standards may be grounded, in some sense, in pragmatic standards. This agnosticism is deliberate, so that basic broad view transactionism is compatible with a wide variety of other, more substantive, versions of transactionism. On one end would be versions of transactionism that take epistemic and pragmatic standards to be more-or-less independent.<sup>5</sup> On the other end would be versions that take epistemic standards to be more-or-less means to pragmatic ends.<sup>6</sup> I introduce basic broad view transactionism as a sort of common divisor: a version of transactionism that anyone who accepts the broad view is liable to accept.

<sup>5</sup> See, for example Janet Kourany, *Philosophy of Science after Feminism* (Oxford and New York: Oxford University Press, 2010), ISBN: 9780199732616.

<sup>6</sup> See, for example, the discussions of Cartwright in §3.4 and the discussion of Dewey in §5.6.



## 6.4 Pure and applied

In this section, I discuss the distinction between pure and applied science. As we saw with the connection argument in chapter 4, there is a deep connection between isolationism and a certain asymmetrical view of the relationship between pure and applied scientific inquiry. One of the three abductive arguments that I offered for the connection hypothesis was that it explains why isolationists assume the narrow view when they distinguish pure and applied scientific inquiry in arguments against transactionism. As we saw in that discussion, these narrow view versions of the distinction support isolationism by prioritizing “pure” representational knowledge over its “applied” application. Isolationists might instead have offered broad view version of the distinction — except that, the connection hypothesis predicts, such versions of the distinction would support transactionism instead.

However, this analysis assumes that there are broad view versions of the pure/applied distinction. It might be thought that the distinction is necessarily a narrow view distinction; that is, that the distinction between pure and applied scientific inquiry is only intelligible on the narrow view. If that is right, then we do not need to explain why isolationists appeal to narrow view versions of the distinction — *all* versions of the distinction are narrow view versions. Furthermore, the plausibility of the pure/applied distinction is as serious a problem for transactionists as isolationists think. So, in this section, I offer broad view versions of the distinction, and show how each is compatible with transactionism. Key to the analysis, in each case, is that, on the broad view, the three primary internal goods of scientific inquiry are distinct yet equal. This gives us different ways of making a distinction between pure and applied scientific inquiry without prioritizing the pure side. I find it convenient to structure my discussion in parallel with the three forms of the distinction developed on the narrow view in §4.2: activities, significance, and attitudes. It follows that there is something to explain in the science and values literature — why isolationists assume only narrow view versions of the distinction — and that this distinction is not as such a problem for transactionists.

### 6.4.1 Activities

The first form of the distinction, discussed in §4.2.1, is a distinction between the activities of developing representational knowledge and applying that knowledge to pragmatic aims. Recall that, on the narrow view, application is best understood in terms of the utilization of representations to produce external goods; on the broad view, by contrast, and as we saw in §3.4.3, application is understood in terms of joint practice. The pure side of this distinction is thus what we might call *isolated scientific inquiry* — scientific inquiry that does not incorporate or is not aimed at promoting either the internal goods of any other practices or any external goods.

Note that this form of pure scientific inquiry does not correspond to any division

between representational knowledge and other internal goods. Pure scientific inquiry, in this sense, can perfectly consistently focus on practical knowledge and technology and either temporarily neglect the production of representational knowledge completely or treat representational knowledge strictly as a means to the practical end. For example, in Hasok Chang's account of the history of thermometry, developing a reliable thermometer required developing a thermometric scale with fixed points for, for example, the freezing and boiling points of water. The scale is a non-trivial representation: it assumes that there is a determinate, fixed temperature at which all water changes state. But this representation, as epistemologically interesting as it is, was not pursued for its own sake or as a final end. Rather, it was pursued for the sake of building the reliable thermometer. This research was pure scientific inquiry, in the sense of not incorporating the ends of any other practices; but it was focused on technology, and the representational knowledge it produced was treated as a means to the end.<sup>7</sup>

This form of the distinction does not imply that pure or isolated scientific inquiry is deficient or inferior to applied scientific inquiry, any more than a line of scientific inquiry that does not promote the internal good of progress is for that reason deficient or inferior. Consider two lines of research, *A* and *B*. *A* is extremely focused, and aims to produce just a small subset of the internal goods of scientific inquiry — say representational knowledge of a very specific phenomenon and an instrument for measuring it. While *B* is interested in this same phenomenon, it is much less focused and aims at representational knowledge and stimulating progress. For the sake of progress, *B* incorporates individuals with a wide variety of backgrounds; *A*, by contrast, is a small group of dedicated specialists. It is reasonable to expect that the participants in *B* would need to devote significant resources to developing common forms of activity, vocabulary, and standards for representational knowledge: by virtue of their different backgrounds, the participants in *B* may have very different ideas of how to construct and analyze a dataset, for example, especially when compared to *A*. While the work of *B* is more likely to stimulate progress than *A*, it will probably not produce as much or as excellent representational knowledge as *A*. Neither *A* — pure scientific inquiry — nor *B* — applied scientific inquiry — is, by virtue of its aims and prospects, deficient or inferior compared to the other. We might say that the *A* and *B* are roughly equal or (because they are focused, for the most part, on distinct internal goods) incommensurable.

Or, better still, *A* and *B* are complementary. The work of *B* promotes the internal good of progress in ways that *A* cannot; but the specialization of *A* makes it reasonable to expect *A* to be more efficient at producing familiar kinds of technology, say, than *B*. The focus and efficiency of pure scientific inquiry complements the innovations of applied joint practice. Indeed, one might even argue that these two forms of scientific inquiry are interdependent in much the same way that the three primary internal

<sup>7</sup> Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford and New York: Oxford University Press, 2004), especially ch. 1, ISBN: 0195171276.

goods of scientific inquiry are interdependent: over the long term, each generally requires the others to flourish.

### 6.4.2 Significance

The second form of the distinction between pure and applied scientific inquiry, discussed in §4.2.2, is a distinction between epistemic and pragmatic significance or value. As we saw there, the goods-oriented narrow view takes epistemic significance to be more important than pragmatic significance.

On the broad view, due to the interdependence of the three primary internal goods, a distinction between epistemic and pragmatic significance seems to be at least difficult to maintain. Consider a representation of a handful of climatic processes, such as the interactions between atmospheric chemistry, atmospheric temperature, ocean temperature, and the solubility of methane in seawater; call it the *methane solubility model*.<sup>8</sup> This representation has clear pragmatic significance, insofar as the practical knowledge of managing these processes (and the technology this practical knowledge requires, of course) is important for anticipating and mitigating some of the effects of climate change. It also has epistemic significance because it provides understanding of these climatic processes.

But these two kinds of significance of the methane solubility model either cannot be distinguished or cannot be separated. The methane solubility model has pragmatic significance — enables practical knowledge and the development of new technology — *because* of its epistemic significance — because of the understanding it provides. More generally, it seems that elements of the interdependence argument of §6.3 can be modified and arranged to imply that epistemic and pragmatic significance cannot be distinguished or separated. In these sorts of situations, we will say that epistemic and pragmatic significance are *entangled*.

It may be objected that pragmatic significance is context-dependent in a way that epistemic significance is not. For example, the methane solubility model might be considered epistemically significant because it is an accurate representation of certain climatic processes, or because it promotes our understanding of these processes, or both. It is pragmatically significant because it enables us to deal with a pressing practical problem, climate change. Further, if climate change were not a pressing problem, the methane solubility model would not be pragmatically significant. Hence, (a) its pragmatic significance is context-dependent — dependent on its relation to some problem or another that is “in the context.” However, even if climate change were not a pressing problem — and hence, even if the methane solubility model were not pragmatically significant — it would still be accurate and promote understanding. It would, that is, still be epistemically significant. Hence (b) epistemic significance is

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<sup>8</sup> Very roughly, as I understand it, warmer seawater is worse at holding methane in solution, and so, as the oceans warm, methane will be released into the atmosphere.

not context-dependent, unlike pragmatic significance. So the two are not entangled even in this sort of case.

The first problem with this response is that it does not provide good support for the claim (b) that epistemic significance is not context-dependent. The example may show that epistemic significance is independent of *some* features of the context. But there may be *other* features of the context on which it does depend, such as the acceptance of certain kinds of explanatory models.<sup>9</sup> Charitably, this response does seem to support the claim that epistemic significance is *less* context-dependent than pragmatic significance. We might gloss this position as the pair of claims that (a') pragmatic significance is *strongly* context-dependent while (b') epistemic significance is *weakly* context-dependent.

The second problem with this response is the inference from the claim that accuracy and understanding are weakly context-dependent to the claim that epistemic significance is weakly context-dependent. This inference is plausible only to the extent that accuracy and understanding are the only two standards for excellent representational knowledge. Consider Kuhn's theoretical virtue of fruitfulness: "a theory should be fruitful of new research findings: it should, that is, disclose new phenomena or previously unnoted relationships among those already known."<sup>10</sup> Fruitfulness is, I take it, strongly context-dependent: whether or not a new representation is fruitful depends on, among other things, what other representations are available and in use at that time. Similarly, coherence or external consistency is strongly context-dependent.<sup>11</sup> The methane solubility model is certainly epistemically significant in part because it is accurate and promotes understanding. But it is also epistemically significant in part because (or to the extent that) it is fruitful and coheres with other representations of the same phenomena. Further, if Longino's criteria of objectivity are included among the standards, epistemic significance would appear to be strongly context-dependent, insofar as it would depend in part on the demographics of scientists and the power structure of scientific inquiry.<sup>12</sup> In general, I suggest, the inference is only plausible to the extent that the standards for representational knowledge are production-independent.<sup>13</sup>

A better response to the argument is to note that widespread entanglement is a much stronger conclusion than the example actually supports. Certainly, in the methane solubility model and numerous other cases, epistemic and pragmatic significance are entangled. But this sort of phenomenon leads to the modest claim that epistemic and pragmatic significance *mutually influence* each other, not that there is *no distinction* between them. To say that we sometimes care about epistemic things

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<sup>9</sup> See §5.2.

<sup>10</sup> Kuhn, "Objectivity, Value Judgment, and Theory Choice," 322.

<sup>11</sup> *Ibid.*

<sup>12</sup> Longino, *Science as Social Knowledge*, ch. 4.

<sup>13</sup> For the distinction between production-dependent and production-independent standards, see §3.2.2.

because they are practically useful, or about pragmatic things because they help us improve our knowledge, and so the significance of each influences the significance of the other, is not to say that there is no difference between epistemic and pragmatic.

But the claim of mutual influence is all a transactionist needs, strictly speaking. With this version of the pure/applied distinction, we might say that pure scientific inquiry is strictly epistemically significant; and applied scientific inquiry is strictly pragmatically significant; but most inquiry will be significant in both respects, and so most inquiry is of “mixed” significance, somewhere between the extremes of pure and applied. On this version of the distinction, pure and applied are ends of a spectrum rather than discrete categories.

### 6.4.3 Attitudes

The third form of the distinction between pure and applied scientific inquiry, discussed in §4.2.3, is a distinction between a particularly epistemic attitude of acceptance towards a representation and other attitudes, especially such pragmatic attitudes as Lacey’s example of “applying them [the representations] in practical life.”<sup>14</sup> As with the other two forms of the distinction, the goods-oriented narrow view takes the epistemic attitude to be more important than pragmatic attitudes, and I discussed at some length how this supports Lacey’s version of isolationism.

Heather Douglas — whose transactionism was briefly discussed in §4.1.1 — has developed an important alternative to the narrow view’s conception of the relation between epistemic and pragmatic attitudes. In recent work, Douglas has defended a distinction between *evidence* and *values*.<sup>15</sup> Evidence, on her view, provides warrant or, we might say, supports the attitude of *acceptance* towards a representation.<sup>16</sup> Values, by contrast, support other attitudes directly and play an “indirect role” in supporting acceptance, but do not play the “direct role” of supporting acceptance that evidence does.

Douglas introduces four nominal categories of “values”: ethical, social, cognitive, and epistemic.<sup>17</sup> Epistemic values include internal consistency, predictive competence, and empirical adequacy; a representation that does not exemplify these values is not much of a representation at all.<sup>18</sup> Consequently, Douglas maintains that epistemic

<sup>14</sup> Lacey, *Is Science Value Free?* 13.

<sup>15</sup> Douglas’s terminology here is meant to be open-ended and inclusive, far more so than mine. For purposes of summarizing her view, I will generally use her terminology; I will return to my terminology as I place her view in the context of the science and values debate as seen from the perspective of the broad view of science as practice.

<sup>16</sup> Heather Douglas, “Norms for Values in Scientific Belief Acceptance” (Paper contributed to the Philosophy of Science Association’s 20th Biennial Meeting. Available at <http://philsci-archive.pitt.edu/3024/>. 2006), 4.

<sup>17</sup> Douglas, *Science, Policy, and the Value-Free Ideal*, 92-5.

<sup>18</sup> Douglas, “Norms for Values in Scientific Belief Acceptance,” 6; Douglas, *Science, Policy, and the Value-Free Ideal*, 94.

“values” are not actually values; they are more like necessary conditions for minimal representational adequacy. Ethical values are characterized as “focus[ing] on the good or the right,” while “social values arise from what a particular society values”; as Douglas points out, “often social values will overlap with ethical values.”<sup>19</sup> From the perspective of ethics, it’s difficult to see just what Douglas is trying to do with the distinction between ethical and social values. And the distinction doesn’t seem important to her argument in any case. Cognitive values are “those aspects of scientific work that help one think through the evidential and inferential aspects of one’s theories and data,” and include many of the values on Kuhn’s list: simplicity, explanatory power, scope, external consistency, and fruitfulness.<sup>20</sup> So, all together, I will read her four nominal categories of values as two categories, *ethical* (including social values) and *cognitive*, with epistemic “values” set to one side for this discussion. Even the distinction between ethical and cognitive values is not a sharp one, since Douglas accepts Longino’s no distinction argument for transactionism.<sup>21</sup>

Douglas’s distinction between evidence and values is quite sharp, however, and this distinction is captured well in terms of the different kinds of attitudes that these two support. Again, evidence supports the epistemic attitude of acceptance, and it does so *directly* or in the *direct role*: evidence gives reason to adopt this attitude. Values — by contrast with evidence, and including both ethical and cognitive values — do not give reason to adopt this attitude. However, values do play this direct role of giving reason to adopt other attitudes, such as the attitude that a line of research is pursuitworthy, that a methodology is *ethically* (distinct from *epistemically*) acceptable, or that a representation is practically applicable to some problem.<sup>22</sup> Consider, for example, Douglas’ analysis of the values of simplicity and explanatory power:

any flaws in the [simple] theory are more likely to be found sooner rather than later because the theory is easier to think with, and the theory is easier to expand into new areas . . . . [and] [e]xplanatory power or scope should enable scientists to make interesting new predictions, which can then be used to test the theory.<sup>23</sup>

Consider two rival representations *A* and *B*, and suppose that there is not yet sufficient evidence to support the acceptance of either. Further, suppose *A* is simpler or more explanatorily powerful or both compared to *B*. The simplicity or explanatory power

<sup>19</sup> Douglas, *Science, Policy, and the Value-Free Ideal*, 92.

<sup>20</sup> Douglas, “Norms for Values in Scientific Belief Acceptance,” 6; Douglas, *Science, Policy, and the Value-Free Ideal*, 93; for explanation, see especially Heather Douglas, “Reintroducing Prediction to Explanation,” *Philosophy of Science* 76, no. 4 (October 2009): 444–63.

<sup>21</sup> Douglas, *Science, Policy, and the Value-Free Ideal*, 90-1; note that Douglas’s own argument is closer to underdetermination arguments.

<sup>22</sup> *Ibid.*, 98-103.

<sup>23</sup> Douglas, “Norms for Values in Scientific Belief Acceptance,” 6.



of  $A$  does not give reason to accept  $A$ ; that would be to confuse these values with evidence. But they do support adopting  $A$  as a tentative hypothesis for further investigation and development – they support the pursuitworthiness of  $A$ .

Still more concretely, suppose a given line of toxicology research on BPA is expected to make at least some modest contribution to our understanding of the endocrine disruption effects of this chemical — that is, pursuing this research would be conducive to some of our ethical values — would not require an excessive amount of time and money, and would not involve inflicting excessive suffering on human and non-human animals. Then these various values — explanatory power, practical applicability, ethical acceptability, “economic” considerations — directly support the attitude that we should pursue this research. These values provide reasons in a *practical* syllogism. But they do not provide reasons to believe that the claims made in this research are true; they do not provide reasons in a theoretical, veritistic, or epistemic syllogism to support the epistemic attitude of acceptance.<sup>24</sup>

However, on Douglas’s view, values also play an *indirect role* in relation to the epistemic attitude of acceptance: they “play a legitimate role in establishing what constitutes sufficient warrant in a particular case, in determining what level of uncertainty is acceptable in that case, given concerns over the consequences of error,”<sup>25</sup> that is, values legitimately influence how much evidence or what degree or kind of evidence is sufficient to support acceptance of a representation. Values act in this way in the argument discussed in §4.1.1. For example, values determine the relative importance of human life and economic productivity, which in turn legitimately influences the standards of statistical significance sufficient to accept one of various representations of the toxicology of BPA.

Douglas’s distinction between evidence and values and her account of the distinct roles they play might be read as a version of the asymmetrical distinction between the particularly epistemic attitude of acceptance — supported by evidence — and

<sup>24</sup> Compare this with Dan McKaughan’s analysis of pursuitworthiness, Daniel McKaughan, “Toward a Richard Vocabulary for Epistemic Attitudes: Mapping the Cognitive Landscape” (PhD diss., University of Notre Dame, 2007), ch. 4 and Daniel McKaughan, “From Ugly Ducking to Swan: C.S. Peirce, Abduction, and the Pursuit of Scientific Theories,” *Transactions of the Charles S. Peirce Society* 44, no. 3 (Summer 2008): 446–68. Kevin Elliott has attempted to clarify Douglas’s distinction between the direct and indirect roles, with some attention to Douglas’s use of the term “reason.” Kevin Elliott, “Direct and Indirect Roles for Values in Science,” *Philosophy of Science* 78, no. 2 (April 2011): 303–24, doi:[10.1086/659222](https://doi.org/10.1086/659222) However, Elliott does not seem to consider casting the distinction, I have done, in terms of the distinction between practical and theoretical reason. For a version of transactionism that combines McKaughan’s and Elliott’s work, see Kevin Elliott and Daniel McKaughan, “How Values in Scientific Discovery and Pursuit Alter Theory Appraisal,” *Philosophy of Science* 76, no. 5 (December 2009): 598–611, doi:[10.1086/605807](https://doi.org/10.1086/605807).

<sup>25</sup> Douglas, “Norms for Values in Scientific Belief Acceptance,” 5; Douglas, *Science, Policy, and the Value-Free Ideal*, cf. 101ff.



other attitudes — including pragmatic attitudes, supported in part by values. That is, Douglas’s account might be read as an isolationist account.

While she does, like many isolationists, distinguish between acceptance and other attitudes, she does not “isolate” the former from the latter, or give it any sort of priority or importance. Again, the standards for acceptance are set, at least in part, by values — including ethical and political values. At least some of these values can be represented as ethical and political values in my sense, namely, as internal goods of ethical and political practices. For example, the ethical and political values involved in setting standards for the acceptance of representations of the toxicology of BPA include intentional attitudes of promoting clean and healthy water; which, in turn, are among the internal goods of the environmentalist movement conceived as a practice. In short, ethical and political practices, as standard setters, legitimately influence the epistemic attitude of acceptance, albeit indirectly. So while Douglas does make a distinction between epistemic and non-epistemic attitudes, the two kinds of attitudes legitimately influence each other, and in this sense are related symmetrically.

Contrast this reading of Douglas with Lacey’s view. On his view, epistemic attitudes legitimately influence non-epistemic attitudes. For example, acceptance of a theory gives us good reason to adopt the theory for practical use. But — and this is what makes his view isolationist — non-epistemic attitudes do not legitimately influence epistemic attitudes. The practical utility of a theory gives us absolutely no reason whatsoever to accept it. Lacey’s view has the asymmetry between epistemic and pragmatic that is characteristic of isolationism; while Douglas’s view has the symmetry between epistemic and pragmatic that is characteristic of transactionism.

In this section, I have examined three forms that the pure/applied distinction might take on the broad view. I have shown that, in each case, because of the interdependence and equality of the three primary internal goods, the distinction is compatible with broad view transactionism. Thus the distinction is conceptually coherent with the broad view, and there is a need to explain why isolationists assume only narrow view versions of the distinction. I have offered the connection hypothesis as an explanation: isolationists assume only narrow view versions because these versions, in contrast with broad view versions, support isolationism.

## 6.5 The threat of domination

As we saw in chapter 4, the threat of domination is one of the most prominent arguments for isolationism in both the popular and academic literature dealing with the science and values debate, and, as per the connection argument, it is the way that the narrow view of science as practice leads to a perceived general threat of domination that makes isolationism so plausible on the narrow view. But how does this threat look on the broad view? Does it loom so large that the possibility must be avoided at all costs? Or is it more modest, and perhaps best conceived as a problem to be dealt with, in the first instance, by practitioners?

Transactionists must be able to address the threat of domination, and in particular must give an account of the legitimate ways ethical and political practices can influence scientific inquiry and the illegitimate kinds of influence that lead to or constitute institutional domination. This account cannot be simply conceptual or abstract. It must be operationalizable — something scientists can actually use when considering whether or not to pursue joint practice (or what may be joint practice) with some other (purported) practice.<sup>26</sup> To this end, the account should provide some useful conceptual tools: to judge reliably whether or not the purported practice actually is a practice, and if so, whether or not or to what extent it suffers institutional domination; to identify at least some of the most prominent ways in which transaction can lead to partly external progress and institutional domination; to suggest ways of preventing and counteracting institutional domination; and so on. And this, in turn, requires a fairly robust and sophisticated empirical sociology of joint practice and other kinds of interaction between scientific inquiry, other practices, and institutions.

This dissertation has only sketched the needed account, and it is not as empirically robust as one would like. But, as my references to sociologists, historians, journalists, and joint practitioners writing self-reflexively about their joint practice (and thereby playing the role of social scientists) throughout this dissertation show, empirical investigation into, for example, the institutional domination of scientific inquiry is already underway.<sup>27</sup> It has been conducted without much of an explicit or systematic conception of scientific practice, but this implies neither that the empirical investigation could not be improved by adopting some or all of the conception of practice nor that previous empirical investigation is completely irrelevant to the claims I am making in terms of the conception of practice.

It may be objected that partly external progress and institutional domination are not strictly mutually exclusive. One is not defined as the negation of the other; isn't it possible, therefore, that the influence of ethical and political practices could lead to *both* partly external progress and institutional domination? A complicated interpretation of Nazi science — and Jordan's work in particular — might provide an example of this.<sup>28</sup>

Suppose certain aspects of the Nazi movement — say, romantic ideals of nationalist cultural revival that stimulated objections to “dehumanizing” versions of mechanism and reductionism in biology and the human sciences — were internal goods, and thus

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<sup>26</sup> This requirement comes from my own pragmatist commitments and methodology. I take it that many philosophers would be content with a purely conceptual account of the distinction.

<sup>27</sup> Recent important examples include Robert Proctor, ed., *Agnotology* (Stanford: Stanford University Press, 2008), ISBN: 9780804756525; Naomi Oreskes and Erik M. Conway, *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (New York: Bloomsbury Press, 2010), ISBN: 9781596916104; Elliott, *Is a Little Pollution Good for You?*

<sup>28</sup> See §5.5.

at least part of the Nazi movement was, indeed, a practice. But suppose as well that the Nazi movement, due to its racism and fetishization of power, suffered an especially pervasive form of institutional domination throughout its entire existence, one on which certain internal goods (say, romanticist cultural revival) were mistakenly identified with certain external goods (say, *Lebensraum* and authoritarian power). This could explain — in terms of an especially “deep” kind of institutional conflict — at least some of the internal tensions that plagued both the Nazi movement and the Nazi state, including its simultaneous antimodern romanticism and hypermodern focus on such external goods as military conquest and nationalist glory.<sup>29</sup>

If this is right, then any influence the Nazi movement would have on scientific inquiry could be expected to lead to both partly external progress and institutional domination. Consider Jordan’s use of metaphors of spontaneous choice in his interpretation of quantum indeterminacy. “Spontaneous choice,” on this understanding of the Nazi movement, should be understood in terms of both internal goods — the spontaneous self-expression of romanticist humanism — and external goods — the unchallengeable decision-making power of a dictator. Its utilization in physics therefore likewise should be understood in terms of both internal goods and external goods — that is, as both partly external progress and institutional domination. A solution to a significant problem of physics — the (apparent) need for an understandable interpretation of quantum mechanics — comes, in part, from an external practice, but it requires incorporating physics into a militaristic and oppressive movement for domestic and internal political power. This partly external progress, as good as it is, comes with an unacceptable cost.

This line of thought requires a significant concession to the transactionist: partly external progress, as such, is genuine and requires joint practice; the line of thought just claims that it is, as it were, outweighed by harms that accompany it necessarily. In order to generalize this — to all or most ethical and political practices, for example — an isolationist would have to claim that the especially “deep” form of institutional domination suffered by the Nazi movement is also quite common — of all or most ethical and political practices. I suggest, first, that this puts the burden of proof on the isolationist, and second, that this requires exactly the sort of empirical historical and sociological investigation that I am recommending. If partly external progress is conceded to be a *pro tanto* good then the burden is on the isolationist to show that it is always, mostly, generally, and so on, actually outweighed by the threat of domination. *A priori* philosophical inquiry suggests important questions for empirical investigation, but cannot give answers itself.

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<sup>29</sup> Beyler, “[Targeting the Organism](#),” 250-1.

## 6.6 The idiom of the narrow view

Because of its widespread acceptance among philosophers, many contemporary transactionists present their views and arguments in terms of the narrow view, with relatively little explicit attention to practical knowledge and technology. Consequently, transactionists who adopt the idiom of the narrow view must fight against both the causal tendency towards isolationism — as described by the connection argument — and assumptions built on the narrow view — such as the asymmetrical relation between pure and applied scientific inquiry. This makes it seem as though transactionist views are not just implausible on their face — that is, that isolationism is obvious and transactionism bears the burden of proof — but also that transactionist views are highly vulnerable to simple objections and misreadings. In this section, I support this analysis with a reconstruction of Douglas’s argument for transactionism.

Douglas writes almost, but not quite, exclusively in the idiom of the narrow view. Consider her discussion of objectivity, which begins by recognizing that “we call many different kinds of things ‘objective’ — objective knowledge, objective methods, objective people, objective observations, and objective criteria, to name a few.”<sup>30</sup> Objective methods might be read as practical knowledge, or a standard of excellence for practical knowledge — as a method, this know-how is objective. And nothing Douglas says rules out adding objective instruments to her list; these instruments would be technology, of course. So, at first, it is plausible to think that she is working with the broad view. But quickly she

focus[es] on the objectivity of knowledge claims, and the processes that produce these claims, rather than the objectivity of persons, panels, or procedures per se. For my purposes, whether a claim can be considered objective, and thus trustworthy, is what we want to gauge. *An objective person who makes no claims or an objective method that produces no claims is of little interest for the policy process.*<sup>31</sup>

Persons, practical knowledge, and technology are only objective in an equivocal or secondary sense; it is representational knowledge, and its production, that is objective in the primary sense. Hence Douglas seems to take the narrow view. The upshot of my analysis in this section is that this is an infelicity on Douglas’s part, since writing in the idiom of the narrow view seems to make her transactionism vulnerable to simple refutations. Note that the narrow view claim, emphasized in the block quotation, was not necessary for her argument. She easily could have said that, for the purposes of the discussion, she was going to focus on objective claims, leaving objective persons and objective methods and so on for another time. This sort of wording would not commit her — at least not so obviously or strongly — to the narrow view.

<sup>30</sup> Douglas, *Science, Policy, and the Value-Free Ideal*, 115.

<sup>31</sup> *Ibid.*, 116, my emphasis.

Now, consider the following, “bare-bones” version of Douglas’s inductive risk argument for transactionism:

- (D-1) Standards of sufficient evidence depend in part on assessments of inductive risk.
- (D-2) Assessments of inductive risk depend in part on ethical and political values.
- (D-3) ∴ Standards of sufficient evidence depend in part on ethical and political values.

The term *inductive risk* is borrowed from Hempel and generalizes both type I and type II statistical errors, that is, the errors of rejecting a true claim and accepting a false claim, respectively.<sup>32</sup> In setting standards of sufficient evidence — to use the familiar example, setting *p*-value thresholds for statistical significance — we must take into account the risks associated with both setting the standard so high (setting the *p*-value threshold so low) that a real effect is rejected as apparently statistically insignificant and setting the standard so low (setting the *p*-value threshold so high) that a spurious or accidental effect is accepted as apparently statistically significant; thus (D-1). A nice argument for (D-2) is borrowed from Hempel: “For some cases, acceptance (or rejection) of a hypothesis will lead to a particular course of action and outcomes with non-epistemic effects. In these cases, the outcomes of the potential actions need to be evaluated using non-epistemic values in order to formulate rules of acceptance.”<sup>33</sup> That is,

- (D-2a) (In some or many cases, or in general) assessments of inductive risk depend in part on assessments of the technology and practical knowledge that the representation in question will be used to produce.
- (D-2b) Assessments of technology and practical knowledge depend in part on ethical and political values.
- (D-2) ∴ Assessments of inductive risk depend in part on ethical and political values.

The crucial premise here is (D-2a). In cases where the representation is expected to be used to produce technology and practical knowledge, it seems clearly true: the inductive risk for a claim that BPA is safe for human consumption depends in part on whether it will be used in baby bottles. In other cases, where there is no such expectation — or where pragmatic uses of the representation are accidental — (D-2a) seems clearly false, and the argument to (D-3) is unsound.

On the narrow view’s conception of applied science as application to external goods, representational knowledge is only produced for the sake of practical knowledge and technology under conditions of institutional domination. (D-2a) thus presupposes

<sup>32</sup> Heather Douglas, “Inductive Risk and Values in Science,” *Philosophy of Science* 67, no. 4 (2000): 561; Douglas, *Science, Policy, and the Value-Free Ideal*, 104-6.

<sup>33</sup> Douglas, “Inductive Risk and Values in Science,” 562.

that scientific inquiry, in some or many cases or in general (depending on the qualifier), suffers from institutional domination. So, on this view, Douglas's argument does not show how science *should* operate, or how ethical and political values can play a *legitimate* role in setting the standards for representational knowledge. Rather, it shows how those standards become *corrupted* by institutional domination: the purely epistemic considerations come under the inappropriate influence of such pragmatic considerations as ethical and political values.

For an example of this misreading of Douglas, consider her analysis of the recent history of science policy in the United States, arguing that “defining and maintaining a clear boundary between” scientific inquiry and policymaking “has proven notoriously difficult” — I will call this the *practical demarcation problem*.<sup>34</sup> Douglas characterizes the practical demarcation problem using her version of transactionism: since epistemic and pragmatic attitudes influence each other, they cannot be demarcated in a practical way, and so we require new organizations for science-informed public policy. In a critical response, Sandra Mitchell rejects Douglas's transactionist account of the problem. Instead, she gives an isolationist account in terms of institutional domination: In the particular cases Douglas discusses, demarcation was problematic because

political interests or the government itself [namely, the Nixon administration], by not listening to scientists, could illegitimately influence [science policy] advisory board members. The insistence on replacing committee confidentiality with disclosure and the attempt to protect [the] ‘independent judgment’ [of scientists] highlights the political character of the executive branch of the government.<sup>35</sup>

That is, the notoriously corrupt Nixon administration did not shy away from the institutional domination of science policy advisory committees, and it was this illegitimate institutional domination, not the legitimate transaction of epistemic and pragmatic attitudes, that made practical demarcation a problem.

Contrast Mitchell's reading of Douglas with a broad view reading. On the broad view, the interdependence of representational knowledge, practical knowledge, and technology means that, in many cases, representational knowledge is pursued for the sake of practical knowledge and technology; depending on the qualifier, (D-2a) is more-or-less completely uncontroversial. Indeed, Douglas's work identifies an important mechanism by which joint practice leads to partly external progress on a small

<sup>34</sup> Heather Douglas, “Border Skirmishes between Science and Policy: Autonomy, Responsibility, and Values,” in *Science, Values, and Objectivity* (University of Pittsburgh Press, 2004), 220–44. The *practical demarcation problem* is to be distinguished from the *conceptual demarcation problem* of distinguishing “science” and “pseudo-science.” Also, the practical demarcation problem can be understood in terms of any of the three forms of the distinction between pure and applied scientific inquiry.

<sup>35</sup> Mitchell, “The Prescribed and Proscribed Values in Science Policy,” 248.



scale: one can readily imagine environmentalists who work in toxicology and epidemiology arguing, in part on the basis of the internal goods of the environmental movement, for lower standards for statistical significance in toxicological assessments and different models of dose-response relationships.<sup>36</sup>

All together, on a simple and superficial narrow view reading, Douglas's argument seems to have fatal problems; but on a deeper and more charitable broad view reading, her argument seems basically sound. By putting her point in terms of the narrow view, Douglas's position is rhetorically weaker than it needs to be.

## 6.7 Reconsidering science and values

Over the last several chapters, I have shown that the science and values debate, when considered from the perspective of practice, is not primarily a debate over epistemology. Certainly epistemological considerations are at the center of the debate. But the full extent of the basic disagreements between the two sides cannot be understood independently of numerous other considerations: the ever-fraught relationship between practices and institutions, internal goods and external goods; the aims and legitimacy of various social movements; the way in which scientific inquiry is funded; the professional status of applied science and engineering; and the aims of scientific inquiry itself. In short, the epistemological issue must be understood in its social context. Knowledge does not, as it were, float freely in Plato's heaven. It is a product of human activity; it is a *concrete* product of human activity; it is a concrete product of human *social* activity; and it is *one among many* concrete products of human social activity.

Let me close this chapter with some suggestions for my fellow transactionists. First and foremost, stop focusing narrowly on epistemology. The epistemological debate has well-worn arguments and replies on both sides; I know of no groundbreaking new epistemological arguments from either transactionists or isolationists of the last several years, and I do not believe that a new consensus is liable to form around the latest variation on underdetermination. This is not to say that we should completely forget or stop talking about the epistemological issues. Rather, we should put them in the richer context of an examination of practices, with special attention to the collaborative social structure of such practices. An empiricist and naturalist — in the senses explained in the introduction to this dissertation — investigation of knowledge-producing practices in their social and economic environment would be quite fruitful.

Consider philosophical work on social epistemology.<sup>37</sup> At one level of description,

<sup>36</sup> Cf. Douglas, “[Inductive Risk and Values in Science](#),” §6.

<sup>37</sup> For an overview and citations, see Alvin Goldman, “Social Epistemology,” *The Stanford Encyclopedia of Philosophy (Summer 2010 Edition)*, 2010, <http://plato.stanford.edu/archives/sum2010/entries/epistemology-social/> (accessed February 13, 2012), especially §§1, 3-5.



it seems to be just what I'm asking for: the study of the ways in which knowledge is created through the interactions of epistemic agents. For example, Miranda Fricker's well-received work on epistemic injustice analyzes ways in which stereotypes marginalize or exclude individuals from activities of knowledge production. The epistemic agency of these individuals — their ability to contribute to these activities — is thereby curtailed, and in some sense this does them an injustice.<sup>38</sup>

But Fricker's account is "social" and involves knowledge "production" in only the most minimal senses. Her paradigm is testimony: the witness  $S_1$  makes some claim  $p$  to the audience  $S_2$ , and  $S_2$  must decide whether or not to accept  $p$ . Her two recurrent examples are taken from novels: Tom Robinson's court testimony in *To Kill a Mockingbird* (an African-American to anonymous white jurors in the Jim Crow South) and the sexist dismissal of Margie Sherwood's claims about her missing fiancé in *The Talented Mr. Ripley*. In both of these examples, the witness  $S_1$  is a single individual and the audience acts as a single individual  $S_2$ . In the Tom Robinson case, Fricker moves seamlessly between the psychology of individuals and the psychology of "the jury," and at no point does she seem to consider the jury's collaborative process of deliberation.<sup>39</sup> Thus the paradigm seems to involve exactly two individual epistemic agents, and knowledge is communicated from one to the other rather than produced collaboratively. Fricker's model does incorporate the broader social context in which the interaction takes place, but again only minimally: by way of "the social imagination" — a term that is never explained, but appears to involve some sort of supra-individual collective mind — the social context causes the audience to form stereotypes ("identity prejudices") — the mechanism here is also never explained — and these stereotypes play a role in weighting the credibility of the witness. In each of the examples,  $S_1$ 's testimony is rejected due to the weighting effects of racist or sexist stereotypes, respectively, in the mind of  $S_2$ . Fricker's entire story could be told, without any modification, about agents under the influence of a Cartesian evil demon or malicious hypnotist or who are brains in vats, communicating electronically.<sup>40</sup> Her account does not seem helpful at all for analyzing and responding to something like politicization and commercialization.

Much of other recent work in philosophical social epistemology is limited in similar ways. While this work is an improvement over the tradition of individualist, abstract epistemology, it is not much of an improvement. The problem, I suggest, is that social epistemologists are taking most of their aims, assumptions, and methods from individualist epistemology. That is, they start with epistemology, as traditionally understood, and make it a little more social. And they do not, by contrast, start with a rich social theory and ask how the activities of knowledge production can be described in terms of this theory.

<sup>38</sup> Miranda Fricker, *Epistemic Injustice* (Oxford University Press, 2007).

<sup>39</sup> *Ibid.*, 23-8.

<sup>40</sup> *Ibid.*, 30-41, does nominally explain the way in which stereotypes are formed, but actually deals almost exclusively with the role of stereotypes in weighting credibility.

Consider once again Fricker's notion of epistemic injustice, this time understood in terms of something like the conception of practice and informed by empirical research rather than novels. Most fields of scientific inquiry are male-dominated; that is, generally and for the most part, women are marginalized or effectively excluded from participation in these practices.<sup>41</sup> To some extent, this marginalization can be measured simply by counting the percentage of scientists who are woman in each field, for example, the percentage of physicists who are women. But this measure is imperfect, since parity in numbers might not be matched by parity in recognized contribution to the production of knowledge. For example, even if 50% of scientists in a given field are women, this 50% may be relegated to the effective marginalized and low-status roles of technicians, data-gatherers, and teachers. Since this curtails the epistemic agency of these women, it seems reasonable to call this at least one important kind of epistemic injustice. Indeed, in light of the analysis of sexism that I will give in §7.5, it might be seen as a kind of institutional domination of scientific inquiry.

Recent empirical work on the causes of the marginalization of women gives some reason to think that it is not caused by stereotypes, at least by anything like the mechanism Fricker describes. Specifically, in a number of recent papers and popular essays, psychologists Wendy Williams and Stephen Ceci have argued that the marginalization of women is not due to "sex discrimination in grant and manuscript reviewing, interviewing, and hiring,"<sup>42</sup> or to innate differences in mathematical abilities. Instead, at least in contemporary academia, they claim that the problem is that women are still expected to provide the majority of care work for their children. Women with children face additional burdens and responsibilities that their men counterparts — and childless women — do not. And academic careers are not well-designed to accommodate or account for these additional burdens and responsibilities. In a slogan, the second shift inflicts epistemic injustice.<sup>43</sup> This argument is controversial, of course. But it is a far more fruitful contribution to social epistemol-

<sup>41</sup> As far as I know, marginalization is an even worse problem for several other groups, including African Americans, Native Americans, Hispanics, people from working-class backgrounds, and disabled people.

<sup>42</sup> Stephen Ceci and Wendy Williams, "Understanding Current Causes of Women's Underrepresentation in Science," *Proceedings of the National Academy of Sciences* 108, no. 8 (February 22, 2011): 1, doi:[10.1073/pnas.1014871108](https://doi.org/10.1073/pnas.1014871108).

<sup>43</sup> Stephen Ceci and Wendy Williams, "When Scientists Choose Motherhood," *American Scientist* 100, no. 2 (March-April 2012): 138, doi:[10.1511/2012.95.138](https://doi.org/10.1511/2012.95.138); Jeffrey Mervis, "Is Motherhood the Biggest Reason for Academia's Gender Imbalance?" *Science* 335, no. 6072 (March 2, 2012): 1030–1, doi:[10.1126/science.335.6072.1030](https://doi.org/10.1126/science.335.6072.1030). Unfortunately, Ceci and Williams often discuss the problem in terms of "the choice to become a mother," displacing the background sexist system of reproductive labor to focus on the individual mother. This makes their work highly susceptible to misreadings; see, for example, John Tierney, "Social Scientist Sees Bias Within," *New York Times* (February 8, 2011): D1.

ogy and understanding epistemic injustice than armchair speculations about the role of stereotypes in one-to-one knowledge transmission.

For all of my criticism, I do not think that transactionists — or any other philosophers of science — are culpable for being focused on epistemology or adopting the assumptions of traditional epistemology. During the second half of the twentieth century, an extremely narrow version of the narrow view dominated Anglophone epistemology and philosophy of science. In this climate, anything that didn't fit into some very restrictive criteria would be dismissed as sociology or history rather than philosophy proper. Since transactionism doesn't have much to do with metaphysics (unless one conflates justification and truth), it could only be articulated philosophically in this climate in epistemological terms. Feminist philosophy of science, for example, has only been widely accepted to the extent that it was concerned with identifying (and routing) androcentric biases, in the sorts of ways I examined in §5.2. That is, transactionism could only be accepted insofar as it was concerned with a very narrow set of epistemological problems. More sophisticated (and radical) work on the gender of scientists, the choice of research programs, and the interactions between scientific inquiry and the gender system of its social context more generally were either (a) classified as history and sociology rather than philosophy or (b) recast (and challenged, or dismissed) as epistemological claims.<sup>44</sup>

My second suggestion is to start addressing the threat of domination. While the threat of domination has been one of the most common arguments against transactionism in the literature for decades, there have been almost no transactionist responses to it. These first two suggestions are closely related: isolationists remain unconvinced by transactionist arguments, I believe, in large part because none of these arguments actually engage the major isolationist worry; and transactionists have been unable to engage the worry because it cannot be articulated well in epistemological terms. The deeper focus — more than just epistemology — is necessary to state the worry clearly.

A sophisticated response to the threat of domination isn't just valuable for making our case within the science and values debate. I think an account of institutional domination and its causes is absolutely essential if our society is to address constructively the entangled political and scientific controversies over, for example, climate change, transgenic organisms, and pharmaceutical research. Much of the current best academic work on these controversies seem simply to presuppose isolationism and make no distinction between, for example, internal and external goods.<sup>45</sup> Thus our public scientific and political discourse has no way to distinguish the influence of the environmentalist movement from the influence of the fossil fuel industries and determine which, if either, is illegitimate.

<sup>44</sup> Cf. Sarah Richardson, "Feminist Philosophy of Science: History, Contributions, and Challenges," *Synthese* 177, no. 3 (2010): 337–62, doi:[10.1007/s1122901097916](https://doi.org/10.1007/s1122901097916).

<sup>45</sup> See, for example, Brown, "The Community of Science<sup>®</sup>," Nestle, *Safe Food*, and possibly also Oreskes and Conway, *Merchants of Doubt*.

My third suggestion is to change the scope of the debate. I suggest that transactionists explicitly broaden our commitments to *broad view transactionism*, and explicitly engage with *narrow view isolationism* as the opposite position. Doing so gives us an opportunity to develop new arguments for our position — thereby re-invigorating the debate — and a conceptual framework in which we can give an account of institutional domination, respond to the isolationists' worries, and hopefully even produce the sorts of conceptual tools needed to address constructively scientific-political controversies. In short, I suggest that transactionists rethink science and values from the perspective of practice.



# Chapter 7

## Justice and practices

### 7.1 Introduction

Another major stage of the project begun in this dissertation would be to make explicit and develop the normative ethics that is implicit in the conception of practice. At bottom, the conception is an account of various kinds of human activities and the reasoning of the humans engaged in them, and like any such account it is both descriptive and normative. Consider the claim that something, *a*, is one of the internal goods of a practice, *A*. To a non-practitioner of *A*, this claim would seem to be purely descriptive: people who engage in *A* typically value such things as *a*. But to a practitioner *p* of *A*, the claim is normative: it is a (*pro tanto*) reason for *p* to pursue or achieve or realize *a*. For example, suppose that *p* is a scientist, and the claim is that double-blinding is one feature of a reliable experiment, or (approximately equivalently) that most other scientists in *p*'s field take double-blinding to be an important feature of a good experiment. To a non-scientist, this claim seems purely descriptive, an "ethnographic" report on the value-judgments of the tribe of science. But, to *p*, this claim gives her some reason to double-blind in her own studies. The claim has a role in *p*'s practical reason, and in that sense it is an ethical claim. In a similar sense, the claim that a course of action would promote the institutional domination of some practice is, for a member of that practice, of ethical concern.<sup>1</sup>

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<sup>1</sup> Note that the use of "ethics" here is formally distinct from its use in "ethical and political practices" and their internal goods, "ethical and political values." In the latter sense, ethics is (among the) internal goods realized by certain specific practices. In the current sense, ethics is a feature of absolutely all practices; roughly, it is a precondition for the realization of any internal goods; compare ethics in this sense with MacIntyre's conception of the "ethics of inquiry." MacIntyre, "[Moral Pluralism without Moral Relativism](#)" In some cases the two usages come back together: some ethical and political practices can have, as their target, specific kinds of institutional domination; thus realizing the internal goods of these practices is important for other practices to realize their internal goods. See the discussion of the institutions of the basic structure

To put the point another way, the trajectory of the project over the last several chapters leads us to a much more expansive view, on which scientific inquiry is one feature of the whole social landscape and must be understood in terms of its normative location or function within that landscape. However, rather than launching us on another 250+ page discussion — complete with 75 pages of definitions, a chapter or two of unhelpful examples, and some obscure diagrams — I shall confine myself in this final third of the dissertation to a sketch of this landscape. Furthermore, I shall take as my *motif* the thought that *the institutional domination of science is an injustice*. However, this does not mean that I will argue in support of this claim. Instead, in §7.2, I will give some examples of scientists expressing worries about politicization — in a sense that doesn't presuppose the conception of practice — and compare two ways of articulating this worry with the account of politicization as institutional domination that I presented, briefly, in §3.2.3. Since the alternatives do seem to understand politicization in ways that make it an injustice, it seems reasonable for me to assume that an adequate conception of justice should take institutional domination to be an injustice. The bulk of the chapter shows how the conception of practice can be used to develop an approach to liberal political philosophy in such a way that the institutional domination of scientific inquiry — among other practices — counts as unjust; this part of the chapter is introduced properly in §7.3.

## 7.2 Politicization and the right to research

In this section, I aim to motivate the thought that institutional domination — especially politicization, as this is the subject of most of my examples — is an injustice. I say “motivate” rather than “argue for” because I am not giving an argument for this thought. Instead, I am showing that scientists are concerned about institutional domination in this way, and concluding that we have some reason to accommodate this thought within a conception of justice. Thus the thought will play something like the role of what Rawls calls a “considered judgment,”<sup>2</sup> or something like the role of the shared unconditional-yet-questionable moral commitments in a flourishing local community.<sup>3</sup> Then, in the second part of this section, I discuss some previous attempts to capture and articulate this thought.

We begin on the political right. According to a published brief biography, Michael Gough holds a Ph.D. in biology from Brown University, had a brief career in academia, and has worked primarily in various government science offices, including the Office of Technology Assessment, Health and Human Services, and Environmental Protection Agency.<sup>4</sup> He is also the editor of *Politicizing Science: The Alchemy of Policymaking*, an anthology published jointly by two conservative think tanks, the Hoover Institution

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in §7.5.

<sup>2</sup> Rawls, *A Theory of Justice*, §4.

<sup>3</sup> MacIntyre, *Dependent Rational Animals*, ch. 13.

<sup>4</sup> Michael Gough, ed., *Politicizing Science: The Alchemy of Policymaking* (Hoover Insti-



and the George C. Marshall Institute.<sup>5</sup> In his introduction to this anthology, Gough first endorses a version of the value-free ideal, then worries about the threat — or reality — of institutional domination:

Those ideals do not exist, have never existed, and probably never will. Interest groups and government agencies succeed in raising a clamor about a purported risk that drowns out other considerations, even, in some cases, in the absence of actual support for the claims. The media, knowing that risks sell papers and draw viewers to news programs, focus on covering the people who assert the importance of the risk. In order to appear fair, a newspaper story about risk might quote a skeptic in the penultimate paragraph, or a two-to-five-minute TV segment might give a skeptic fifteen or twenty seconds at the end . . . .

Politicians, seeing that the public treats a risk as real, often decide to “get out in front of the problem,” even though they are aware that they have little or no knowledge of the science that supports the existence or importance of the risk. Scientists, aware of the political interest in risk and the public’s awareness of it, will seek research funds, knowing that a result that buoys up the risk, or, at least, a result that does not sink it, is more likely to be published, to receive public attention, and to result in further funding. Or, they may elect to bypass the usual norms of science entirely, call a press conference and try to convince the public about a risk with no scientific peer review.<sup>6</sup>

Note that institutional domination seems to extend beyond just scientific inquiry: assuming good public policymaking and journalism are practices, they also seem to suffer institutional domination, according to Gough.

The primary culprit, Gough thinks, is government interference:

Science and politics have become inseparable because of funding and regulation policies. Moreover, politicians intervene in the practice of science, sometimes diverting science and the interpretation of scientific findings away from where the evidence leads to directions deemed political desirable.<sup>7</sup>

Several of the later contributors — all of whom have Ph.D.s or M.D.s in a scientific field and are established bench and administrative scientists in either academia or government — speak similarly of interference, especially government interference and by the Clinton administration in particular.

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tution Press, 2003), xiv-xv.

<sup>5</sup> For a history of the Marshall Institute, see Oreskes and Conway, *Merchants of Doubt*.

<sup>6</sup> Gough, *Politicizing Science*, 2.

<sup>7</sup> *Ibid.*, 3.

Gough goes on to criticize extrapolations from animal models to humans (in toxicology) and the use of simplified computer simulations (in climate science) as unfalsifiable (and so, according to the criteria of “the leading twentieth-century philosopher of science,” Karl Popper, unscientific);<sup>8</sup> stresses caution over the conclusions of consensus panels (they “are driven by social dynamics that can substitute the value of cohesion — ‘group think’ — for independent, critical thinking”);<sup>9</sup> and rejects the precautionary principle as either so weak that it would require “few changes from the current scheme of risk assessment and management” or so strong that it “would toss science aside.”<sup>10</sup> These issues have all long been targets of critique from the political right, and each argument is elaborated in one or more of the other contributions to the anthology.

In each case we have the politicization of scientific inquiry, whether by environmentalists (this anthology is refreshingly free from talk of “special interests”), government bureaucrats, politicians, or combinations thereof. And again, the influence of “politics” is typically treated as an interference with the proper operation of scientific inquiry. Now, recall that this anthology is written in the loosely libertarian context of the Hoover Institution and George C. Marshall Institute. Injustice, for libertarians, is defined in terms of interference, especially (for right libertarians) governmental interference. Thus, while they never use this language explicitly, it would seem that, in context, it is reasonable to say that the politicization of scientific inquiry is an injustice.

Furthermore, the contributors sometimes discuss harmful consequences of politicized science, such as the rise in the incidence of malaria after the use of DDT was banned in the wake of the work of Rachel Carson.<sup>11</sup> Such harms do not clearly constitute injustices according to familiar versions of libertarianism, but they arguably do according to familiar versions of two other prominent approaches to justice, namely, egalitarian liberalism and utilitarianism.<sup>12</sup> So, again, the politicization of scientific inquiry at least contributes to injustice.

Turn now to the left. Specifically, I will examine two of the numerous reports published by the Union of Concerned Scientists on politicization and commercialization of scientific inquiry: *Scientific Integrity in Policymaking*, a well-known 2004 report

<sup>8</sup> Gough, *Politicizing Science*, 11.

<sup>9</sup> *Ibid.*, 16.

<sup>10</sup> *Ibid.*, 17.

<sup>11</sup> *Ibid.*, 19.

<sup>12</sup> Examples of egalitarian liberals would include John Rawls, Martha Nussbaum, and Thomas Pogge. Left or egalitarian libertarians such as Hillel Steiner and Michael Otsuka share some common ground with both egalitarian liberals and libertarians, but it is not clear to me what, if anything, they might have to say about the politicization of scientific inquiry. Consequently, I will not discuss left libertarianism much in this chapter; when I refer to “libertarianism,” I will generally mean the more familiar right version of libertarianism. Examples of right libertarians include Robert Nozick and Jan Narveson.

on science and the Bush administration; and *Driving the Fox from the Henhouse*, a 2010 report of a survey of health and safety scientists at FDA and USDA.

Seth Shulman is credited as the “lead investigator and primary author” of *Scientific Integrity*; as best as I can tell, Shulman is a professional science journalist and writer, and the content of *Scientific Integrity* is similar to a full-length book he wrote on the same topic a few years later.<sup>13</sup> The four credited authors of *Fox* are all associated with the UCS Scientific Integrity Program. Three of the four have a graduate degree in a scientific field from a prestigious private university; their titles include “former analyst,” “senior scientist,” “program manager,” and “associate analyst.” Both reports were prepared with the cooperation of scientists and whistleblowers employed by the government.

The language of *Scientific Integrity* is much the same as we saw with Gough: in the conclusion, Shulman asserts that he has “provided substantial evidence that objective scientific knowledge is being distorted for political ends” and that “the administration’s political agenda has permeated the traditionally objective, nonpartisan mechanisms” for science-based public policy.<sup>14</sup> Roughly the first half of the report documents politicization on such issues as climate change, air quality, abstinence-only sex education, breast cancer, antibiotic resistance in airborne bacteria, the Endangered Species act, and forest management.<sup>15</sup> Of course, where Gough claimed that the Clinton administration was politicizing scientific inquiry, Shulman’s target is the Bush administration.

Shulman does make a number of critiques that are not prominent in Gough. For example, he accuses the Bush administration of using inappropriate “political litmus tests” for appointees.<sup>16</sup> But the basic complaint, as with Gough, is of political interference: the “suppression and distortion of scientific findings,” blatant tampering “with the integrity of scientific analysis,” seeking “to exaggerate uncertainty by relying on disreputable and fringe science reports,” censorship, and not merely “ignor[ing] the scientific evidence” but “distorting science-based performance measures.”<sup>17</sup>

The *Fox* report deals primarily with the results of a survey of FDA and USDA employees, especially scientists and health and safety inspectors at these government agencies. The tone and language of the *Fox* report is similar to the other two examples. The primary concern of this report is that we have “a food safety system where, far too often, special interests and public officials inhibit the ability of government scientists and inspectors to protect us.”<sup>18</sup> Summarizing the results of the survey, the

<sup>13</sup> Seth Shulman, *Undermining Science: Suppression and Distortion in the Bush Administration* (Berkeley and Los Angeles: University of California Press, 2006), ISBN: 0520247027.

<sup>14</sup> Seth Shulman, *Scientific Integrity in Policymaking: An Investigation into the Bush Administration’s Misuse of Science* (Union of Concerned Scientists, 2004), 31.

<sup>15</sup> *Ibid.*, 4-19.

<sup>16</sup> *Ibid.*, 20.

<sup>17</sup> *Ibid.*, 2; 5; 7; 8, among others; 11.

<sup>18</sup> *Driving the Fox from the Henhouse: Improving Oversight of Food Safety at the FDA*

authors write the following:

The survey results paint a complex picture of the federal food safety system. On the one hand, the reported levels of *political and corporate interference* both at the USDA and FDA are troublingly high, many scientists claim that they are *not free to discuss their findings* with the media or to speak out about their agency's work, and FDA respondents often cite [sic] insufficient resources to meet their mission. On the other hand, most survey respondents feel that the agencies are moving in the right direction and acting effectively to *protect the public health*.<sup>19</sup>

Again, the concerns about politicization are stated in terms of interference, lack of freedom, and downstream effects on citizen well-being. That is, in egalitarian liberal terms.

One notable difference between *Fox* and the other two sources is an emphasis on commercialization, here meaning illegitimate influence from commercial interests. Indeed, the *Fox* authors argue that the influence of such “outside entities” as “corporate [read “commercial”] interests, nongovernmental organizations (NGOs), and members of Congress” was “even more widespread than internal political interference.”<sup>20</sup> In particular, twenty-seven percent of survey respondents “reported frequently or occasionally experiencing ‘instances where the public health has been harmed by businesses withholding safety information from agency investigators’” in the year prior to the survey,<sup>21</sup> and “forty-four percent of respondents said that business interests were given a lot of weight or much weight in agency decisions.”<sup>22</sup>

So, all together, I suggest that we see familiar, albeit watered-down, ideas from political philosophy in these scientists' discussions of politicization. On the political right the worries are stated in libertarian terms, especially government interference with individual freedom. On the political left the worries are in line with egalitarian liberalism, especially concern for the well-being of citizens in general and a suspicion of commercial interests.

It might be objected that the “libertarian” language of interference is prominent in the sources from both the right and the left; in particular, it is used by people who are not libertarians, and hence might be related to the political philosophy in any but an accidental way. This language might simply be the most rhetorically effective way of making the desired points or recruiting allies in the particular partisan project of these reports.

But why is this language so effective? Why do non-libertarians use language that, on its face, seems so close to libertarianism? I concede that this might be accidental:

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*and USDA* (Union of Concerned Scientists, 2010), 3.

<sup>19</sup> *Driving the Fox from the Henhouse*, 19, my emphasis.

<sup>20</sup> *Ibid.*, 23.

<sup>21</sup> *Ibid.*, 24.

<sup>22</sup> *Ibid.*, 26.

through, as it were, memetic drift rather than selective pressure, scientists have just happened to adopt the language of interference. But there is also the possibility that this language has been adopted because of a widespread acceptance of some — again, watered-down — libertarian ideas in our public political culture. That is, the language of libertarianism is effective because many people in our society have at least some libertarian commitments. Or, in the evolutionary metaphor, acting a little bit like a libertarian is adaptive when one is surrounded by many other people who act a little bit like libertarians.<sup>23</sup> This is a question for empirical investigation, of course, but for that reason it is reasonable for me to assume for the time being some sort of non-accidental connection between the rhetoric and the political philosophy.

Furthermore, the kinds of explanations suggested by the objection do not seem to explain important differences between the right and the left. Scientists on the right worried primarily about governmental interference, with some concerns about remote downstream effects on human health and well-being, such as the unnecessary costs of overregulation and slower development of new technologies. Scientists on the left worried about both commercial and governmental interference, and often tied these concerns to nearly immediate downstream effects on human health and well-being, such as outbreaks of food-borne pathogens. At least on their face, these variations between the two groups of scientists seem to be best explained by variations in implicit political philosophy. Again, this is an issue for more careful empirical investigation, and again for that reason it is reason for me to assume for the time being that the difference is one of political philosophy.<sup>24</sup>

Political theorists Mark Brown and David Guston have done some analysis of these worries about politicization, and related calls to defend a “right to research” as responses to politicization. Specifically, they consider and reject an account of politicization that draws on the tradition of liberal political philosophy — especially its classical and libertarian versions — then argue for an alternative account that draws on what is called the republican tradition.<sup>25</sup> I shall follow them along this

<sup>23</sup> Compare Hicks, “On the Ideal of Autonomous Science.”

<sup>24</sup> The reader may not be convinced that politicization is an issue of *justice*. But hopefully I have adequately motivated the thought that political philosophy should have something to say about politicization, whether in terms of justice or in terms of some other social excellence. Given the connection between, on the one hand, the language scientists use to articulate their worries about politicization and, on the other hand, libertarian and egalitarian liberal accounts of justice — justice is non-interference and so on — I think that “justice” is the right term to use. I also can’t think of a term that would fit better; politicization doesn’t seem to be a matter of legitimacy or democracy or productivity, for example. Paul Weithman has suggested “mis-valuation of goods,” which is accurate but, to my ear, sounds like a specific kind of injustice. In any case, very little of what I am up to in this chapter turns on whether politicization is a matter of justice specifically, so the reader should feel free to substitute a more appropriate term when “justice” appears.

<sup>25</sup> Note that the names of these traditions do not correspond — in pretty much any way

path, then present my preferred account, which (unsurprisingly) will be in terms of the conception of practice.

On the liberal account, politicization is understood as the illegitimate interference, by partisan political agents, with scientific inquiry.<sup>26</sup> Politicization thus infringes upon (what the account takes to be) the rightful autonomy of scientists.<sup>27</sup> Correspondingly, the liberal account's right to research is a negative right — a right to non-interference — that, if legally enforced, would protect the autonomy of scientists to freely and privately organize themselves for the pursuit of their own conception of the good, specifically, scientific inquiry.<sup>28</sup> I believe that this is a reasonable way of capturing much of what we saw in the first part of this section, with scientists on both the right and the left taking politicization (and commercialization) to be a form of unjust interference. While none of my examples calls explicitly for a right to research, all call for protections against interference with the work of scientists. Brown and Guston note that there are three basic constitutional arguments for a right to research, each of which appeal to First Amendment protections of free speech: non-politicized scientific inquiry is either “an element of academic freedom, a precondition for free expression, or a form of expressive conduct.”<sup>29</sup>

Brown and Guston offer several criticisms of the liberal account; I focus on just three here. First, the account seems to take roughly all forms of political influence on scientific inquiry to be illegitimate; in my terminology, it is isolationist. In light of several decades of transactionist criticism of isolationism, Brown and Guston assert that the assumption of isolationism is “either naïve or [itself] partisan.”<sup>30</sup> Brown and Guston themselves distinguish two forms of politicization, only one of which they see as problematic:

When politics is reduced to partisan one-upmanship, it poses a threat to both intelligent politics and socially and intellectually productive scientific inquiry. But when politics involves inclusive deliberation aimed at articulating diverse perspectives and seeking compromise in the public interest, then politicizing science becomes a means of ensuring that it serves public purposes.<sup>31</sup>

Second, the liberal account and efforts taken to promote its conception of the right to research have not succeeded in reducing political controversy over scientific inquiry.

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— to the views of either “liberals” in US public political discourse or members of the Republican party.

<sup>26</sup> Brown and Guston give citations to about ten examples of this liberal account.

<sup>27</sup> Mark Brown and David Guston, “Science, Democracy, and the Right to Research,” *Science and Engineering Ethics* 15, no. 3 (2009): 353, doi:[10.1007/s11948-009-9135-4](https://doi.org/10.1007/s11948-009-9135-4).

<sup>28</sup> *Ibid.*, 358.

<sup>29</sup> *Ibid.*, 355.

<sup>30</sup> *Ibid.*, 353.

<sup>31</sup> *Ibid.*, 354.



For example, in 2004, California Proposition 71 established \$3 billion in public funding for stem cell research, proclaimed “a right to conduct stem cell research,”<sup>32</sup> and exempted the public institute for stem cell research that it created “from conflict-of-interest and open meetings laws.”<sup>33</sup> Unsurprisingly, Proposition 71 was challenged both before and after the election by right-to-life organizations. The Proposition therefore increased, not reduced, political controversy over stem cell research. Indeed, Brown and Guston seem to think that this is the inevitable result of any liberal account of and response to politicization: “Part of the reason [for the failure of depoliticization] may reside in the liberal-protective conception of rights with which proponents of a right to research have defended their claims.”<sup>34</sup> Later they elaborate:

It often happens, of course, that majorities rule on the basis of their narrow self-interest. But insofar as the liberal-protect language of rights reinforces a view of individual interests as prior to politics, it actually increases the likelihood of this sort of majority tyranny. By insisting on a protective right to research, scientists may actually reinforce the majority’s tendency to embrace a politics of narrow self-interest, which is what threatens their minority interests in the first place.<sup>35</sup>

The thought here seems to be that, on any liberal account, the interest of scientists in the free pursuit of scientific inquiry is the interest of a private association — an interest group, and just one of many. As such, scientists must wrangle with rival interest groups, such as right-to-life organizations, within the political system for wealth, power, status, and other resources. Political controversy — this wrangling within the political system — is unavoidable. Furthermore, as political theorist Iris Marion Young argued, this interest group model of politics eliminates participatory public deliberation in favor of agonistic struggle: “interest-group pluralism makes no distinction between the assertion of selfish interests and normative claims to justice or right” and so “collapses normative claims to justice into selfish claims of desire.”<sup>36</sup> Declaiming the politicization of scientific inquiry and calling for a right to research is nothing more than a rhetorical ploy in the worst sense. At best, members of one interest group can appeal to the self-interest of another interest group to form temporary political coalitions or alliances. For example, claims by stem cell researchers that their work is liable to yield (at some vague point in the future) treatments for degenerative chronic diseases, such as Alzheimer disease or ALS (Lou Gehrig’s disease), can be seen as efforts to form coalitions with patients’ advocacy organizations for greater political influence. In short, the second criticism seems to be that, in a deep way, the liberal account is at best self-defeating.

<sup>32</sup> *Ibid.*, 351, quoting the text of the Proposition.

<sup>33</sup> *Ibid.*, 357.

<sup>34</sup> *Ibid.*

<sup>35</sup> *Ibid.*, 361.

<sup>36</sup> Young, *Justice and the Politics of Difference*, 73.



The discussion of the last paragraph leads to the third criticism. The right to research would only seem to protect the small set of researchers:

Just as most seventeenth-century defenders of private property rights meant not a general “right to property” but rather the rights of the propertied, proponents of research rights seem to be thinking not of a general “right to do research” but primarily of the rights of researchers . . . . This limited generalizability of the claim for a right to research limits its moral force.<sup>37</sup>

This criticism would seem to be incompatible with the constitutional arguments that ground a right to research in free speech rights protected by the First Amendment. However, Brown and Guston argue that these arguments are weak: First Amendment jurisprudence has introduced specific criteria for protections of expressive conduct, and these do not apply to such as activities as conducting experiments.<sup>38</sup> Also, such arguments can be seen as part of the rhetorical tactics of the interest group of scientists — a rationalization rather than a justification for protecting the interests of this group.

In light of these criticisms of a liberal account of politicization and the right to research, Brown and Guston develop their account from the republican tradition of political philosophy. This tradition is understood as historically entangled with, but still distinct from, the liberal tradition that is dominant in contemporary political discourse and academic thought. Standard examples of republican thinkers include Cicero, Machiavelli (especially the *Discourses on Livy*), Montesquieu, Blackstone, Jefferson, Madison, and — in the contemporary context — Quentin Skinner and Philip Pettit.<sup>39</sup> On a generic republican view, political institutions, public participation in these institutions, and citizens’ rights are valued instrumentally, for their role in protecting citizens (individually, collectively, or both) from domination. For example, Brown and Guston draw heavily on the work of political theorist Robert Dahl, who views “fundamental political rights as comprising all the rights *necessary* to the democratic process.”<sup>40</sup> Brown and Guston further distinguish three sorts of such rights:

- (1) *political rights*, which are integral to the democratic process, such as freedom of speech and assembly;

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<sup>37</sup> Brown and Guston, “Science, Democracy, and the Right to Research,” 359.

<sup>38</sup> *Ibid.*, 355.

<sup>39</sup> Frank Lovett, “Republicanism,” *The Stanford Encyclopedia of Philosophy (Summer 2010 Edition)*, 2010, <http://plato.stanford.edu/archives/sum2010/entries/republicanism/> (accessed January 25, 2012).

<sup>40</sup> Dahl, quoted in Brown and Guston, “Science, Democracy, and the Right to Research,” 360, emphasis in Brown and Guston.

- (2) *social rights*, which are external to the democratic process but may be a precondition for it, such as basic education, health insurance, and satisfaction of basic material needs; and
- (3) *civil rights*, which are external to the democratic process but entailed by human equality, and hence, supportive of the type of moral culture that democracy requires, such as religious liberty, moderate property rights, and privacy.<sup>41</sup>

In terms of this taxonomy, Brown and Guston argue that inquiry — a broader category that includes scientific inquiry — is integral to the democratic process in such a way that a right to inquiry should be included in the set of political rights. The argument is straightforward:

Democracy requires a citizenry informed about any number of topics, including those related to understanding political decisions . . . , the formation of preferences . . . , and the substantive outcomes of government policies. If citizens were not free to conduct such inquiries, then fundamental political activities — the act of voting, for example — would be rendered as useless as if there were only one candidate on the ballot.<sup>42</sup>

The democratic process integrally involves public deliberation and inquiry in a general sense, and so political rights should protect deliberation and inquiry.

This conclusion is too general to give much advice to jurisprudence or legislation, so Brown and Guston suggest that “those inquiries that best support democratic goals have the strongest claim to protection and support. That is, the right to inquiry is stronger when the inquiry makes a distinct contribution to democratic processes than when it does not.”<sup>43</sup> For example, freedom of the press and political speech should enjoy more protection and support than nutrition labeling, and forms of speech that harm or interfere with democratic processes (such as, arguably, hate speech and pornography) can (or even should) be restricted.<sup>44</sup> Scientific inquiry in general falls in the middle of this hierarchy, with significant variation:

In some cases the social sciences may have a stronger claim than the natural sciences to a right to research. To the extent that natural scientific and engineering inquiries produce knowledge useful for democratic governance, they would have greater protection as well . . . . Indeed, if democracy is conceived as a mode of collective problem solving, then some areas of scientific research might be deemed integral to democracy [and so receive the highest protection and support].<sup>45</sup>

<sup>41</sup> Quoting freely from *ibid.*, 360-1.

<sup>42</sup> *Ibid.*, 361.

<sup>43</sup> *Ibid.*, 362.

<sup>44</sup> *Ibid.*, 362, 364.

<sup>45</sup> *Ibid.*, 363.

Other areas of scientific inquiry “are not integral to the democratic process but contribute to its material preconditions”; these areas would not be covered by political rights at all, but instead by social or civil rights.<sup>46</sup> Specifically,

Scientific research pursued primarily for curiosity’s sake, for example, may deserve some degree of protection as a civil right, analogous to freedom of religion or the right to private property, but it may not deserve the stronger protection afforded to research conceived as a political or social right.<sup>47</sup>

Keeping these distinctions in mind, it seems appropriate to speak of plural rights to research.

Perhaps surprisingly, Brown and Guston say almost nothing explicit about the content of any of these rights. As we saw above, they reject the liberal account’s negative conception of these rights. Yet they consistently describe the function of these rights on the republican account in terms of protection.<sup>48</sup> They do occasionally use the language of support — “the state may have an obligation to protect and even support a citizen’s right to inquiry,” for example — so it perhaps the rights to research have both positive and negative components.<sup>49</sup> Still, this does not seem to challenge the assumption that rights to research would provide “barrier[s] against government interference with science” — which assumption, they say, “may be quite mistaken.”<sup>50</sup>

More importantly for my purposes, Brown and Guston only give a very oblique characterization of problematic politicization — the bad that rights to research are meant to protect against. To an extent, this can be worked out negatively: given that rights to research are meant to protect and support inquiry, they would seem to protect against failed or dysfunctional inquiry, or the complete absence of inquiry from civic life. And this seems to cohere with the one instance where Brown and Guston distinguish problematic and unproblematic kinds of politicization:

When politics is reduced to partisan one-upmanship, it poses a threat to both intelligent politics and socially and intellectually productive scientific inquiry. But when politics involves inclusive deliberation aimed at articulating diverse perspectives and seeking compromise in the public interest, then politicizing science becomes a means of ensuring that it serves public purposes.<sup>51</sup>

<sup>46</sup> Brown and Guston, “[Science, Democracy, and the Right to Research](#),” 363.

<sup>47</sup> [Ibid.](#)

<sup>48</sup> For example, I count eleven such usages of “protect” and derivative terms in just two pages, [ibid.](#), 362-3.

<sup>49</sup> [Ibid.](#), 362.

<sup>50</sup> [Ibid.](#), 352.

<sup>51</sup> [Ibid.](#), 354.

So science is politicized in the *good* sense when it contributes to and is the subject of effective public discourse. And it is politicized in the *bad* sense when it is appropriated for use in partisan one-upmanship.

Now let us consider how Brown and Guston's republican account fares with respect to the three criticisms of the liberal account. The first criticism was the uncritical, naïve, or partisan assumption of isolationism. On the republican account, by contrast, scientific inquiry is often a constitutive element of democratic politics, and politicization in this sense "becomes a means of ensuring that [scientific inquiry] serves public purposes."<sup>52</sup> While this is strictly compatible with narrow view isolationism as I have defined it, it is clearly much more at home with broad view transactionism.<sup>53</sup>

The second criticism was that the liberal account is self-defeating, in that it represents scientists as just another interest group, destined to struggle interminably against other interest groups for influence and wealth in the political system. The right to research, on this view, seems to be little more than a cynical rhetorical cover for a special interest. On the republican view, scientific inquiry contributes (directly or indirectly) to democratic politics, and thus actively promotes the (collective and individual) interests of scientists' fellow citizens. Non-scientists can understand the work of scientists as contributions to the public good, rather than a purely private interest, and recognize that this work requires protection from manipulation for the sake of partisan (and financial) gain, in the same way as the work of judges, legislators, and journalists. The republican account does not aim to promote depoliticization or non-politicization, but rather a kind of politicization that promotes the flourishing of scientific inquiry; with respect to its aim, and unlike the liberal account, the republican account does not seem to be self-defeating.

The third criticism was that the liberal account of the right to research seems to protect the interests of only a very small or narrow group; while there are attempts to justify this right by appeal to the broader rights of free speech, they do not seem very compelling (at least to Brown and Guston). By contrast, the republican argument for the right to research starts with a general account of rights to inquiry — rather than speech as such — and protects and supports the activities of a narrow group (researchers) to the extent that these activities conduce to the interests of citizens broadly or as a whole. The right to research protects and supports researchers, but for the sake of protecting and supporting citizens in general.

Brown and Guston seem to have a strong case against liberal accounts of politicization and for the republican account. However, I argue that the conception of practice — and the broad view transactionist conception of scientific practice specifically — provides a still better account. In later sections, I will argue that this conception is compatible with liberal political philosophy. Hence, liberalism can provide an account of politicization — and institutional domination more generally — that is superior to the republican account offered by Brown and Guston.

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<sup>52</sup> *Ibid.*

<sup>53</sup> Note that Guston and Brown briefly but explicitly reject the narrow view, *ibid.*, 363.

The basic elements of the practices account of politicization should be obvious by now: politicization is understood as one kind of institutional domination, and institutional domination is a bad. Assuming that our conception of justice includes the claim that institutional domination (of scientific inquiry) is unjust, it follows that politicization (of scientific inquiry) is an injustice. At a minimum, the corresponding right to research involves protection from institutional domination. However, protection cannot be understood negatively, as non-interference or non-influence by every institution; this is because every practice requires support from *some* institution. Instead, the right to research involves managing or balancing the unavoidable tension between the practice and its institutions. On the one hand, it requires some degree of support from some institutions; on the other hand, it requires that this support value the internal goods of scientific inquiry as such, rather than merely as means to such ends as partisan political power or the profits of a given industry.

Let us consider the practices account and the republican account. Again, I use Brown and Guston's three criticisms of the liberal account to structure the discussion.

Obviously the practices account as such does not assume isolationism; while an uncritical narrow view account would be vulnerable to the first criticism, a broad view transactionist account is not.

The practices account might be self-defeating, in a way similar to the liberal account, if it led to widespread worries about institutional domination. Suppose, for example, that it isolated non-scientists from scientific inquiry in such a way that they (the non-scientists) failed to recognize the internal goods of scientific inquiry as such, and instead believed scientific inquiry to be nothing but the pursuit of external goods. The mechanism might be basically the same as that described by the connection argument. In this case, non-scientists would treat scientific inquiry in the same way that they would treat any (other) institution: as a source of external goods, and not needing any sort of protection from (other) institutions. Indeed, for the sake of efficiency, non-scientists might even support closer integration between scientific inquiry and (other) institutions. This would encourage commercialization and politicization, and so the practices account would have produced exactly the problem it is attempting to avoid.

However, I do not see how the practices account is liable to lead non-scientists to fail to recognize the internal goods of scientific inquiry as such. Indeed, assuming that the account of science as a practice is situated within the more general conception of practices, reflection on the recognition claim seems to encourage us to be cautiously open-minded about the possibility that a given activity is a practice. If this is right, then non-scientists would be liable to take an approach similar to that taken towards Nazi science in §§5.5 and 6.5, carefully considering the possibility that scientific inquiry is a practice. Recognizing this possibility leads to the possibility that joint practice with scientific inquiry could stimulate partly external progress in other practices. And since this possibility requires that scientific inquiry not suffer institutional domination, non-scientists can reach the conclusion that scientific inquiry

should be protected from institutional domination. Scientific inquiry is not believed to be yet another interest group or institution; rather, it is a practice, with its own internal goods, and we (scientists and non-scientists) may be able to help each other pursue our internal goods, through joint practice.

This analysis for the second criticism can be extended to the third criticism. Recall that this criticism was that the right to research only protects the interests of a very small group, researchers themselves, and so has very limited justificatory strength. As we saw, attempts to justify the liberal version of the right to research treat it as an instance of general free speech rights. The practices account could follow the same strategy: assuming that every practice has a right against institutional domination, scientific inquiry in particular has a right against institutional domination, which right is called the right to research. By contrast, an argument like that of the republican account would claim that scientific inquiry should be protected for the sake of protecting other practices and the interests of all citizens in general. The argument of the last paragraph follows exactly this strategy: protecting scientific inquiry (and other practices) from institutional domination improves the prospects for joint practice with, and so partly external progress in, other practices. The interests of other practices — the interests of non-scientists, and citizens in general — are protected and supported by protecting and supporting the interests of scientific inquiry and scientists.

Thus the practices account avoids the problems of the liberal account. It also provides leverage for a criticism of Brown and Guston's account, at least as presented. On the account of politicization that I extracted from their paper, science is politicized in the good sense when it contributes to and is the subject of effective public discourse. From the perspective of the conception of practice, however, we should be cautious about the language in which this discourse is conducted, and there is reason to think that the right kind of discourse is not as easy to achieve as Brown and Guston seem to think.

Suppose that public discourse is conducted in entirely classical utilitarian terms: all of the participants agree, at least for the purposes of determining public policy, that all goods are to be evaluated in terms of subjective pleasures and pains, and the overall aim is to maximize the aggregate total of such pleasures. I take it to be more than plausible that such discourse, if it is effective — that is, if it can be and is utilized in public policy — will lead to institutional domination. Internal goods will be routinely sacrificed, throughout all of society, for the sake of even very small increases in subjective pleasure. More realistically, if all goods are evaluated in terms of wealth — if all deliberation is carried out as a cost-benefit analysis — then we will also see (and arguably have also seen) the routine sacrifice of internal goods for the sake of even very small increases in wealth. If public discourse, whatever its other virtues, is conducted exclusively in terms of external goods, then it is liable to produce institutional domination. From the perspective of the conception of practice, public discourse *in general* is adequate; this discourse must be conducted in terms of

internal goods.<sup>54</sup>

All together, Brown and Guston’s republican account of politicization and the right to research seems superior to the liberal account. And the account generated by the conception of practice seems superior to both.

### 7.3 The framework of political liberalism

I turn now to my defense of the claim the conception of practice is compatible with liberal political philosophy, and a liberal conception of justice specifically. To the extent that this defense is successful, some (at least one) liberal conception of justice can support the claim that the politicization of scientific inquiry — in my sense of that term — is an injustice.

My claim cannot be merely that the conception of practice is formally consistent with some liberal conception of justice. This could be accomplished, more or less, by taking the union of two sets of claims with a disjoint vocabulary, that is, a set of claims in the language of practices and a set of claims in the language of the target liberal conception. This will not do, because precisely the feature that makes the union consistent — the formally disjoint vocabulary — will prevent it from implying the claim that institutional domination is unjust.

My approach will be to start with a general framework for liberal political philosophy — specifically, political liberalism, as developed by John Rawls. This framework — as the term suggests — provides a starting point for developing a liberal conception of justice, but the development can proceed in numerous different ways. For example, Rawls’s political liberalism provides a starting point for his conception of justice as fairness.<sup>55</sup> But the same framework has also been used for the very different capabilities approach to justice.<sup>56</sup> So I will sketch a third development of the framework,

<sup>54</sup> The recognition claim raises another worry about public discourse. It will be presented and addressed in §7.6.

<sup>55</sup> The name “justice as fairness” refers to the elements of Rawls that are most familiar to non-political philosophers, including the veil of ignorance and the two principles of justice with the lexical priority of liberty and the difference principle. The point of this section is to introduce the elements from Rawls that I will actually use.

<sup>56</sup> The capabilities approach as such has its origins in the work of Amartya Sen in the 1970s and ’80s; see, for example, Amartya Sen, *Equality of What?* (The Tanner Lectures on Human Values, 1979); Amartya Sen, *The Standard of Living* (The Tanner Lectures on Human Values, 1985); Amartya Sen, *Development as Freedom* (New York: Anchor Books, 1999), ISBN: 0385720270. Sen’s version of capabilities antedates the idea of political liberalism. However, Martha Nussbaum is explicit that her version of the capabilities approach is a political conception in precisely Rawls’s sense; see Martha Nussbaum, *Frontiers of Justice* (Cambridge, MA: Harvard University Press, 2006), 6, *et al.*; ISBN: 0674019172; Martha Nussbaum, *Creating Capabilities: The Human Development Approach* (Cambridge, MA and London: Harvard University Press, 2011),



one which aims at supporting the claim that the institutional domination of scientific inquiry is unjust. This means, among other things, that I will say very little about the original position, and my development of the framework does not incorporate it. While the original position plays a central role in Rawls's justice as fairness, it is not essential to the framework of political liberalism as such; it does not appear at all in Nussbaum's version of the capabilities approach, for example.

The framework of political liberalism is based on a distinction between *comprehensive conceptions* and *political conceptions*.<sup>57</sup> A comprehensive conception is a set of views on metaphysics and ethics that is taken to be more-or-less total, in the sense that it is meant to cover all aspects of human life. Characteristically, a comprehensive conception makes reasonably controversial "claims to universal truth" and "claims about the essential nature and identity of persons."<sup>58</sup> By *reasonably controversial*, I mean that there is ongoing controversy over the truth of these claims among reasonable people. Rawls argued that, due to what he called the burdens of judgment — conflicting and complex evidence, the problem of weighting incommensurable criteria, vagueness, the influence of social location, and so on — "many of our most important judgments are made under conditions where it is not to be expected that conscientious persons with full powers of reason, even after free discussion, will all arrive at the same conclusion."<sup>59</sup> Consequently, the only way to ensure widespread agreement about important and basic issues of metaphysics and ethics would be to coercively eliminate dissent. And since a liberal society cannot accept such means, liberal political philosophy must accommodate the *fact of reasonable pluralism*.<sup>60</sup> Specifically, no comprehensive conception — precisely because so many of its claims are reasonably controversial — can serve as the basis for an adequate conception of justice for a liberal democracy: this conception of justice must be beyond reasonable controversy, more or less and at least in the context of ideal theory. In place of a comprehensive conception, Rawls suggested that "we look . . . to our public political culture itself, including its main institutions and the historical traditions of their interpretation,

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89-93, ISBN: 9780674050549.

<sup>57</sup> In Anglophone political philosophy, the development of this distinction — and the framework of political liberalism more generally — is usually attributed exclusively to John Rawls. Martha Nussbaum notes that Charles Larmore — writing in French, and apparently independently — also developed a version of this distinction. My touchstone will be Rawls's work. Martha Nussbaum, "Perfectionist Liberalism and Political Liberalism," *Philosophy and Public Affairs* 39, no. 1 (Winter 2011): 4.

<sup>58</sup> John Rawls, "Justice as Fairness: Political Not Metaphysical," *Philosophy and Public Affairs* 14, no. 3 (Summer 1985): 223.

<sup>59</sup> John Rawls, *Political Liberalism* (New York: Columbia, 1993), 56-7; 58, ISBN: 9780231130882.

<sup>60</sup> Note that Rawls's argument from the burdens of judgment is meant to be a political or practical rather than epistemic argument; as he puts, he is arguing for the "practical impossibility" of reaching interpersonal consensus and he "does not argue that we should be hesitant and uncertain, much less skeptical, about our own beliefs." *ibid.*, 63

as the shared fund of implicitly recognized basic ideas and principles. The hope is that these ideas and principles can be formulated clearly enough to be combined into a conception of political justice congenial to our most firmly held convictions.<sup>61</sup> That is, a political conception develops and organizes concepts that are familiar to citizens who hold a wide variety of comprehensive conceptions. For example, Rawls understands his development of social contract theory in exactly this way.

Martha Nussbaum offers Isaiah Berlin and Joseph Raz as contemporary examples of comprehensive liberals, that is, proponents of liberalism as a comprehensive conception. Raz, she says, “defends a controversial doctrine of autonomy as the key to what makes lives valuable in general, and he urges that this value ought to be the core value in a liberal society.”<sup>62</sup> The “controversial doctrine of autonomy” is a Millian or Kantian one, on which free thinking and challenging (or even outright rejecting) tradition and authority are essential features of the good human life. This view is controversial in the context, for example, of adherents of a religion — such as many forms of Christianity and Islam — who *both* believe that love of and submission to God and God’s earthly authorities is the highest human good *and* support a liberal democratic society, including its freedoms of conscience and religious practice. Such views are examples of comprehensive conceptions that reject the classical liberal value of individual autonomy while still accepting a liberal *political* conception. They are, that is, reasonable comprehensive conceptions.

Obviously, political liberalism leans heavily on the distinction between reasonableness and unreasonableness, and Rawls’s treatment of this distinction is uncharacteristically convoluted. He uses the adjective “reasonable” to describe kinds of persons, comprehensive conceptions, and controversy or pluralism, but also associates it with the central concept of fairness.<sup>63</sup> The definitions of all of these terms are impredicative, that is, self-referential. For example, one definition of a reasonable person is someone who recognizes reasonable pluralism, that is, disagreement among reasonable persons. Finally, as Nussbaum points out, following Samuel Freeman, Rawls’s account of reasonable persons and comprehensive conceptions seem to entwine or slide between epistemic and ethical aspects.<sup>64</sup>

However these issues are best worked out, Rawls’s conception of reasonableness seems to be tied to his ideal theory. Since I am interested in non-ideal theory, I’ll bracket the Rawls exegesis here, and give instead a more Deweyan account. This account starts, not with reasonableness, but rather with *unreasonableness*. Unreasonableness is a name we give to a characteristic problem of contemporary liberal democratic societies, and specifically a kind of failure of public deliberation, in which people are unable to agree on coordinated courses of action due to a misunderstanding of each others’ basic goals or commitments. The people who fail in this way are

<sup>61</sup> Rawls, “Justice as Fairness,” 228.

<sup>62</sup> Nussbaum, “Perfectionist Liberalism and Political Liberalism,” 11.

<sup>63</sup> For the concept of fairness, see §7.7.3.

<sup>64</sup> Nussbaum, “Perfectionist Liberalism and Political Liberalism,” 22-31.

said to be unreasonable. Note that, while the individuals involved are unreasonable, this is secondary; it is their interaction, the disagreement, that is unreasonable in the primary sense.

The recognition claim creates a paradigm kind of unreasonable disagreement. Consider the ongoing dispute between pro-life and pro-choice activists. It is clear that each side in this dispute claims that the other is an institution: pro-lifers believe that pro-choicers are primarily interested in consequence-free sexual pleasure (recall that pleasure, in the subjective sense, is an external good), and pro-choicers believe that pro-lifers are primarily interested in subjecting women to male power. But, if one engages carefully with each side, it becomes clear that each contains, at least as a significant part, a practice: many pro-lifers consider developing fetuses something like pre-born children, and consequently care for them in the same way that caregivers care for other children; and many pro-choicers are trying to promote certain kinds of bodily autonomy that have historically been denied to women. So each side sees the work of the other as something like efforts at institutional domination: the pro-lifers mistakenly think that pro-choicers are trying to replace parental care with sexual hedonism, and the pro-choicers mistakenly think that the pro-choicers are trying to reclaim patriarchal power. Each misunderstands the other's basic goals and commitments, seeing external goods where there are (at least to some extent) internal goods. And so they are unable to agree on coordinated courses of action, that is, public policy with respect to abortion and surrounding issues. Instead, they simply shout epithets at each other, and struggle for control over the reins of political power.<sup>65</sup>

Since unreasonableness names the problem, reasonableness names the solution. Specifically, *reasonableness* is the virtue (now in the standard sense, not the technical sense of previous chapters) of making a thoughtful effort to understand the basic goals or commitments of people with whom one disagrees. People with this virtue are said to be reasonable; but again, it is the interaction that is reasonable in the primary sense. For example, a reasonable pro-choicer would be someone who engages with pro-lifers — not (a) to prevent them from implementing pro-life legislation, (b) to harass and insult them, or even (c) convince them that they are wrong — but rather (d) to better understand them, their goals and commitments, the assumptions and arguments that support their goals and commitments, and so on. The reasonable pro-choicer does not have to abandon or moderate his own commitments in any way to do this, though it may have that effect. Instead — paradigmatically, I suggest — he is trying to understand better the nature of his disagreement with the pro-lifer. For example, he might recognize that he must understand pro-lifers better in order

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<sup>65</sup> Compare MacIntyre's characterization of our society as an "emotivist society," MacIntyre, *After Virtue*, chs. 1-3. Another example that is structurally similar to the abortion dispute is the so-called vaccine war, between physicians who support and parents who oppose mandatory vaccinations. This example incorporates some interesting science and values issues, since much of the debate is over the strength of the evidence that vaccines do not cause autism. But it is rather less familiar than the abortion example.

to be able to formulate public policy proposals that all concerned could accept as a temporary compromise. Or he might be concerned that his own views are defective in some way that is unknown to him but that could be uncovered from a different perspective.<sup>66</sup>

This is not to say that reasonableness requires assuming that everyone is pursuing internal goods. Instead, reasonableness involves a more-or-less open-minded, empirical investigation: Does this person describe their own activities in terms that could be related to internal goods? In their activities and deliberation, do they distinguish their ends from external goods? If I previously thought that they only pursued external goods, could I have been wrong? If they do seem to have internal goods, do they prioritize them in cases of institutional tension? And so on. This is the same kind of investigation as we conducted of Nazi science in §5.5. As I argued there, it's not entirely clear whether the Nazi movement had any internal goods, but it's not implausible to maintain that they did. However, even if the movement did have internal goods, they suffered severe institutional domination, and this extended to scientific inquiry under the Nazi regime.

In cases like the abortion dispute, if joint practice is possible it will require reasonableness. Indeed, a successful joint practitioner, by fostering the kind of mutual understanding that is currently missing from the abortion dispute, might make more people more reasonable. This suggests that reasonableness is an ongoing project: to deal with the pernicious problem of unreasonableness, we must cultivate the virtue of reasonableness.

So the fact that non-ideal political liberalism confronts is somewhat less the fact of reasonable pluralism, and more the fact of unreasonable disagreement. Cultivating the virtue of reasonableness helps, but this is an ongoing process, and we can't wait to raise issues of justice and deliberate over public policies until we've all become perfectly reasonable. In particular, we can't wait until we all share the same comprehensive conception of the good — until we agree on some fundamental metaphysical and ethical commitments. However, assuming everyone involved is reasonable to at least *some* degree (though perhaps even this assumption is too bold), to some extent we can bracket the fundamental disagreements by articulating our views and concerns in a “mid-level” language, using terminology that we all share. For example, Americans seem to generally, more-or-less, agree that freedom, equality, and fairness are important, although they do not agree on the more fundamental ideas that support these three concepts. Then, if we can articulate the badness of politicized science in terms of violations of freedom or fairness, we may be able to achieve some

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<sup>66</sup> In some respects, this account of reasonableness resembles MacIntyre's account of what is required for interactions between rival ethical traditions; see, for example, MacIntyre, “[Moral Pluralism without Moral Relativism](#).” But MacIntyre's account is agonistic: he takes the exchange between traditions to be, in the first instance, the exchange of criticisms of the rival traditions. My account is much more irenic: what is exchanged is, in the first instance, positive explanations of one's own views.

general, more-or-less agreement that politicized science is bad, despite the ongoing disagreements at the more fundamental level.<sup>67</sup> This mid-level language, along with the substantive claims articulated in it, is the political conception.

At this point we can return to Rawls. The aim of a political conception is not truth, but rather the “practical” aim of “serv[ing] as a basis of informed and willing political agreement between citizens viewed as free and equal persons”;<sup>68</sup> it is “a shared point of view from which [citizens] can resolve questions concerning the constitutional essentials.”<sup>69</sup> This is what Rawls means when he calls the political conception the object of a “reasonable overlapping consensus”: “the political conception is supported by the reasonable though opposing religious, philosophical, and moral doctrines that gain a significant body of adherents and endure over time from one generation to the next.”<sup>70</sup> Thus a political conception claims something much closer to warranted assertability in the context of a contemporary liberal democracy than universal (or universally-recognizable) truth.<sup>71</sup> Citizens, of course, have their own comprehensive conceptions — they are members of different religions, for example. Thus they do not all accept the political conception *for the same reasons*. For example, one group of citizens might accept the political conception based on the principles of Thomistic natural law and the Catholic doctrine of *imago dei*, while another group might accept it based on comprehensive liberal principles of rational autonomy. Simplifying things immensely, we can think of the political conception as entailed by each reasonable comprehensive conception, but not entailing any of them. Over time, citizens’ comprehensive conceptions may be influenced by the political conception — the political equality and autonomy they enjoy as citizens might lead them to push for more egalitarian relations within their church, for example, or lead economic egalitarians to be unwilling to restrict political and religious liberties. But the political conception does not *require* this, in any sense.

At the core of the framework of political liberalism is a commitment to “the underlying ideas of citizens as free and equal persons and of society as a fair system of cooperation over time.”<sup>72</sup> This does not mean that citizens are taken to be free in the sense of having free will, or equal in the sense of having the same fundamental human nature, but rather free in the sense of being politically responsible for having and pursuing their conception of the good and “being entitled [individually] to make

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<sup>67</sup> How much agreement? It turns out that’s not quite the right question; we’ll come back to these issues in §7.6.

<sup>68</sup> Rawls, “Justice as Fairness,” 230.

<sup>69</sup> John Rawls, *Justice as Fairness: A Restatement*, ed. Erin Kelly (Cambridge, MA: Harvard University Press, 2001), 32, ISBN: 9780674005105.

<sup>70</sup> *Ibid.*

<sup>71</sup> For a strong — sometimes too strong — interpretation of this last point, see Richard Rorty, “The Priority of Democracy to Philosophy,” in *Objectivity, Relativism, and Truth* (Cambridge, UK and New York: Cambridge University Press, 1991), 175–95.

<sup>72</sup> Rawls, *Political Liberalism*, 450.

claims on their institutions,”<sup>73</sup> and equal in the sense of being able to participate (in various ways) in the ongoing activities of their society.<sup>74</sup> These claims are not metaphysical — they do not claim that persons are created in the image of God, nor that the only unconditionally good thing is the Kantian autonomous will. Political liberalism’s understanding of freedom is compatible with both of these options, and (arguably) is supported by both, but does not entail either. This is what Rawls means when he describes the conception of justice as “political not metaphysical.”

A development of this framework operationalizes these ideas of freedom, equality, and fairness in such a way that they support an account of justice.<sup>75</sup> Rawls operationalizes these ideas in various features of his preferred political conception, which he calls justice as fairness. For example, the commitment to fairness is operationalized in part by maximin reasoning behind the veil of ignorance: roughly, behind the veil, I do not know my determinate conception of the good or other aspects of my individual interests, and so cannot choose principles of justice that maximize that interest; instead, I maximize the interest of the least advantaged person, whatever their conception of the good may be, and thereby promote the interests of all persons in general; that is, the chosen principles are fair. However, Rawls is explicit that justice as fairness is only one way of developing the framework of political liberalism:

Others will think that different ways to identify these principles [of justice] are more reasonable. Thus, the content of public reason is given by a family of political conceptions of justice, and not by a single one. There are many liberalisms and related views, and therefore many forms of public reason specified by a family of reasonable political conceptions.<sup>76</sup>

Other members of this family include Habermas’ discourse ethics, Catholic Social Thought, and the capabilities approach.<sup>77</sup>

So I take it that the following are the criteria for the development of a political conception:

- (1) Negatively, the development is “political, not metaphysical” rather than a comprehensive conception, meaning
  - (a) It does not rely on reasonably controversial metaphysical claims,
  - (b) It deals only with the basic structure of society, and

<sup>73</sup> Rawls, *Justice as Fairness*, 23.

<sup>74</sup> *Ibid.*, 20.

<sup>75</sup> Because different reasonable comprehensive conceptions understand the justification of an account of justice — and other ethical claims — in different ways, political liberalism must be careful about the support relationship between the operationalized underlying ideas and the account of justice. For Rawls’s discussion of this issue with respect to his preferred political conception, see Rawls, *Political Liberalism*, lecture III.

<sup>76</sup> *Ibid.*, 450.

<sup>77</sup> Rawls is explicit about the first two: *ibid.*, 451-2.



- (c) It aims to be the object of an overlapping consensus among reasonable comprehensive conceptions.
- (2) Positively, it operationalizes the three underlying ideas or commitments, namely,
- (a) To citizens as free and
  - (b) Equal persons, and
  - (c) To society as a fair system of cooperation over time.

Note that these criteria do *not* include or entail any of the following:

- The device of the original position, or any other hypothetical procedure,
- The priority of the right to the good, in the sense that the conception of justice is independent of a substantive conception of the good,<sup>78</sup> or
- An ideal theory approach to justice.

Recall from §1.2.5 that, on an ideal theory approach to justice, the primary aim of political philosophy is an account of the perfected state of society, that is, of a perfectly just society. By contrast, according to non-ideal theory, the primary aim is an account of the actual, imperfect state of our society, that is, of the specific justices and injustices of our actual society. Non-ideal theorists, more so than ideal theorists, are explicit about intending to produce only partial and tentative accounts. I gave the example of Charles Mills, who recognizes that racial injustices are not the only injustices but (at least in *The Racial Contract*) aims to give only an account of racial injustices.

A development of political liberalism that draws on the conception of practice should, I think, be a non-ideal theory, and this for two reasons. First, practices and institutions are inevitably tied to each other — every practice needs the support of some institution — and yet inevitably at odds — designed to pursue radically different goods. A perfectly just society, on such a view, would be precariously balanced between institutional and practical priority, and I do not see how such a balance could be stabilized. The tension between practices and institutions, while sometimes lessened, can never be eliminated in any remotely realistic possibility.

Second, and more contingently, I see no straightforward way to combine the conception of practice with a discussion of what we might call “basic injustice.” As a first pass, suppose our conception of justice simply identified injustice with institutional domination, and consider how such a conception would give an account of the injustice of ethnic genocide. Presumably the ethnic power structure of society could be analyzed as an institution,<sup>79</sup> and presumably it is true that, in carrying out the genocide, this institution interferes with the practices of members of the targeted group in

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<sup>78</sup> *Ibid.*, lecture V.

<sup>79</sup> See below, §7.5.



ways that constitute institutional domination. That is, genocide is unjust (on such an account) because it interferes with the artistic, farming, family, and other practices of the target group. The claim after “because” in the last sentence is entirely true. But as an account of injustice it is, to borrow from Bernard Williams in a radically different context, one thought too many. Genocide is unjust because it is systematic, hate-based murder on a massive scale. The interference with practices is, as it were, an incidental and relatively unimportant side effect of the systematic murder.<sup>80</sup>

So a conception of justice in terms of practices and institutional domination is only partial. In this respect, it is like Mills’s account of racial injustice in *The Racial Contract*: while recognizing that there are other important injustices not covered by the account, it deals exclusively with a narrowly focused class of injustice. Hopefully, in the future, the account can be expanded, or supplemented, or combined with accounts that deal with these other injustices. But, for now, we are only dealing with institutional domination. Such an approach, I think, is much more at home with other partial, non-ideal theories.

With the framework of political liberalism laid out, I turn to the question of whether it can be developed in terms of the conception of practice, and in such a way that it supports the claim that the institutional domination of scientific inquiry is a practice. Over the next several sections, I shall use the five criteria given above to organize my answer to this question. In each case, my foil will be an objection of the form that the conception of practice is incompatible with the criterion.

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<sup>80</sup> The account in terms of practices may be able to defend itself in the case of genocide. If the aim of genocide is less to kill off all members of the group in question than to disrupt permanently their status as a group, then it might be plausible to say that the aim of genocide is to disrupt the practices that constitute this group as a group. For example, it may be plausible to claim that the purpose of the Jewish Holocaust under the Nazi regime was less to kill off every single Jew than it was to eliminate the practices that constituted Jewish life. Similarly, the genocides of indigenous peoples in North America and Australia were not (or not just) campaigns to kill off and displace these peoples; rather, they were campaigns to eliminate the practices that constituted the ways of life of these peoples. For an analysis of the Bosnian genocide along these lines, see the essays in Catharine MacKinnon, *Are Women Human? And Other International Dialogues* (Cambridge, MA and London: Harvard University Press, 2006), part three, ISBN: 9780674025554. I take it, however, that similar moves are much more difficult to make in other cases of basic injustice, such as mass starvation due to government corruption and resource mismanagement.

## 7.4 Individualism and the metaphysics of collective agents

### 7.4.1 Individualism, metaphysical and otherwise

The objection considered in this section is that the conception of practice relies on metaphysical claims that are reasonably controversial. The framework of political liberalism speaks of individual citizens and the social organizations that they form; it is individualist, like other versions of liberalism. But the conception of practice is the product of an intellectual tradition that, running back through Marx and the Young Hegelians, has criticized liberal individualism. The conception of practices refers to entities — practices, or non-equivocally, their organized communities of practitioners — that are incompatible with the metaphysical assumptions of any version of liberalism, including political liberalism.

The sketch of the preceding paragraph must be filled in further before I can respond to it; and, as we shall see below, despite being often phrased in terms of “metaphysics” or “ontology,” I think the concern is not really a metaphysical one. To fix the terminology a bit, start with a distinction between *individual agents* and *collective agents*.<sup>81</sup> An individual agent is a single human being (or other social and rational being, such as a chimp or a wolf) engaged in some activity, typically self-conscious purposive activity involving the exercise of reason and other capabilities. An individual agent *might* be taken to act (on a given occasion or in general) in her own self-interest, without collaboration or assistance or as part of a team or group, and so on. But she may *also* be taken to act collaboratively and for the sake of mutual benefit (as such) or the realization of common goods. That is, an individual agent might but need not act egoistically. A collective agent — also often called a group agent — is a collection of individual agents engaged in some collaborative activity. I will say that a collective agent is composed of its individual agents; this composition relation must be understood ambiguously or equivocally, since different kinds of collective agents will relate to their individual agents in different ways. Hence the terminology itself should not bear much philosophical weight; I simply wish to avoid such awkward constructions as “single member of a collective agent” to refer to individual agents who compose collective agents. The organized set of practitioners of a practice — which, synechdocheally, I have often referred to as the practice — is an example of a collective agent, and the practitioners can be said to be individual agents even when engaged in the collaborative pursuit of the internal goods of the practice. So, for example, a lab of scientists is a collective agent and each scientist in

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<sup>81</sup> For overviews of the literature on collective agency see Roth, “[Shared Agency](#)”; Miller, “[Social Institutions](#)”; one account that has influenced my thinking is Mark Brown, *Science in Democracy: Expertise, Institutions, and Representation* (Cambridge, MA: MIT Press, 2009), ISBN: 9780262513043; two recent accounts include List and Pettit, *Group Agency*; and Isaacs, *Moral Responsibility in Collective Contexts*.

the lab is an individual agent, even when several of the latter are working together to produce some representational knowledge.

The terminology of individual and collective agents lets us start distinguishing various kinds of individualism. Again, the aim at this point is to clarify the objection sketched in the first paragraph of this section.

**ontic individualism:** There are no collective agents, only individual agents.

Ontic individualism is a metaphysical claim, and the strongest sort of individualism: if ontic individualism is true, then every other sort of individualism is trivially true. Generally liberal political philosophers reject ontic individualism, because generally they accept the existence of *associations*. An association, as I will understand it, is a collective agent that its individual agents organize and join voluntarily in order to promote some set of their exogenously-given aims. Society, on Hobbesian accounts, are paradigmatic examples of associations: Individuals in the state of nature have various desires, which are frustrated by the interference of other individual agents. These individuals establish a society, that is, agree to either live by the laws established by a state or be subject to coercive force at the hands of state representatives; this is done in order to minimize the frustrating interferences of other individuals.<sup>82</sup> Note that membership in such a society is voluntary: individuals are not required to join society, and are free to leave at their discretion. And the desires and aims of citizens within society are taken to be the same as their desires and aims as extra-social or pre-social individuals; in this sense, their desires are exogenous to the association.

More strictly, I think the notion of *exogenicity* is best understood as a relation between the aims of a collective agent and the aims of its individual agents, rather than a property of one or the other set of aims. On the one hand, the aims of the collective agent depend only on (some subset of) the aims of its individual agents. On the other hand, the aims of its individual agents are not influenced by the collective agent as such. This is not to say that either the *means* by which individual agents pursue their aims or their *success* at realizing their aims are not influenced by other agents, including any collective agents of which they might be a part. Exogenicity is consistent with the fact that I cannot realize my aims of both being a successful academic philosopher and living in my small, semirural hometown. It is not consistent with the fact that I aim to live in my small, semirural hometown because I was, for at least for an important part of my life, part of the collective agent of my small, semirural hometown.

Liberals also generally recognize the existence of *aggregations*. An aggregation is a set of individual agents who each have some feature that is taken to be socially salient (in a given social context, and so on). For example, if race (however it is defined) is

<sup>82</sup> For two influential contemporary Hobbesian accounts, see David Gauthier, *Morals by Agreement* (Oxford and New York: Oxford University Press, 1986), ISBN: 0198249926; Jan Narveson, *The Libertarian Idea* (Philadelphia: Temple University Press, 1988), ISBN: 0877225699.

taken to be socially salient, each distinct race is a distinct aggregation. Similarly, in all human societies, each gender (however many there are) is an aggregation. Aggregations can also be understood purely behaviorally; for example, routine consumers of organic produce form an aggregation. Note that, strictly speaking, all collective agents are aggregations, but some aggregations are not organized or internally coordinated, and hence not all aggregations are collective agents. Often “aggregation” is used for aggregations that are not collective agents, which we might also call strict aggregations.

Practices are not associations. In at least some cases, participation in them is not voluntary. Family membership is almost entirely involuntary: with the exception of spouses, we do not choose our relatives, and all human societies have governmental and social mechanisms for enforcing obligations to our relatives (though the extent of these obligations vary widely among societies). More importantly, the aims of practitioners, as such — the internal goods — are not exogenous, in two ways. First, per the recognition claim, the internal goods cannot be recognized independently of participation in the activity of the practice. As an individual is gradually educated into the practice, and so improves their understanding of the internal goods (especially recognizing that their value cannot be measured in terms of external goods), their aims change. Hence their aims are influenced by the collective agent as such. Second, as noted in the discussion of the internal good of progress in §2.4.2, it is strictly incorrect to say that the aim of the practice is the production of its internal goods as they are understood at any given moment. In other words, the practitioners may be *wrong* about (on a telic conception) or have an *inadequate understanding* of (on a melioric conception) the internal goods of their practice. Hence the aims of the practice as a collective agent do not depend only on the aims of the individual practitioners, at least at a given time.

The analysis of the last few paragraphs indicates that the issue of ontic individualism that is relevant to the objection — whether the framework of political liberalism denies the existence of practices — really turns on another issue, namely, the relationship between the aims of individual and collective agents.

**independence individualism:** For any individual agent, achieving that agent’s aims does not require that agent’s membership in any collective agent.

In the communitarian-liberalism debate of the 1980s, independence individualism is known most infamously as *atomism*.<sup>83</sup> And the conception of practice, which assumes that every individual agent has among her aims the internal goods of some practice, is incompatible with independence individualism.

It is not clear, however, just how widely independence individualism has been held in the liberal tradition of political philosophy. In the *Second Treatise of Government*, for example, Locke writes that

<sup>83</sup> See, for example, Charles Taylor, “Atomism,” in *Philosophy and the Human Sciences* (Cambridge, UK: Cambridge University Press, 1985), 188–210.

God having made man [sic] such a creature that, in God's own judgment, ought not to be alone, drew him strongly — by need, convenience, and inclination — into society, and equipped him with understanding and language to keep society going and to enjoy it. The first society was between man and wife, which gave rise to the society between parents and children; to which in time the society between master and servant came to be added . . . . Each of these smaller societies, like the larger one of the entire household, fell short of being a *political* society . . . .<sup>84</sup>

That is, Locke seems to believe that even in the state of nature (outside of “political society”), well-being requires that individual agents become members of collective agents — paradigmatically, families. Similarly, Mary Lyndon Shanley argues that Mill's *The Subjection of Women* “emphasizes the value of nonsinstrumental relationships in human life” and offers a “vision of marriage as a locus of mutual sympathy and understanding between autonomous adults” that, when realized, “will enable couples to live in genuine equality, mutuality, and reciprocity.”<sup>85</sup> That is, Mill seems to think that membership in a specific type of collective agent — an egalitarian, though still heterosexual, family — is required for individuals' well-being. And Jan Narveson's argument for a libertarian society (with a minimal state) comes down to the claim that such a society Pareto-dominates a Hobbesian state of nature.<sup>86</sup> That is, every individual is significantly better off in libertarian society than in the Hobbesian state of nature. In each case, the individual agent's well-being or aims requires participation in the right sort of collective agent, apparently contradicting independence individualism.

In response to this reading of the liberal tradition, we can distinguish two ways in which the well-being of an individual agent depends on membership in a collective agent.

**instrumental dependence:** The most effective and efficient way for an individual agent to achieve her or his aims involves membership in some collective agent.

**constitutive dependence:** Among the aims of the individual agent is membership in some collective agent.

On instrumental dependence, membership in the collective agent is instrumentally valuable; this sort of dependence is compatible with exogenicity. On constitutive

<sup>84</sup> John Locke, *Second Treatise of Government*, ed. Jonathan Bennett (Some Texts from Early Modern Philosophy, 2008), §77, 25, some editorial marks omitted, <http://www.earlymoderntexts.com/lo2tr.html>.

<sup>85</sup> Mary Lyndon Shanley, “Marital Slavery and Friendship: John Stuart Mill's *The Subjection of Women*,” *Political Theory* 9, no. 2 (May 1981): 230 See also Nussbaum, “[The Feminist Critique of Liberalism](#),” 60.

<sup>86</sup> Narveson, *The Libertarian Idea*, 175-7.

dependence, by contrast, membership in the collective agent is (at least partly) constitutive of the agent's set of aims; this sort of dependence is not compatible with exogenicity. The arguments from Locke and Narveson seem to be for instrumental dependence but not constitutive dependence; the quotations from Shanley on Mill could be read either way. Furthermore, the conception of practice requires constitutive dependence. Indeed, arguments for constitutive dependence are central to the critique of liberal political philosophy of the broadly Marxian tradition that produced the conception of practice.<sup>87</sup> I suggest that this is in the sense in which liberalism is thought to be wrongly individualistic.

We now have sufficient background to articulate the key premise in the objection that I am considering in this section: *The conception of practice's commitment to constitutive dependence is reasonably controversial.*

The objection could also be put, as it were, from the other side, that is, from the leftist intellectual tradition that rejects liberalism on the grounds that liberalism is incompatible with constitutive dependence. Indeed, MacIntyre himself is part of this tradition. Recall that *After Virtue* can be divided into two halves. In the first half, he criticizes liberalism on roughly the grounds that, as both an intellectual and social project, it has failed according to its own standards. In the second half, he introduces his alternative, a renewal of Aristotelean ethical and social theory. (At this point in his career, MacIntyre is not quite a Thomist.) The conception of practice is introduced roughly halfway through this second half — roughly three-quarters of the way through the book — in his first argument for the classical virtues, which I called the *practice argument* back in chapter 2. From this perspective, liberalism, with its rejection of constitutive dependence, has all but collapsed; the conception of practice is meant to replace liberalism, not fruitfully inform a new version of it.

To respond to this objection, I first note that none of the quotations from liberal thinkers above are arguments against constitutive dependence, or for the corresponding form of individualism; rather, they are arguments for instrumental dependence. Furthermore, I follow Charles Taylor in thinking that worries about constitutive dependence are usually worries about the use of tyrannical state power to coerce citizens into certain forms of collective agent membership — about appeals to constitutive dependence to rationalize totalitarianism.<sup>88</sup> I take it that there is no reasonable defense of totalitarianism, nor is there reasonable disagreement over whether constitutive dependence has been utilized in this way. So a reasonable conception of practice must

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<sup>87</sup> See, for example, Sean Sayers, "Individual and Society in Marx and Hegel: Beyond the Communitarian Critique of Liberalism," *Science and Society* 71, no. 1 (January 2007): 84–102; Michael Sandel, *Liberalism and the Limits of Justice*, second edition (Cambridge, UK and New York: Cambridge University Press, 1998), esp. 142ff.; ISBN: 9780521567411; Jaggar, *Feminist Politics and Human Nature*; Taylor, "Atomism"; and, of course, MacIntyre, *Dependent Rational Animals*.

<sup>88</sup> Charles Taylor, "What's Wrong with Negative Liberty?" In *Philosophy and the Human Sciences* (Cambridge, UK: Cambridge University Press, 1985), 211–29.



recognize limits on the use of the state to promote collective agent membership. And I see absolutely no reason to think that this cannot be done.

I will grant that, within the tradition of liberal political philosophy, little weight has been put on our constitutive dependence, and this is why criticisms of liberalism have often taken the form of arguments for constitutive dependence. However, in response to feminist, disability theorist, and communitarian arguments, liberal political philosophers have begun to take constitutive dependence much more seriously.<sup>89</sup> Furthermore, two historic liberal political philosophers explicitly endorsed constitutive dependence, and used their understanding of it to do significant theoretical work: John Dewey and John Rawls. By investigating the role of constitutive dependence in their contributions to liberal political philosophy, we can see that liberalism as such is not incompatible with constitutive dependence.

### 7.4.2 Dewey

In *Reconstruction in Philosophy*, among numerous other works, Dewey criticizes the two views in social and political philosophy that were dominant in his day. On the one hand is individualism, which is based “upon the belief that individuals are alone real, that classes and organizations are secondary and derived.”<sup>90</sup> On the other is “organicism,” a loosely Hegelian view on which “society and individuals are correlative, organic, to one another, society requiring the service and subordination of individuals and at the same time existing to serve them.”<sup>91</sup> I will only discuss Dewey’s criticisms of the former here.

The basic or primary problem with individualism is its rejection of constitutive dependence. In the following paragraph, Dewey first rejects individualism on these grounds, then presents his own positive view:

The real difficulty [with the individualist view] is that the individual is regarded as something *given*, something already there. Consequently, he can only be something to be catered to, something whose pleasures are to be magnified and possessions multiplied. When the individual is taken as something given already, anything that can be done to him or for him can only be by way of external impressions and belonging: sensations of pleasure and pain, comforts, securities. Now it is true that social arrangements, laws, institutions are made for [people], rather than that [a person] is made for them; that they are means and agencies of human welfare and progress. But they are not means for obtaining something for

<sup>89</sup> See, for example, Amy Gutmann, “Communitarian Critics of Liberalism,” *Philosophy and Public Affairs* 14, no. 3 (Summer 1985): 308–22; Nussbaum, *Frontiers of Justice*.

<sup>90</sup> John Dewey, *Reconstruction in Philosophy*, in *John Dewey: The Middle Works, 1899–1924*, ed. Jo Ann Boydston, vol. 12: 1920 (1920; Carbondale and Edwardsville: Southern Illinois University Press, 1982), 190.

<sup>91</sup> *Ibid.*, 187.



individuals, not even happiness. They are means of *creating* individuals . . . . Individuality in a social and moral sense is something to be wrought out. It means initiative, inventiveness, varied resourcefulness, assumption of responsibility in choice of belief and conduct. These are not gifts, but achievements. As achievements, they are not absolute but relative to the use that is to be made of them.<sup>92</sup>

First, Dewey rejects individualism in the form of what I have called exogenicity: on his view, the individual agent's aims are not independent of the aims of the collective agent; rather, membership in (certain kinds of) collective agents causes the aims of individual agent to include or become the aims of individuality. These aims are not given; they are the product of membership in (again, certain kinds of) collective agents. Second, in the last few sentences, Dewey hints at the idea that these aims themselves involve (further) membership in (certain kinds of) collective agents. Initiative, inventiveness, and so on, are not realized by setting off into the asocial wilderness. Rather, they are ways of being involved with the activities of collective agents.

This last point is especially clear on the next page, when Dewey discusses “the old dispute of whether reform should start with the individual or with institutions”:

When self-hood is perceived to be an active process it is also seen that social modifications are the only means of the creation of changed personalities. Institutions are viewed in their educative effect:—with reference to the types of individuals they foster. The interest in individual moral improvement and the social interest in objective reform of economic and political conditions are identified. And inquiry into the meaning of social arrangements gets definite point and direction. We are led to ask what the specific stimulation, fostering and nurturing power of each specific social arrangement may be.<sup>93</sup>

A reformer, as a member of a collective agent undergoing self-reform, requires individuality in exactly the sense of the last block quotation: the reformer takes initiative, invents new forms of the activities of the collective agent, assumes responsibility for their beliefs about the aims of the reform and the actions they and others will take to realize those aims. As the work of reform incorporates other individual members of the collective agent — as the reform movement gains followers — it fosters individuality in them as well. On the other hand, it is not enough for the reformers to simply adopt the aims of reform and exhort others to join them; effective reform requires changing the aims, activities, and social organization of the relevant collective agents.

This leads to a much deeper problem with the version of individualism that Dewey is attacking: insofar as the individual agent is taken to be given or fixed, the work of

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<sup>92</sup> [Ibid.](#), 190-1, his emphasis.

<sup>93</sup> [Ibid.](#), 192.

reform cannot change the individual agent in any way. Either reform merely tweaks the instrumental efficiency of associations in achieving the given aims of individual agents or it requires surd, Pauline changes in the aims of individual agents. And thus, for example, the insurmountable practical problem for the tradition of utilitarian reformers, from Bentham to welfare economists to Peter Singer: getting those with a great deal of utility to sacrifice for the benefit of those with very low or negative utility. In contrast, Deweyan reform, even radical reform, cultivates and realizes the individuality of the individual reformers, and so becomes constitutive of their individual interests. There is a practical problem here, but it is a relatively minor epistemic problem rather than an insurmountable problem of motives and interests: potential reformers have to recognize initially that the work of reform can lead them to develop, realize, and be deeply satisfied by aims that they do not currently have.

Operationalized, these two problems lead to very different forms of moral education. For utilitarians, there are empirical reasons to think that reasoning and arguments are ineffective, or at least not sufficiently effective,<sup>94</sup> and so moral education ultimately takes the form of irrational emotional manipulation: exposing the well-off to wrenching representations of the lives of the poorly-off in the hopes that feelings of guilt and sympathy will lead to sacrifices. On a Deweyan model, moral education takes the form of involving individuals — well-off or otherwise — in activities that contribute to the aims of reform, having them reflect on these activities and their own individual aims, and providing them with further opportunities to involve themselves in reform.<sup>95</sup> In abstract terms, it involves cultivating Deweyan individuals by introducing them to the right kinds of collective agents.

### 7.4.3 Rawls

In the work of John Rawls, the conception of society as a “social union of social unions” plays an important but often overlooked role in the argument for justice as fairness. In *A Theory of Justice*, it appears in part three, which is devoted to Rawls’s

<sup>94</sup> For example, Peter Singer’s early essays on poverty and famine have been widely read in introductory philosophy classes for on the order of four decades, but do not seem to have had an effect on charitable donations or support for funding for poverty alleviation of anything like the magnitude Singer envisioned. See, for example, Peter Singer, “Famine, Affluence, and Morality,” *Philosophy and Public Affairs* 1, no. 3 (Spring 1972): 229–43.

<sup>95</sup> Perhaps obviously, I have service learning in mind here. For a discussion of service learning in Deweyan terms, see Judith Green, “Deepening Democratic Participation through Deweyan Pragmatism: Concepts and Models for Service-Learning in Philosophy,” in *Beyond the Tower*, ed. C. David Lisman and Irene E. Harvey (Washington, DC: American Association for Higher Education, 2000), 123–38, ISBN: 1563770164. For a discussion of the effectiveness of “short-term,” intensive service learning as moral education, see Nicholas Bowman et al., “Sustained Immersion Courses and Student Orientations to Equality, Justice, and Social Responsibility: The Role of Short-Term Service-Learning,” *Michigan Journal of Community Service Learning* 17, no. 1 (Fall 2010).

argument for the “congruence of the right and the good.” The precise meaning of this phrase will not concern us here; all that matters is that Rawls takes this argument to be extremely important for his project and that it “depends in large part upon whether a well-ordered society achieves the good of community”;<sup>96</sup> the idea of social unions, and the conception of society as a social union of social unions, is introduced to argue that a well-ordered society does indeed achieve this good. By *Political Liberalism*, however, the idea of social unions has nearly vanished; it is now used as just one of three arguments (and the weakest of the three) for the priority of the basic liberties in the ordering of the two principles of justice as fairness.<sup>97</sup> While Rawls does argue that “the good of political society” is part of the conception of the good in justice as fairness, this argument does not appeal to the concept of a social union.<sup>98</sup> It is worth taking a few paragraphs to understand why Rawls changed his argument in this way.

Rawls explicitly derives the idea of a social union from Wilhelm von Humboldt, saying that it “must have occurred to many and is surely implicit in numerous writings” yet von Humboldt gives one of the “few definite formulations of it as expressed in this section.”<sup>99</sup> In both *Theory* and *Political Liberalism*, Rawls gives the same long quotation from von Humboldt, which I excerpt here:

Every human being . . . can act with only one dominant faculty at a time . . . . It would, therefore, seem to follow that man [sic] is inevitably destined to a partial cultivation . . . . But man [sic, etc.] has it in his power to avoid one-sidedness, by attempting to unite distinct and generally separately exercised faculties of his nature, by bringing into spontaneous cooperation, at each period of his life, by dying sparks of one activity, and those which the future will kindle . . . . What is achieved in the case of the individual, by the union of past and future with the present, is produced in society by the mutual cooperation of its different members . . . . It is through social union, therefore, based on the internal wants and capabilities of its members, that each is enabled to participate in the rich collective resources of all the others.<sup>100</sup>

So far, a social union might sound like an association, assembled to overcome instrumental dependence: individuals are unable to realize all of their ends on their own, so they coordinate their activities with other individuals for more efficiency. As Rawls puts it, “persons need one another, since it is only in active cooperation with others that any one’s talents can be realized, and then in large part by the efforts of

<sup>96</sup> Rawls, *A Theory of Justice*, 456.

<sup>97</sup> Rawls, *Political Liberalism*, 320ff.

<sup>98</sup> *Ibid.*, 201-6.

<sup>99</sup> Rawls, *A Theory of Justice*, 459n.

<sup>100</sup> Quoted in Rawls, *A Theory of Justice*, 459n; Rawls, *Political Liberalism*, 320-1; first ellipses Rawls’s, others mine.

all.”<sup>101</sup> However, in *Theory*, Rawls introduces social unions by contrasting them with another kind of collective agent, which he calls a “private society”:

Its [private society’s] chief features are first that the persons comprising it . . . have their own private ends which are either competing or independent, but not in any case complementary. And second, institutions are not thought to have any value in themselves, the activity of engaging in them not being counted as a good but if anything as a burden. Thus each person assesses social arrangements solely as a means to his private aims . . . . (Expressed more formally, the only variables in an individual’s utility function are commodities and assets held by him, and not items possessed by others nor their level of utility.)<sup>102</sup>

In short, a private society is precisely what I have called an association, and the salient difference between private societies and social unions is what I have called exogenicity. Rawls then goes on to reject constitutive independence:

It is sometimes contended that the contract doctrine entails that private society is the ideal . . . . But this is not so . . . . Now the sociability of human beings must not be understood in a trivial fashion. It does not imply merely that society is necessary for human life, or that by living in a community [humans] acquire needs and interests that prompt them to work together for mutual advantage . . . . Nor is it expressed by the truism that social life is a condition for our developing the ability to speak and think, and to take part in the common activities of society and culture . . . . For all these things are equally true of persons who view their relations purely instrumentally.

The social nature of [humankind] is best seen by contrast with the conception of private society. Thus human beings have in fact shared final ends and they value their common institutions and activities as good in themselves. We need one another as partners in ways of life that are engaged in for their own sake, and the successes and enjoyments of others are necessary for and complement to our own good.<sup>103</sup>

That is, Rawls rejects the view that the only collective agents are associations precisely on the grounds of constitutive dependence. He introduces the idea of social unions precisely to illustrate our constitutive dependence.

Social unions share a number of features with practices. Both are directed towards shared final ends, and achieving these ends is a collective achievement of all the participants as such. Furthermore, “the common aim is often profound and complex, being defined by the respective artistic, scientific, or religious tradition; and to

<sup>101</sup> Rawls, *Political Liberalism*, 321.

<sup>102</sup> Rawls, *A Theory of Justice*, 457, his parentheses.

<sup>103</sup> *Ibid.*, 458.

understand this aim often takes years of discipline and study.”<sup>104</sup> Of course, both are incompatible with exogenicity. And the examples that Rawls gives are the same that MacIntyre gives in *After Virtue*: science, families, friendships, games, art, religion, and culture.

This is not to say that social unions are the same as practices, or that we can simply “plug in” practices for social unions in Rawls’s justice as fairness. My claim here, rather, is that Rawls’s framework is compatible with constitutive dependence, and so the objection — that this framework is incompatible with the conception of practice because the latter assumes constitutive dependence — is unsound.

Finally, according to the argument of *Theory*, a well-ordered society is itself a social union; thus the conception of society as the social union of social unions, and the realization of the good of community in justice as fairness.<sup>105</sup> If the conception of social union were compatible with the framework of political liberalism, then it would seem that the conception of practice is just as compatible. But, as mentioned above, Rawls abandons the argument of the last several paragraphs in his later work.

It is not that Rawls completely rejects the concept of a social union as such as being incompatible with political liberalism. Again, social unions play a small role in the reworked argument for justice as fairness. While Rawls does not lean as heavily on constitutive dependence in *Political Liberalism* as he did in *Theory*, there are still hints that he rejects exogenicity: in the well-ordered society, persons’ “conceptions [of the good] will be enlarged and sustained by the more comprehensive good of social union.”<sup>106</sup>

To understand why Rawls abandons the argument from social unions, we need to examine the argument in *Political Liberalism* that takes its place, that is, the argument that the good of “political society” is part of the conception of the good of justice as fairness.<sup>107</sup> Again, Rawls’s foil is the view that all collective agents are associations. But now, Rawls seems compelled to concede that

justice as fairness does indeed abandon the ideal of political community if by that ideal is meant a political society united on one (partially or fully) comprehensive religious, philosophical, or moral doctrine. That conception of social unity is excluded by the fact of reasonable pluralism.<sup>108</sup>

The appendix to the discussion of the new argument clarifies Rawls’s reasoning. Political liberalism is incompatible with “civic humanism,” the view that a human “is a social, even a political, animal whose essential nature is most fully realized in a

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<sup>104</sup> *Ibid.*, 461.

<sup>105</sup> *Ibid.*, 462ff.

<sup>106</sup> Rawls, *Political Liberalism*, 323.

<sup>107</sup> *Ibid.*, §7.

<sup>108</sup> *Ibid.*, 201.

democratic society in which there is widespread and vigorous participation in political life . . . . [T]aking part in democratic politics is seen as the privileged locus of the good life.”<sup>109</sup> In the next paragraph, Rawls calls civic humanism a “comprehensive conception.” And now Rawls’s worry is clear: the instance of constitutive dependence that he endorsed in *Theory* — namely, that among the aims of the individual agent is, or should be, membership in the social union of the social union, concretely understood as the political system of the well-ordered society — is a version of civic humanism. As a comprehensive conception, Rawls cannot draw on it when developing the political conception of justice as fairness. The idea of a social union as such is not problematic — which is why Rawls can appeal to *that* idea elsewhere — but he cannot give an account of the good of community or political society in terms of the *particular* social union that is the well-ordered society.

But the explication in the last sentence shows that the conception of practice, with two conditions, is entirely at home in a political conception. So long as the “political conception of practices” neither (a) singles out one practice as the highest or all-encompassing practice (as the social union of social unions is) nor (b) covers all human activities, it can serve in a political conception.

However, satisfying these two conditions requires a dramatic departure of *my* conception of practice from MacIntyre’s. Satisfying (b) requires explicitly limiting the conception to the basic structure of society; this will be done in §7.5. To satisfy (a), we must reject the view that the practice of politics, as MacIntyre understands it, is at the top of the hierarchy of practices of a flourishing local community. As MacIntyre sees it, politics is a practice; in brief, the internal good of this practice is an ordering of the internal goods of all of the other practices of the community, that is, an understanding of relations among these practices, and especially an understanding of their relative importance and priority. Such an ordering is required for both the flourishing of the local community and the flourishing of the individual practices it comprises. Politics is thereby regulative and, in some sense, encompasses all of the other practices of the community; indeed, we might say that it *is* the practice of the community. Clearly this is a very Aristotelean — indeed, very Thomist — comprehensive conception.<sup>110</sup>

However, I see nothing about the conception of practice that requires us to agree with MacIntyre and the Rawls of *Theory* here. I see only one general reason to arrange practices hierarchically, and that is when one practice exists more-or-less exclusively to produce tools (or other material resources) required by another. For example, there seems to be no point to publishing — no internal good of publishing, that is —

<sup>109</sup> Rawls, *Political Liberalism*, 206.

<sup>110</sup> Of course, MacIntyre’s understanding of politics enjoys significant progress relative to both Aquinas and Aristotle. For instance, it is both much more democratic and much more tolerant of those who do not share the fundamental commitments of the local community; see MacIntyre, “[Politics, Philosophy and the Common Good](#),” especially §§4-5.

except to publish the work of authors. The practice of publishing seems to be said rightly to be subordinate to the practices of writing. But each of these hierarchies will, I think, involve only a small number of practices, and I see absolutely no reason to require all of these small hierarchies to be combinable into one all-encompassing hierarchy in any but rather contingent and temporary ways. The internal goods of writing and football seem to me to be radically incommensurable, and I see no reason whatsoever to take, as a principle, either that writing is more valuable than football or vice versa. In particular cases — say, contemporary higher education — there may be reason to think that football has been overvalued, or that football has or threatens to produce institutional domination of certain forms of writing. So we may say, here-and-now-and-for-the-time-being, that (specific kinds of) writing are more valuable than (specific kinds of) football. But that falls far short of the Thomistic natural hierarchy that MacIntyre wants.

Furthermore, I am skeptical of the thought that there must be some one special practice, called “politics” or whatever else, that is responsible for even such rank ordering as seems necessary. This view seems to assume that the participants of the practices and institutions in question, as such and necessarily, are incapable of doing this. Now, it may be that we want to call any activity of considering and formulating judgments of relative importance “politics,” and take politics in this sense to be an essential part of many or all practices. But then politics, if it counts as a practice at all, is diffuse; it is not the Athenian Assembly, and it doesn’t even seem to be the *agora*. Also, we may need to settle some disagreements over relative importance (among other things) with coercive force, and it is plausible to call the collective agent responsible for this the political system.<sup>111</sup> But politics, in this sense, is clearly an institution rather than a practice.

So the political conception that I am sketching in this chapter will not take politics to be the highest or all-encompassing practice. And it will do the same with every other practice, including other practices in the basic structure. It will recognize that participation in some practices is generally part of the good life for individuals, and in this way it will recognize constitutive dependence. But it will also recognize the freedom of individuals not to participate in these practices, and so it is not a comprehensive conception.<sup>112</sup> All together, it does not rely on reasonably controversial assumptions, metaphysical or otherwise; and liberalism as such is not incompatible with constitutive dependence.

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<sup>111</sup> Compare the definition of “political” in Mark Warren, “What is Political?” *Journal of Theoretical Politics* 11, no. 2 (April 1999): 207–31, doi:[10 . 1177 / 0951692899011002004](https://doi.org/10.1177/0951692899011002004).

<sup>112</sup> See §7.7.1.



## 7.5 The basic structure

### 7.5.1 Preliminaries

A political conception of justice is not comprehensive. In one sense of “comprehensive,” this limits the subject matter of the conception: it deals only with society’s “main political and social institutions and the way they hang together as one system of cooperation,”<sup>113</sup> or what is called the *basic structure* of society, rather than all aspects of citizens’ lives. More helpfully, Rawls says that “the effects of the basic structure on citizens’ aims, aspirations, and character, as well as on their opportunities and their ability to take advantage of them, are pervasive and present from the beginning of life.”<sup>114</sup> So I will take the basic structure to be *those institutions* (in Rawls’s sense, not mine) *that have a pervasive and lifelong influence on the aims, aspirations, character, and opportunities of citizens in general*. “Pervasive” is meant to capture the synchronic notion that there is no, or almost no, part or aspect of society or group of citizens that is not significantly influenced by the basic structure; or, that eliminating such influence requires deliberate and extensive efforts. For example, the state can decide whether to strongly enforce laws criminalizing certain drugs, weakly or not enforce such laws, or not have such laws at all; and this, combined with the means of enforcement, influence the economic geography of the major cities where many citizens live. Eliminating this influence requires deliberate and extensive efforts, whether by creating a privately-policed community, by changing the methods of policing, or by changing the laws. “Lifelong” is meant to capture the diachronic notion that the effects of the influence of the basic structure apply to a given individual’s entire life, even well after the direct, particular influence has ceased. For example, I consider education part of the basic structure below; while someone may only be *directly* influenced by this practice for the first part of his or her life, the effects of this influence cascade through most aspects of their aims, aspirations, character, and opportunities for his or her entire life.

Note that, to be part of the basic structure, an institution must be *both* pervasive and lifelong. An institution that is, for example, lifelong but not pervasive will not count. In this sense, the basic structure provides the social background or framework, against with or within which other activities take place. Also, in line with the discussion of the last section, I do not claim that the institutions of the basic structure are more important than other institutions. We might say: the institutions of the basic structure (and the activities that constitute them, and the various kinds of goods that are their aims or goals) are *among* the important elements of a good human life, but do not *exhaust* these elements. Because (due to their pervasiveness and lifelong influence) citizens have only very limited opportunities to escape or “opt out” of the basic structure, it is essential for justice that the basic structure be properly

<sup>113</sup> Rawls, *Justice as Fairness*, 8-9.

<sup>114</sup> *Ibid.*, 10.

organized; for other institutions, with more limited influences over citizens' lives, this is not essential for justice. And the basic structure is not beyond criticism; indeed, as the discussion of sexism below should make clear, many institutions of the basic structure are appropriate targets of criticism.

So, in line with one of the conditions discussed in the last section, in this section I show how the conception of practice can be limited to the basic structure of society by identifying the practices and institutions (now in my sense) that make up the basic structure.

Drawing on the conception of practice, the basic structure will comprise various practices and institutions, though not all. For example, particular religious practices, since they do not have the right sort of influence over their citizens (in the paradigmatic large, diverse democratic societies with which we are dealing), will not be part of the basic structure.<sup>115</sup> Hence, at least as such, they will not be covered by the conception of justice. For lack of better terminology, call those practices and institutions within the basic structure and so covered by the conception of justice *basic practices* and *basic institutions*, respectively.

The composition of the basic structure is closely related to the conception of rational advantage, which is Rawls's term for the good or well-being of individual citizens. The goods (and bads) of citizens will be understood in terms of the internal and external goods of those practices and institutions. For example, participation in democratic governance is part of the good or well-being of individual citizens — citizens who are deprived of opportunities for participation are *ipso facto* worse off — and so having opportunities for participation is rationally advantageous for each citizen. And participation in democratic governance is also an internal goods of politics, in at least one sense. Thus there are two ways we might proceed. On the first way, we start by characterizing the basic structure, and then use that characterization to identify the relevant internal and external goods. This approach is quite similar to the approach that Rawls takes in the development of justice as fairness. For example, in *Justice as Fairness* the institutions of the basic structure are laid out first — they include, on his view, the political and judicial systems, the property system, the economy (including both the system of production and the market or other provisioning system), and the family — in §4, while the index of social primary goods — in terms of which rational advantage is measured, and including the rights that protect opportunities for democratic participation — does not appear until §17.<sup>116</sup>

Nussbaum's version of the capabilities approach takes a rather different approach. She *first* identifies the set of central capabilities — the goods — and *then* identifies the prevalent social institutions that promote or frustrate these capabilities.<sup>117</sup> While

<sup>115</sup> More on this at the end of the section.

<sup>116</sup> Rawls, *Justice as Fairness*.

<sup>117</sup> Martha Nussbaum, *Women and Human Development* (Cambridge University Press, 2000), esp. 71-2, 74, and 78-80. See Alison Jaggar, "Reasoning about Well-Being: Nussbaum's Methods of Justifying the Capabilities," *Journal of Political Philosophy* 14,

the overall aim of her approach is basically the same as Rawls's — principles of justice to guide public policy — her method of developing those principles runs, as it were, in the opposite direction.

I suggest a pluralist methodology. In some cases, it will be easiest (or otherwise better) first to identify the relevant practices and institutions, and then characterize the relevant internal goods and external goods. In other cases, it will be easiest first to identify the relevant internal goods and external goods, and then characterize the relevant practices and institutions. And, in still other cases, it may be best to go back and forth: perhaps first identifying some relevant internal goods, then the practice to which they are attached, then other internal goods and the institutions that support the practice. Once again, since we are engaged in non-ideal theory and not attempting to give either a total or complete account, there is no reason to designate one or the other level as the “foundation” for the rest of the analysis.

### 7.5.2 The state and the economy

Throughout his entire body of work, Rawls includes two institutions (in his sense) in the basic structure: the state and the economy or market. Both, on my account, combine various practices and institutions, and these should be distinguished at least conceptually.

First the state, understood broadly to include all three standard branches of government and their various bureaucracies, at various levels of organization (federal, national, local, and so on). On the one hand, the state serves as one of the major fora for public political deliberation, and questions of public policy — questions about what course of action the state should take — are typical occasions for public political deliberation in other fora. For example, legislators might debate the ethical status of a fetus and abortion, and a piece of proposed legislation concerning abortion might be an occasion for debates over abortion on cable TV or political blogs. Taking this sort of public political deliberation to be a practice, we can see the state as a practice. On the other hand, as libertarians rightly remind us again and again, the state holds a monopoly on the use of coercive force, and there is a familiar sense of “politics” on which it is nothing more than a partisan struggle for control over the state's coercive power. From this perspective, the state appears to be an institution.

I suggest that both perspectives on the state are accurate but incomplete. The state serves to promote both internal goods and external goods — it is both a practice and an institution. The role of legislators and jurists — at least, when all is working correctly — is the role of public political deliberators, and the role of the executive branch and its bureaucracy — again, when all is working correctly — is to exercise coercive power in order to carry out the projects determined by public

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no. 3 (2006): 301–22, doi:[10.1111/j.1467-9760.2006.00253.x](https://doi.org/10.1111/j.1467-9760.2006.00253.x) for a criticism of Nussbaum's apparent reliance on intuition in this methodology.

deliberation. While these two functions of the state are practically inseparable, they are still conceptually distinct.

While complicated, this approach lets us accommodate two sets of criteria for government offices that would otherwise seem to conflict. On the one hand, it seems quite appropriate to award positions within government agencies — postmaster, say — on the basis of efficiency and productivity. On the other hand, it seems quite inappropriate, if not completely inappropriate, to award political and judicial offices on the basis of such criteria. Instead, we want lawmakers and judges who are thoughtful and reflective about the basic ethical commitments of citizens generally and the role of the state in enacting those commitments. So, all together, government offices both should and should not be awarded on the basis of efficiency and productivity.

The last move requires collapsing the distinction between the practice of the state — public political deliberation, with the specific roles it has for lawmakers and judges — and its institutions — the postal service and other agencies. Bracketing concerns about the way in which large governmental institutions as such might prevent citizens in general from participating in public political deliberation,<sup>118</sup> it seems appropriate to award offices in the institutions on the basis of external goods while awarding the offices of the practice on the basis of internal goods. This conceptual distinction between the two parts of the state doesn't solve all such problems: there are the political appointees at the top of the various governmental institutions who are, as it were, the interface between the practice and the institutions. But it does narrow the set of such difficult cases.

Turn now to the economy. As with the state, it has aspects of both practices and institutions. On the one hand are a vast array of activities organized collaboratively around the production of material goods, many (though the number has decreased over the last few centuries) requiring specialized training of various kinds. In other words, the system of production seems to comprise an array of practices. On the other hand is the market, the exchange and provision of the internal goods of these practices, especially as commodities, that is, for the sake of wealth. The market seems to be the paradigm of an institution, and the relationship between the market and the system of production — commodification, in both a Marxian sense and my technical sense — is arguably the paradigm of institutional domination.

I suggest including all four of these — the practice and institutions that compose the state, and the practices and institution that compose the economy — be included in the basic structure.

### 7.5.3 Scientific inquiry

Next — and not surprisingly, in light of the first section — I take scientific inquiry to be part of the basic structure. This is especially easy to see on the broad view,

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<sup>118</sup> Young, *Justice and the Politics of Difference*, ch. 3.

when we focus on technology. Over the last 250 years, technological change has created, destroyed, or radically reconfigured every major productive activity; in short, technology is pervasive. And the massive power of technology — coupled with the pervasiveness of the market — to create or undermine broad classes of occupations has a lifelong effect on individuals' opportunities and aims. Until the twentieth century, the profession or career of electrician and electrical engineer — someone who builds and maintains electrical technology — was simply inconceivable. Similarly, in the early twentieth century, developments in communications technology have virtually done away with telephony — the design, construction, and maintenance of communications technology — as it was understood for most of the preceding century. There are basically no longer any opportunities to work with analog telephones for one's career, for example — a fact which has major implications for telephonic engineers who were trained in the 1970s and '80s.

The influence of scientific knowledge, as such, is more subtle than that of technology, but I do think the narrow view can also take scientific inquiry to have a pervasive and lifelong influence. For example, the atomic theory of matter, the past existence of currently-extinct species, plate tectonics, the germ theory of disease, the concept of a gene, and the perfect market theorem, all seem to be familiar and widely accepted by Americans, albeit only in very vague and sometimes problematic forms (as with genetics and perfect markets). The epistemic authority of science is widely recognized and accepted; the exceptions to this generalization (natural selection and climate change, for example) are quite few in number, for all that they attract a great deal of attention. Many of the implications and assumptions of these representations become deeply fixed in individuals' views of themselves, their lives, and the possibilities open to them. This is perhaps easiest to see with a simplified version of some of the assumptions of neoclassical economics: individuals act so as to maximize their rationally expected individual (egoistic) utility (hedonic pleasure). Even phrased as a very weak generalization, this claim is spectacularly empirically false. And yet many individuals who do not actually act in this way — in several respects — think of themselves as acting in this way. Indeed, I would argue that this assumption is pervasive in the contemporary US. Its influence also seems to be lifelong.

#### 7.5.4 Families

Another important family of basic practices are what I will call *affective practices*, especially families and friendships. Friendships often overlap with other practices in complex ways — I am friends with several of my fellow philosophers, and in part for that reason — so I will concentrate on families.

As generations of feminist philosophers have pointed out, human beings are born as helpless infants and almost all of us — whether from injury, illness, or infirmity — die just as helpless. Furthermore, many of us spend significant periods of our lives in a relative helpless state — a state where we need help with at least some crucial

activities. While we are in these states, we require *care* from other human beings. And, historically, the work of providing care has been done by relatives, especially female relatives. As Sara Ruddick points out, care work — specifically, the kind of care of children that is done by mothers — is more than just a matter of ensuring the helpless individual continues to survive, which she calls *preservation*. Beyond this, it also involves *growth* and *social acceptability*.<sup>119</sup> That is, second, it involves cultivating and directing the developing capabilities of a child, such as by teaching the child the language of her people, then teaching her to read that language, then understanding and repairing machines or preparing food or tending the herd or mending clothes. And, third, it involves “shaping their children’s growth in ‘acceptable’ ways,”<sup>120</sup> so that the children will flourish in and contribute to their society as adults.

Ruddick goes on to argue that the work of mothers requires periodic reflection on not just the most effective means to realize the three goals — whether to always comfort a crying child, so he feels loved, or let him cry himself to sleep, so he learn to be independent — but also on the goals themselves. Does social acceptability require teaching a child to conform to established sexual and gender roles? If the mother and children are members of an oppressed minority, does it require teaching the child to defer to their racial or ethnic “superiors?”<sup>121</sup> Thus motherhood requires what Ruddick calls maternal thinking, and one of the products of maternal thinking is progress — an improved understanding of the goals of motherhood and how to realize them.

At this point, it should be obvious that motherhood — and, by extension, other forms of care work, including caring for companion animals and caring for elderly people — is a practice.<sup>122</sup> The goods of motherhood can only be realized by mothers, in transaction with their children;<sup>123</sup> more generally, the goods of care work can only be realized by caregivers, in transaction with their patients. It should also be obvious that the work of mothers, perhaps more than many other affective practices, has a deep influence on our aims and opportunities. Many of us learn our most fundamental commitments from other mothers. And often our obligations to care for members of our families — such as our obligations to mother our children — shape our opportunities to pursue our other aims. Thus I follow many other feminist commentators on Rawls by including families in the basic structure of society.<sup>124</sup>

<sup>119</sup> Sara Ruddick, *Maternal Thinking* (Boston: Beacon Press, 1995), 18ff, ISBN: 0807014095.

<sup>120</sup> *Ibid.*, 21.

<sup>121</sup> Compare Alison Bailey, “Mothering, Diversity, and Peace Politics,” *Hypatia* 9, no. 2 (Spring 1994): 188–98.

<sup>122</sup> Indeed, cf. Ruddick, *Maternal Thinking*, 15ff.

<sup>123</sup> Note that Ruddick explicitly allows that men can be mothers in her sense; *ibid.*, 17.

<sup>124</sup> Note that my argument is somewhat different from standard arguments for this conclusion; contrast Moller Okin, *Justice, Gender, and the Family*, ch. 5; Nussbaum, *Women and Human Development*, ch. 1; Ruth Abbey, “Back toward a Comprehensive Liberalism?: Justice as Fairness, Gender, and Families,” *Political Theory* 35, no. 5 (February 2007): 5–28.

### 7.5.5 Statism

There is a standard liberal worry about including families in the basic structure, and thus subject to the principles of justice: it seems to require that the state (here referring to the institution, not the practice) constantly interfere in the internal organization and operation of families, violating their privacy. Shouldn't individuals be free to organize their families however they see fit?<sup>125</sup>

However, this worry requires a significant assumption: *the primary agent of justice is the state*, that is, the agent responsible, in the first instance, for realizing justice or rectifying injustice is the state. I call this assumption *statism*.<sup>126</sup> While justice may require something of individual agents — that they comply with the duly-enacted laws established by the government, for example — theirs is only a *secondary* responsibility. In the context of non-ideal theory, the presumptive solution to all present injustices is some state action or another.

This does not mean that the best or ultimately endorsed solution is always some state action or another. Consider, for example, Rawls's discussion of the "institutions" — that is, the political-economic systems of the basic structure — that would (recall that this is ideal theory) best realize his two principles.<sup>127</sup> Rawls identifies five candidates, in the following order:

- (1) laissez-faire capitalism,
- (2) welfare-state capitalism,
- (3) state socialism with a command economy,
- (4) property-owning democracy, and
- (5) liberal (democratic) socialism.<sup>128</sup>

<sup>125</sup> I believe that this considered judgment explains Rawls's contortions in response to Okin. For example, in *Justice as Fairness* and explicitly in response to Okin, he first includes "the" family in the basic structure. He then acknowledges that "the principles of political justice are to apply directly to this [viz, the basic] structure." But immediately he adds that "[the principles] are not to apply directly to the internal life of the many associations within [the basic structure], the family among them." *Justice as Fairness*, 163. He then tries to explicate this paradoxical point *about part of the basic structure* with a discussion of *ecclesiastical governance* and by recalling the distinction between "the point of view of people as citizens" and "their point of view as members of families and of other associations." (164-5)

<sup>126</sup> By coincidence or convergence, Justin Weinberg, "Norms and the Agency of Justice," *Analyse und Kritik* (February 2009): 319–38, gives exactly the same definition of "statism" and also identifies it as a significant and overlooked assumption.

<sup>127</sup> Rawls, *Justice as Fairness*, part IV.

<sup>128</sup> *Ibid.*, 136.



Laissez-faire is dismissed on the grounds that fails to secure fair value of the political liberties and fair equality of opportunity, and has “a rather low social minimum.”<sup>129</sup> State socialism goes too far in the other direction, realizing perhaps even a robust version of the difference principle, but by doing away with the basic rights and liberties all but entirely.<sup>130</sup> The welfare state does better — it ensures a decent minimum through unemployment compensation and the redistribution of income — but it “may develop a discouraged and depressing underclass many of whose members are chronically dependent on welfare” and thereby lack fair value of the political liberties, fair equality of opportunity, and the social bases of self-respect.<sup>131</sup> In Rawls’s discussion, the first possibility presents the injustices of unrestricted capitalism; the second and third consider state-based solutions to these injustices, but find them wanting. That is, Rawls’s first reaction to the problems created by laissez-faire is to have the state take economic action. These state-based solutions are only rejected when they either do not solve the problem (as in welfare-state capitalism) or create other problems (as in state socialism).<sup>132</sup>

To deny statism is not to affirm that the state does or should play no role in realizing justice or rectifying injustice. It is not even to deny that the state is *among* the primary agents of justice. Rather, it is simply to deny that the state is *the* primary agent of justice. Other agents are (also) responsible, in the first instance, for realizing justice or rectifying injustice. For example, the feminist movement (and its individual practitioners) is, I suggest, responsible, in the first instance, for rectifying injustice against women, including the patriarchal division of labor within families that renders wives economically dependent on their husbands.<sup>133</sup> Feminism has done a great deal to promote more egalitarian gender roles in families over the last several decades. In part this is due to changes to the law, such as changes to divorce law and alimony and child support. But it is also due in part — and perhaps an even greater part — to “cultural” challenges to the assumptions of patriarchal gender roles, without the use of the coercive power of the state. I might even go so far as to suggest that, in general, the state has typically been rather late on the scene in addressing injustices, and has usually only done so in response to ethical and political practices (if at all): abolitionism (at least arguably), the women’s movement (both prior to

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<sup>129</sup> [Ibid.](#), 137.

<sup>130</sup> [Ibid.](#), 138.

<sup>131</sup> [Ibid.](#), 139; 149.

<sup>132</sup> Rawls himself does not say much about property-owning democracy and liberal socialism. Samuel Freeman, *Rawls* (London and New York: Routledge, 2007), 226-31 offers eleven features that distinguish property-owning democracy from welfare-state capitalism. Seven of these concern actions taken by the state in order to better realize the difference principle, and the state is the only agent identified as acting towards this aim.

<sup>133</sup> See also the discussion of sexism as an institution in the next subsection. As in that discussion, this paragraph oversimplifies the situation in the contemporary US.

1920 and after 1963), the labor and welfare movements, the anti-war movement, the civil rights movement, environmentalism, and the LGBTQ movement.<sup>134</sup>

In short, practices are among the primary agents of justice, and not only on the practical grounds that giving them this responsibility means we needn't wait around for the state. If we have a conception of justice on which justice involves both (a) realizing some internal goods and (b) limiting the use of the coercive power of the state, then we have more principled reasons for assigning some practices the responsibility of realizing these internal goods. And, on the other hand, practices and institutions other than the state are among the primary agents of injustice. Epidemiological inquiry into the carcinogenic effects of tobacco that was stymied and obfuscated by the tobacco industry for the sake of avoiding costly lawsuits and regulation suffered institutional domination not (directly) because of any actions on the part of the state, but (directly) because of the actions of the tobacco industry. More generally, we cannot give an adequate account of commercialization so long as we assume statism.

### 7.5.6 Sexism, racism, and other institutions

Again, I do not claim that the sketch I am giving in this chapter — even if it were filled in — is adequate for all issues that we might reasonably take to be issues of justice. It is hard to see, for example, how it could be used to support an account of exploitation, or the cultural complexities associated with deep poverty and systemic racism and sexism. But some of the features of these injustices can be articulated in my terms: by viewing them as institutions, either parts of the institutions of the market or the state or as distinct institutions that are pervasive in their own ways and so interact with the market and the state in complex ways. I shall sketch this picture for sexism; I think the first lines of the picture for racism, heterosexism, economic exploitation, and many other “class” systems, will be quite similar.

In terms of external goods, the “goods” of the institution of sexism can be stated quite simply: generally and for the most part, it provisions men with greater wealth and power (especially power over women) than women. Further, it glorifies — provisions *kudos* to — men and women who satisfy norms of gendered and sexual behavior that reinforce the gendered inequality of wealth and power, and threatens those who do not satisfy these norms (and thereby at least indirectly challenge the gendered inequality of wealth and power) with violence. This much has said by many feminists.<sup>135</sup>

<sup>134</sup> The line of thought of the last few paragraphs is inspired in large part by the discussion of “insurgency” in Young, *Justice and the Politics of Difference*, ch. 3.

<sup>135</sup> See, among many others, Barbara Ehrenreich, “What is Socialist Feminism?” *Women's International Network News* (June 3, 1976), <http://marxists.org/subject/women/authors/ehrenreich-barbara/socialist-feminism.htm>; Adrienne Rich, “Compulsory Heterosexuality and Lesbian Existence,” *Journal of Women's History* 15, no. 3

The basic account of institutional domination can be used to fill in further details of the feminist picture — and, indeed, some points from gender studies can produce some insights for thinking about the relationship between practices and institutions.<sup>136</sup> Sexism, as an institution, can deprive women of opportunities to participate in practices, such as fine art and scientific inquiry.<sup>137</sup> At least with respect to practices — such as scientific inquiry — that are part of the basic structure, women suffer by being excluded from these practices in a way that is relevant for justice. In short, the lack of opportunity for women to engage in scientific inquiry is an injustice, and one produced by the institution of sexism. Sexism produces injustice.

This injustice is produced *not just* by virtue of the unjust distribution of external goods that it creates, but also by virtue of the unjust distribution of *internal* goods.<sup>138</sup> Further, it is reasonable to expect that the influence of sexism on scientific inquiry will lead to representational knowledge that reinforces sexism — for example, that reinforces the ideology that women are irrational and irresponsible, and so require

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(Autumn 2003): 11–48, doi:[10.1353/jowh.2003.0079](https://doi.org/10.1353/jowh.2003.0079); Jaggar, *Feminist Politics and Human Nature*; MacKinnon, *Toward a Feminist Theory of the State*; and Patricia Hill Collins, *Black Feminist Thought*, second edition (New York and London: Routledge, 2000), ISBN: 0415924847.

<sup>136</sup> The following is exaggerated in certain respects for the contemporary US. However, it is certainly entirely accurate for the US just a few decades ago, and is entirely accurate for many other societies today. For example, today women are not explicitly and deliberately excluded from scientific inquiry in the US in the ways that they were fifty years ago, though arguably the career structure of scientific inquiry and the persistent tradition of the unequal gendered division of care work effectively limit opportunities today. Since I am just offering a basic sketch here, I wish to avoid working through the contemporary complexities; they would, however, be essential for developing this sketch further.

<sup>137</sup> For art, see Linda Nochlin, “Why Have There Been No Great Women Artists?” Chap. 7 in *Women, Art, and Power* (Westview Press, 1988), 145–78, ISBN: 0064301834; Linda Nochlin, “Why Have There Been No Great Women Artists? Thirty Years after,” in *Women Artists at the Millennium*, ed. Carol Armstrong and Catherine de Zegher (Cambridge, MA: MIT Press, 2006), 21–32, ISBN: 9780262012263. For scientific inquiry, see the discussion of feminist transactionism, §5.2.

<sup>138</sup> Note that sexism also effectively deprives *men* of opportunities to participate in certain practices, such as cooking, knitting and sewing and other forms of clothing-construction, and “mothering” or care work. Ruddick, *Maternal Thinking*. It also, by eroticizing sexual violence, seriously disrupts the practices of egalitarian romantic relationships, thereby generally depriving both men and women of the opportunity to participate in this practice. MacKinnon, *Toward a Feminist Theory of the State*, ch. 11. So this account can capture the feminist insight that “patriarchy harms men, too.” It also suggests an explanation of how this is compatible with the claim that women suffer more than men under sexism: men’s practices are taken to be more valuable than women’s practices, especially in contemporary sexist societies, and consequently men generally enjoy greater wealth, power, and status than women.

fatherly and husbandly control.<sup>139</sup> Since this involves either persistently sacrificing the epistemic quality of the representations in question or fitting the standards for epistemic quality themselves to the needs of sexism, this is a form of institutional domination: the internal goods of the practice (excellent representations) have been sacrificed for the sake of the institution (sexism). Consequently, sexism *also* prevents *men* practitioners from realizing internal goods: scientists are led to produce representations that act as apologies for sexism rather than embody epistemic excellence.

One early insight of gender studies is that gender is “omnirelevant”: gender is not its own distinct category of behavior, confined to a narrow class of social situations, but rather potentially a feature of any situation and any behavior.<sup>140</sup> At any time or place, we can be praised — receive *kudos* — for complying with standards of gendered behavior, or blamed — deprived of *kudos* or even threatened with violence — for failing to comply. But this is to say that it has a pervasive and inescapable influence on individuals’ opportunities throughout their entire lives. In my terminology, we can somewhat capture this fact by saying that the gender system — and the institution of sexism, as one of its major components — is part of the basic structure. More strongly, I suggest that sexism is an institution on a par with the market and the state. *Any* practice may potentially receive some benefits — a greater provision of external goods — by “working with” sexism, and any practice may potentially be threatened with institutional domination by sexism.<sup>141</sup>

Unlike the market and the state, however, it is quite implausible to claim that sexism has any redeeming value whatsoever. Over the past century, feminist and

<sup>139</sup> This use of “ideology” follows Young, *Justice and the Politics of Difference*, 74.

<sup>140</sup> Candace West and Don Zimmerman, “Doing Gender,” *Gender and Society* 1, no. 2 (June 1987): 125–51, <http://www.jstor.org/stable/189945>.

<sup>141</sup> Feminist practice might be defined as the practice whose primary internal good is the destruction of the institution of sexism. It might therefore seem impossible for feminist practice to be threatened with institutional domination by sexism, for this would require that (seemingly impossibly) feminist practice “work with” precisely the institution that it is trying to destroy. But consider Christina Hoff Sommers, *Who Stole Feminism?* (New York: Touchstone, 1994), ISBN: 0684801566. Sommers distinguishes “equity feminism” — which she supports — from “gender feminism” — which she criticizes and identifies as “the prevailing ideology among contemporary feminist philosophers and leaders.” (22) Because of her support for “equity feminism,” Sommers is arguably a feminist. Because of her criticism of “gender feminism,” she is a resident scholar at the America Enterprise Institute, a conservative or libertarian think-tank and has served on the national advisory board of the Independent Women’s Forum, also a conservative or libertarian think-tank. Arguably, then, her work has been supported by institutions of sexism — Sommers, a feminist, has chosen to “work with” sexism. I take it that this argument, even if unsound, gives reason to think that it is *possible* for feminist practice to “work with” sexism. But then it is not impossible for feminist practice to be threatened by institutional domination from sexism. Let me stress that I am *not* claiming here that Sommers’s work *actually* suffers institutional domination.

GLBTQ practices have done a great deal to dismantle this institution — it is reasonable to say that such is their primary internal good — but it is far from completely eliminated.

### 7.5.7 Education and an open problem

The educational system is an interesting test case. Grant for the moment that primary and secondary education are practices.<sup>142</sup> Young people’s access to the education system — and the quality of the education they receive therefrom — does, of course, have exactly the sort of pervasive and lifelong influence that would qualify it for the basic structure under Rawls’s methodology. But many primary and secondary schools operate under the auspices of a comprehensive conception — for example, Roman Catholic parochial schools. Including the educational system in the basic structure would seem to require us to either make an arbitrary (with respect to education) distinction between secular and religious schools or inappropriately (because of their limited influence on citizens as a whole or for reasons of religious freedom) include religious organizations in the basic structure.

This seeming paradox involves a fallacy of composition. Parochial schools are one *part* of the Roman Catholic Church. They are also part of the educational system, and for that reason are part of the basic structure of our society. Other parts of the Roman Catholic Church — say, the Magisterium — are not part of the basic structure.

This is not to say that difficult issues — even deep dilemmas — do not result from including parochial schools in the basic structure. For example, suppose considerations of sexual well-being imply that the educational system has an obligation to provide all secondary students with comprehensive sex education, that is, education on the proper use of artificial contraceptives. This obligation would conflict with the comprehensive conceptions of the more orthodox members of the faculty at Roman Catholic parochial schools, who believe that the use of artificial contraceptives is deeply unethical and thus should not be taught in their schools. On the one hand, allowing parochial schools to not teach their students comprehensive sex education would appear to violate their obligations as part of the basic structure. On the other hand, requiring parochial schools to do this would appear to violate the freedom of conscience of the teachers. Such dilemmas, however, do not provide an argument against including parochial schools in the basic structure without the (I think, manifestly implausible) premise that there are no real ethical dilemmas.

### 7.5.8 What’s left?

At this point, it might seem that the basic structure could include pretty much any practice or institution. So it is worthwhile to consider one family of practices that, I

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<sup>142</sup> The status of teaching as a practice was briefly discussed in chapter 2.

claim, do not qualify for inclusion in the basic structure, at least for the contemporary United States: religion.

Recall first our definition of the basic structure: those practices and institutions that have both a pervasive and lifelong influence on citizens' aims, aspirations, character, and activities. A practice that has one — even to a great extent — but not the other is, for that reason, not a basic practice. It does not shape the lives of citizens to the extent that the basic structure does.

Undoubtedly religious practices have a lifelong influence on citizens who participate in them, even only children who are “raised in the church” but leave it as or after becoming adults. For example, it is absolutely clear that my deep-seated tolerance, egalitarianism, pluralism, syncretism, irenism, and so on, are due in significant part to being raised in a low church Episcopalianism that was, theologically, quite close to the United Church of Christ or, in some respects, even Unitarian Universalism. While my daily life retains absolutely no spiritual commitments or practices in any way close to the standard sense — unless one counts singing Christmas carols three weeks out of the year — the religious practices of my childhood certainly have a lifelong influence on my aims, aspirations, and character.

However, it is not clear to what extent religion has this sort of lifelong influence on any individual's opportunities. Many forms of religious discrimination in US society seem to have been eliminated, even *de facto*, and practitioners of various kinds of atheism, agnosticism, Christianity, Judaism, and Buddhism all seem to enjoy roughly the same array of opportunities, as such. A relatively small worry about this sweeping claim is the opportunities that LGBTQ people and women enjoy (or not) within socially conservative contexts compared to socially liberal contexts: by virtue of the views of their co-religionists, LGBTQ individuals have different opportunities to form families, and women have different opportunities to pursue leadership vocations within the church or even to have non-domestic careers. A relatively large worry is continued discrimination against Muslims and Sikhs, both *de jure* and *de facto* and both with respect to their religious practices (such as prohibitions on certain kinds of dress or carrying ritual weapons) and with respect to other, non-religious opportunities (such as employment discrimination).

Still, even if one maintains that religion does have a lifelong influence on individuals' opportunities, it does not seem to have a pervasive influence. The practices that constitute contemporary Roman Catholicism, as such, have very little influence on contemporary Protestantism; both have still less influence on contemporary Buddhism; and none of these seems to have much influence on, say, the market, the state, the system of public education as a whole, scientific inquiry, racism, professional sports, pop music, and on and on. This is not to say that these religious practices have *no* influence — religious pop music is certainly a financially robust corner of pop music, and certainly some theological commitments are relevant in debates over issues such as abortion and abstinence-only sex-ed. But these influences are limited and often highly attenuated. In many of our interactions with our fellow citizens,

our religious practices are irrelevant. By contrast, as discussed above, our genders, sexual identities, economic status, race and ethnicity, are omnirelevant — it is always possible for these institutions to exercise some influence, in any situation whatsoever. Similarly, the use of technology has pervaded virtually all aspects of our lives, and it is only through great effort that we can “unplug” and limit the influence of scientific inquiry. Religious practices, for all of their deep lifelong influences on the lives of their practitioners (current and former), are not pervasive in the way basic practices are.

The argument of the last paragraph needs one important qualification: I am speaking of the contemporary United States, and on the national level. First, for much of its history, Calvinist, Lutheran, and charismatic versions of Protestantism were pervasive in the United States, and consequently Catholics and Jews suffered severely limited opportunities (to put the point too mildly). Up until a few decades ago, a particular family of religious practice was pervasive, and so would have counted as a basic practice. Second, other liberal democracies still have a state religion or a culturally-dominant religious practice, and this practice is indeed pervasive. For example, one might argue that a sort of Christian-liberal semi-religious or quasi-religious comprehensive conception is pervasive in France and Switzerland, and that this was behind the campaigns in those countries against the niqab (veil) and minarets; these campaigns, insofar as they were successful, limited the religious practices of some Muslims. And, third, on a smaller geographical scale, religious practices may indeed be pervasive, even within the contemporary US. Examples include the Church of Latter Day Saints (LDS, or the Mormons) in much of the desert West; Catholicism in certain cities, neighborhoods, and small towns throughout the Northeast, Midwest, and Southwest; Lutheranism in Minnesota; and so on. As these examples indicate, the pervasiveness of one particular religious practice can but need not imply a significant restriction of opportunities for practitioners of other religions. But it will still have some influence — it can be difficult to watch certain movies, read certain books, or drink certain beverages in a predominantly-Mormon town in Utah, for example.

As these qualifications suggest, the precise composition of the basic structure may turn out differently on different analyses that respond to different problems or issues. And this is in line with non-ideal theory: I am not attempt to give a complete and all-inclusive account of the basic structure, but rather develop a tool for social analysis.

This example shows that non-basic practices and institutions are not unimportant or uninfluential. Certainly many individuals take their religious practices to be the most important aspect of their lives. But, at least when we are talking about the contemporary US on a national level, it is not so influential that it serves as part of the background against which all of our other activities take place.



## 7.6 Overlapping consensus and recognition

As a political conception, the conception of justice that I am sketching in this chapter aims to be the object of an overlapping consensus. In the strongest sense, this means that the political conception is or can be affirmed by all reasonable citizens.<sup>143</sup> However, I think this strongest sense is too ambitious, even for Rawls's conception of justice as fairness. Rawls's conception is contractarian, and I do not think all reasonable citizens will affirm a contractarian account, even one that is "simply laid out" as a "procedural device of representation."<sup>144</sup> But I do think that they can accept that contractarianism is *one among several reasonable ways* of articulating, organizing, and presenting the content of political liberalism.

I think it is worthwhile to spend a paragraph unpacking that last thought. As a philosophical methodology, political liberalism aims at reflective equilibrium rather than justification.<sup>145</sup> It takes up certain concepts from what Rawls calls the public political culture — concepts such as freedom, equality, and fairness — offers a more substantive conception of these concepts (articulating them), identifies and stabilizes relations among these concepts (organizing them), and in doing so makes them operationalizable (presenting them). The overall aim is to show how these concepts work together — and identify implicit tensions between them — as the internal goods of the practice of public political culture. As Rawls recognizes and as discussed above, there are many different ways of doing these three things while remaining more-or-less faithful to the ordinary use of these concepts in the public political culture. The contractarian approach of justice as fairness is one way; the more Aristotelean capabilities approach is another; and the conception of practice aims to be a third.

Now, each of these several versions of political liberalism is, of necessity, selective. The concepts that each takes up from the public political culture — from ordinary use — are rich, complex, and not very coherent. And the articulation, organization, and presentation of each version responds to the needs of a different group of citizens. For example, Rawls's justice as fairness is especially appropriate for the needs of policymakers and the judicial system during the tumultuous social changes in the US of the 1960s and '70s and the first skirmishes of the "culture war" during the late '70s and '80s. The capabilities approach, by contrast, seems to be a response to the needs of governments and international NGOs working on development and poverty alleviation in the 1980s through today. And the conception of practice, at least as I have developed it, is a response to the needs of communities of practitioners — sci-

<sup>143</sup> Rawls, *Political Liberalism*, xix.

<sup>144</sup> *Ibid.*, 103.

<sup>145</sup> Obviously, the account of reflective equilibrium that I give in the next several sentences goes quite far beyond Rawls' original understanding of the concept. This development is motivated by the move to non-ideal theory and my emphasis on concrete activity rather than abstract theory-building. Contrast Rawls, *A Theory of Justice*, 18-19, among others.

entists, insurgency social movements, and others — struggling with the domineering institutions of the market and state.

I further assume that, if a given political conception does not respond to the needs of a given set of citizens, it is not liable to be accepted by these citizens.<sup>146</sup> But these citizens can (under at least some conditions) recognize that the political conception is a good faith effort to develop the same original concepts in response to the needs of some other citizens. In this way, different political conceptions are complementary rather than rivals and recognized as such by different groups of citizens. This suggests a weaker sense of overlapping consensus: a political conception aspires not even to universal acceptance, but to recognition as one member of the family of complementary political conceptions.

Our non-ideal theory approach and the recognition claim complicate things further. As I said in the discussion of reasonableness in §7.3, on a non-ideal approach it makes more sense to start from the fact of unreasonable disagreement and misunderstanding, and the recognition claim causes a specific kind of misunderstanding. While we cannot expect to ever completely eliminate this misunderstanding, we can treat overcoming it as an ongoing project. The objection that I present in this section looks more closely at the social preconditions for this ongoing project, arguing that, in principle, the conception of practice cannot be the object of an overlapping consensus, even in the sense of being recognized as one member of the family of complementary political conceptions.

Recall the argument against the strong version of the recognition claim discussed in §2.6. Roughly, the argument points out that MacIntyre's recognition claim (with a few other assumptions) requires that all members of the community participate in every practice. In that discussion, I invalidated the argument by introducing the notion of joint practice and replacing MacIntyre's strong recognition claim with what I called the weak recognition claim: An individual  $a$  can give or accept an intrinsic justification (a justification in terms of internal goods) of practice  $A$  only if there is an individual  $b$  such that there is a practice  $B$  of which both  $a$  and  $b$  are practitioners and  $b$ 's participation in both  $A$  and  $B$  provides some basis for the intrinsic justification of  $A$  to  $a$ . I will abbreviate the consequent by the phrase *there is a justifying joint practitioner*; that is, the weak recognition claim says that an individual can give or accept an intrinsic justification only if there is a justifying joint practitioner.

That argument can be modified to claim that even the weak recognition claim prevents the conception of practices from being the object of an overlapping consensus. Consider the following:

- (OC-1) The conception of practice can be the object of an overlapping consensus (even in the weak sense) only if all reasonable citizens can accept an intrinsic justifi-

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<sup>146</sup> I take it that Rawls is speaking synecdochically when he talks about comprehensive conceptions as such accepting a political conception. Strictly speaking, it is the individuals who hold the comprehensive conception who do the accepting.

cation of all basic practices.

- (OC-2) A citizen can accept an intrinsic justification of a practice only if there is a justifying joint practitioner for that practice.
- (OC-3) ∴ The conception of practice can be the object of an overlapping consensus only if, for all reasonable citizens and basic practices, there is a justifying joint practitioner. (OC-1, -2)
- (OC-4) In a large, complex, diverse society, for some reasonable citizens and basic practices, there is no justifying joint practitioner.
- (OC-5) ∴ In a large, complex, diverse society, the conception of practice cannot be the object of an overlapping consensus. (OC-3, -4)

I accept (OC-1) and (OC-2) based on the weaker conception of an overlapping consensus and the the weak recognition claim, respectively. Hence, if (OC-4) is true, I take the argument to be sound.

The objector might support (OC-4) by pointing to widespread scientific illiteracy: many, if not most, Americans are utterly unfamiliar with representational knowledge that scientists take to have long since been well-established, in basically any field of scientific inquiry, and they are still less familiar with the methods by which this representational knowledge is produced. It is this lack of familiarity with scientific inquiry that is exploited, for example, when excellent representational knowledge of the current and future state of the climate is publicly challenged by appealing to infallibilist standards of evidence under the guise of “sound science.”<sup>147</sup> The objector might claim that the lack of a justifying joint practitioner (who could also explain the current state of climate science) is the best explanation for citizens’ scientific illiteracy. Then, abductively, widespread scientific illiteracy would provide a reason to believe these instances of (OC-4).

While this line of thought might be plausible initially, it is difficult to develop rigorously. General ignorance of scientific inquiry is not the same as not having an intrinsic justification of the internal goods of scientific inquiry, and neither seems to be a good explanation for the interminability of the climate change dispute. Public opinion polling by Gallup indicates that, since at least 1989, a majority of Americans have been worried at least a fair amount about global warming.<sup>148</sup> Furthermore, as of March 2012, the level of concern was roughly the same across four educational levels

<sup>147</sup> Daniel Sarewitz, “How Science Makes Environmental Controversies Worse,” *Environmental Science and Policy* 7, no. 5 (2004): 385–403, doi:10.1016/j.envsci.2004.06.001; Douglas, *Science, Policy, and the Value-Free Ideal*, 13ff

<sup>148</sup> “Americans’ Worries About Global Warming Up Slightly,” The Gallup Organization, March 30, 2012, <http://www.gallup.com/poll/153653/Americans-Worries-Global-Warming-Slightly.aspx> (accessed March 30, 2012). I thank Mark Brown for reminding me of these data.

(postgraduate, college graduate, some college, and high school or less). If scientific illiteracy were responsible, we would expect concern to be unequal across these levels. By contrast, political affiliation or identity — combined with a profoundly antagonistic political culture — seems to be a much better explanation: while thirty-four percent of Republicans and thirty-seven percent of conservatives are concerned at least a fair amount about climate change, the corresponding numbers are seventy-four percent for Democrats and seventy-two percent for liberals.

I also have an argument against (OC-4). Scientific inquiry, as a basic practice, is part of the basic structure. Basic practices, as such, have a pervasive and widespread effect on citizens' beliefs, preferences, and opportunities. Among many other ways, scientific inquiry has and continues to produce technology and practical knowledge that dramatically change numerous aspects of our lives. This technology requires people to produce, utilize, and maintain it — applied scientists, engineers, and mechanics. These people are all scientists, at least on the broad view. While very few Americans are liable to know, personally, a particle physicist or molecular biologist, even those in impoverished communities are still liable to know, personally, nurses and doctors, engineers, and high school science teachers. All of these individuals are scientists, especially on the broad view, and are in a good position to act as joint practitioners and thereby be justifying joint practitioners.

Similar considerations give reason to think that (OC-4) is false more generally. By definition, the basic structure is pervasive and inescapable; we all spend much of our lives working within it. If anything is to be in a position to have a justifying joint practitioner, it is a basic practice. I suggest that the burden of proof should be placed on the objector to give us convincing reasons in support of (OC-4); with respect to practices in the basic structure, it is highly implausible on its face.

One response to the argument of the last two paragraphs is to claim that, while citizens have a justifying joint practitioner, the work of the joint practitioner does not effectively provide their fellow citizens the needed justification. Consider the example of science teachers. At least stereotypically, the work of such teachers is, primarily, having students memorize terminology and formulae and conduct pre-scripted laboratory “experiments.” This does absolutely nothing to familiarize students with the work of actual scientists, or the standards by which they evaluate representational knowledge, practical knowledge, or technology. So, in their science teachers, citizens have potential justifying joint practitioners, but this potential is not realized.

I suggest that this response be treated as a diagnosis of a contingent problem rather than an in-principle objection. Why do science teachers use the methods that they do, rather than methods that would familiarize students with actual scientific practice? What alternative methods would be more effective in this sense? These are questions best answered by empirical investigation, and the answers given by this investigation hopefully will suggest ways to improve science education, such that science teachers can act as effective justifying joint practitioners. Furthermore, we can hope that science teachers as joint practitioners will lead the effort in education

reform.

More generally, the interconnection among basic practices provided by joint practitioners should be seen as an ongoing achievement to be realized by deliberate social action, and indeed as a part of the aims (the internal goods) of the work of exactly these joint practitioners. The mutual understanding and respect that this produces — piecemeal and fallibly — will be our non-ideal conception of an overlapping consensus, and the object of this consensus — the political conception itself — is developed gradually through these interactions between practices.

We might put the point in terms of Rawls's idea of public reason — the common language in terms of which the political conception is stated and public deliberation is carried out.<sup>149</sup> Rawls seems to treat public reason as a given resource: this language or common understanding is already available to us, prior to laying out the political conception, and so an overlapping consensus is simply a matter of making claims in this language. Clearly this is ideal. But, on the non-ideal conception that I am sketching here, we do not have a given common understanding; indeed, we often fail to understand each other. One significant cause of this is the lack of joint practitioners. So an overlapping consensus is an ongoing achievement, the product of concrete social action, not just a matter of abstract philosophical assertion.

## 7.7 Freedom, equality, and fairness

Much of this chapter has ended up devoted to, in a sense, preliminaries: other than the discussion of the basic structure in §7.5 and the claim that institutional domination should end up counting as an injustice, I have said very little about the *content* of the political conception that I am developing. Until the ground covered in the preceding sections was cleared, it was hard to see how liberalism and the conception of practice could even possibly be combined. But now that ground has been cleared, and we can start the substantive development of the conception.

Recall that, according to the criteria for the development of a political conception presented at the end of §7.3, the political conception operationalizes three underlying ideas, commitments, or concepts, namely, to citizens as (a) free and (b) equal persons and to society as a (c) fair system of cooperation over time. These concepts, specified in various ways, are central features within the history of the liberal tradition, and so I take it that a political conception counts as liberal insofar as it incorporates these three concepts. The corresponding objection, then, is that the conception of practice is too deeply illiberal or non-liberal to incorporate these three concepts. So, in response, in each of the next three subsections, I first examine Rawls's development of his conception of one of freedom, equality, and fairness, then use this as an inspiration (whether positive or negative) for my own conception.

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<sup>149</sup> Rawls, *Political Liberalism*, lecture VI and part 4.

### 7.7.1 Freedom

As a political conception, Rawls's conception of justice as fairness cannot rely on reasonably controversial metaphysical or ethical claims, such as the metaphysical claim that human beings have free will or the ethical claim that the highest or unconditional good is the autonomous will. In particular, it cannot understand freedom in terms of the Kantian interpretation of the original position as a procedural version of the categorical imperative.<sup>150</sup>

Instead, citizens are free, as it were, in their status as citizens and with respect to the basic structure of society. Rawls specifies this in three ways:

- (1) "they conceive of themselves and of one another as having the moral power to have a conception of the good," that is, *as citizens*, though not necessarily as, say, adherents of a particular religious faith, since they are "independent from and not identified with any particular" comprehensive conception,<sup>151</sup>
- (2) "they regard themselves as self-authenticating sources of valid claims" against the basic structure of society, rather than as members of a subset of society, adherents of a particular religious faith, or dependents of other citizens;<sup>152</sup> and finally
- (3) they are "capable of taking responsibility for their ends," that is, "citizens are thought to be capable of adjusting their aims and aspirations in the light of what they can reasonably expect to provide for."<sup>153</sup>

The idea behind the first two, I think, is that citizens have standing to make claims against each other and the basic structure as individuals rather than members of aggregations or some collective agent. A married Catholic woman, for example, is not valued as a citizen by virtue of being (or insofar as she is) Catholic, a wife, a mother, or any other interpersonal relations, *except* those of the basic structure itself. When she makes claims of justice against the state, she can make these claims straightforwardly as a citizen.

So these two senses of freedom seem to embody a commitment to *normative individualism*. At a minimum, normative individualism involves prioritizing the interests of individual agents, as such, over the interests of collective agents and aggregations, as such. This does not mean that no consideration should be given to the interests of collective agents. It does mean that, generally and for the most part, the interests of collective agents do not outweigh the interests of individual agents.<sup>154</sup> I will take

<sup>150</sup> Rawls, *A Theory of Justice*, §40.

<sup>151</sup> Rawls, *Political Liberalism*, 30.

<sup>152</sup> *Ibid.*, 32.

<sup>153</sup> *Ibid.*, 33-4.

<sup>154</sup> Martha Nussbaum and Elizabeth Anderson both appeal to normative individualism in their responses to non-liberal feminists. See Nussbaum, "The Feminist Critique of

up the question of whether normative individualism is compatible with constitutive dependence below.

The third way in which Rawls characterizes citizens' freedom is closely connected to the conception of fairness: citizens are entitled to their fair share of the benefits of society, but no more than their fair share, and they must also shoulder their fair share of the burdens. In terms of freedom, we might say that citizens are expected to exercise positive freedom, that is, control over their desires and actions. They are not, as it were, slaves of their passions, at least with respect to their responsibilities to other citizens.

The conception of freedom for the conception of practice will be rather more intricate than Rawls's. I want to retain the two elements that I have identified in Rawls in the last few pages, namely, normative individualism and a connection with fairness by way of some sense of positive freedom. But I also want to incorporate a notion of autonomy, operating at the level of both individuals and practices, and I want to do this without falling into a comprehensive conception. I also want to incorporate an explicit commitment to negative freedom, in the form of a commitment to familiar basic liberties (freedom of speech and conscience, freedom of religion, rights to privacy, personal property, and bodily integrity, and so on); it seems to me that either Rawls thinks that negative freedom follows from normative individualism or he finds negative freedom too obvious to include explicitly in the conception of freedom.

All together, the freedom of citizens on the "political conception of practice," will involve the following:

**normative individualism:**

- (1) Claims for the interests of practices, institutions, and other collective agents are made by and with respect to the interests of individual citizens, though possibly as members of these collective agents.
- (2) Citizens are capable of achieving progress within their practices, especially partly external progress through joint practice.
- (3) Citizens have the right to reasonably minimize their participation in the basic structure.

**autonomy: of individuals:** Citizens are capable of achieving progress within their practices, especially partly external progress through joint practice.

**of practices:** Practices should be valued for their contribution to the achievement of internal goods, especially their own. Institutions should be valued for their contribution to the achievement of internal goods.

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Liberalism," 58, 62; Elizabeth Anderson, "Toward a Non-Ideal, Relational Methodology for Political Philosophy: Comments on Schwartzman's *Challenging Liberalism*," *Hypatia* 24, no. 4 (Fall 2009): 133.



**positive freedom:** Citizens have a reasonable responsibility to promote and respect the freedom, fairness, and equality of other citizens.

**negative freedom:** “Each person has an equal right to a fully adequate scheme of equal basic liberties which is compatible with a similar scheme of liberties for all.”<sup>155</sup>

I will briefly comment on each element of this conception.

**Claims for the interests of practices, institutions, and other collective agents are made by and with respect to the interests of individual citizens, though possibly as members of these collective agents.**

It may seem that this statement of normative individualism is incompatible with constitutive dependence and the autonomy of practices. We can see that this is not the case when we keep in mind that a practice does not exist except in and through its practitioners. The practice of scientific inquiry (as a collective agent) is nothing more than the organized set of its practitioners and their collaborative, goal-oriented activity. When we say, for example, that a certain lab has just published a new study, we really mean that a certain set of organized, collaborating individuals have just published a new study. And when we say that an individual scientist’s aims have been constituted by science as a practice, we are saying that her aims have been shaped in a deep way by her work with other individual scientists. In the same way, to say that among her aims is participation in scientific inquiry is to say that among her aims is to collaborate with other scientists. Note that, on either way of putting it, we are claiming interests for her as a member of a collective agent, namely, as a scientist.

Often it is much simpler and more perspicuous to speak directly in terms of collective agents: “politicization is bad for science” rather than “politicization interferes with the ability of scientists to collaborate with each other on the production of new representational knowledge, practical knowledge, and technology.” But at times it is essential to recognize that the collective agent is merely one’s colleagues. For example, rhetorically speaking, it is one thing to ask a young scientist to sacrifice his family life in order to be a more effective collaborator or produce slightly more knowledge than others. It is quite another thing to ask him to make this sacrifice for some transpersonal Idea of Science.

**Citizens are capable of achieving progress within their practices, especially partly external progress through joint practice.**

As I have stressed repeatedly, the understanding of the internal goods of a practice is not fixed. In this sense, practices are progressive, and in a corresponding sense they

<sup>155</sup> Rawls, *Political Liberalism*, 291, *et al.*

are not conservative or tradition-bound. So they are, in a sense, free. Furthermore, in line with the points of the last two paragraphs, practices can only achieve progress through the activity of their practitioners. So if practices are free in the sense of being able to progress beyond established tradition, it must be because their practitioners are free in the sense of being able to progress beyond established tradition. And one especially important kind of progress is partly external progress, which requires joint practice. So it is especially important that citizens be regarded as free to engage in joint practice.

Note that freedom in this sense does not mean complete independence from tradition.<sup>156</sup> At a given time and place, a practice will have its own understanding of progress. This understanding is itself a product of the diachronic development of the practice, which we might as well call “tradition.” In an aphorism, progress is measured by tradition.

**Citizens have the right to reasonably minimize their participation in the basic structure.**

Think of this as the freedom to “opt out.”<sup>157</sup> The conception of practice takes participation in some practices — and perhaps, to an extent, some institutions — to be at least partly constitutive of the good life. That is, a good life involves participating in at least some of the activities of public political discourse, scientific inquiry, caring interactions with family and friends, and so on. And, generally and for the most part, people do seem to value their participation in these activities. But there are also exceptions: people who do not wish to do some of these things, and perhaps even occasional individuals who do not wish to do any of them.

Consider an extreme case: a religious hermit or radical Deep Ecologist, an individual who wishes to leave human civilization completely and live by themselves (or perhaps with their God or with non-human organisms) in the wilderness. Perhaps tragic and traumatic experiences have given this individual an aversion to her fellow human beings; perhaps she is pursuing some extreme version of more sociable religious or environmentalist practices. In any case, I see no reason to force her to remain a member of society. If complete solitude is what she desires, and she does not claim unreasonable assistance from us, then I think she should be free to go.

More modest and much more common are individuals who, while willing to remain in and make fair contributions to society, have no real interest in the activities that most of us find edifying and important.<sup>158</sup> Or individuals who choose to avoid certain practices and institutions as antithetical to their comprehensive conceptions:

<sup>156</sup> Compare Charles Taylor, *The Ethics of Authenticity* (Cambridge, MA and London: Harvard University Press, 1991), ISBN: 0674268636.

<sup>157</sup> Cf. Nussbaum, *Frontiers of Justice*, 79-80.

<sup>158</sup> Compare Rawls’s discussion of the blade counter as a counterexample to the Aristotelian Principle, *A Theory of Justice*, 379-80. While Rawls’s problem here is rather different from mine, our responses are similar: Such individuals are not common, and

Amish Mennonites who refuse to participate in national politics or use the products of scientific inquiry; Shakers, with their radical celibacy and rejection of aspects of the gender system of their day; communities of lesbian and GLBTQ separatists, who want to avoid participating in the contemporary gender system; freegans or members of utopian communes who do not participate in the market.<sup>159</sup> So long as these citizens do not expect unreasonable accommodations from others, it seems to me that they should be entitled to “opt out” of these parts of the basic structure.

However, the pervasive nature of the basic structure limits the possibilities for opting out. In a contemporary capitalist society, the institution of the market cannot be avoided entirely; or, to prevent otherwise unavoidable free-rider problems, the Amish may be required to pay taxes even though they do not vote or gain any advantage from the goods and services bought with those taxes. For these harder cases, I think we should examine carefully the reasons for opting out. In many of the examples listed in the last paragraph, I believe the basic reason is worry about institutional domination. In contemporary capitalist societies, the institutions of the market, the state, the gender system, racism, and so on, are extraordinarily influential, and I think the members of many practices are right to worry about the harmful effects of such influence. This freedom might thus be understood in terms of the autonomy of practices: the freedom of practices, even those outside the basic structure, from harmful interactions with institutions and perhaps even other practices, especially those within the basic structure.

If this is right, then one way to promote and protect the right to “opt out” would be to “spread out” the largest institutions, so as to minimize their potentially harmful influence. With respect to the state, this is (like) one of the familiar arguments for federalism: to prevent the state from overwhelming civil society, state power should be held by many different bodies rather than a single authority. With respect to the economy, it might be considered an argument for what Otto Neurath called “economic tolerance”:

If socialisation wanted to destroy the culture of the peasants and the craftsmen [sic] and prevent neo-communism [roughly, autonomous anarchist communes], it would not just encounter enormous resistance, but also pave the way to new conflicts, which modern social engineering had better prevent and avoid. Mere majority decisions will give way to an *economic tolerance* that can support several non-capitalist forms of economy simultaneously, just as in the United States settlements of Quakers are tolerated next to settlements of Mormons. (Do we really need an economic Thirty Years War to teach us tolerance?)<sup>160</sup>

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they can be reasonably accommodated.

<sup>159</sup> As MacIntyre puts it, “genuinely free markets are always local and small-scale markets in whose exchanges producers can choose to participate or not.” [“Politics, Philosophy and the Common Good,”](#) 249.

<sup>160</sup> Neurath, [“A System of Socialisation,”](#) 354-5, his emphasis and parentheses.

In context, Neurath is worried about internecine warfare among various socialist factions: Marxist-Leninists, social democrats, syndicalists, solidarists, and anarchists. While some of these factions, such as the Bolsheviks, were committed to violent and undemocratic forms of social change, Neurath believes that there is a critical mass of peaceful, democratic socialist factions. If these factions can find a way to respect their specific economic differences — if the factory-based social democrats can tolerate the craft traditionalism of the peasants and guild socialists, and both can tolerate the anarchism of the neo-communists — the prospects for effective socialist change seem to Neurath to be quite good.<sup>161</sup> Concretely, this proposal would require a wide variety of different institutions to replace corporations and the market of contemporary capitalism: we might still have a market (though perhaps a tightly regulated, “market socialist” sort of market, structured so as to support entrepreneurs and small for-profit businesses), but also a coordinated network of syndicalist guilds and various kinds of co-ops, and more-or-less self-sufficient agrarian peasant communities. This is, of course, rather imaginative, though it is notable that such alternative economic systems were considered live possibilities only a century ago. Perhaps such an alternative will seem like a live possibility once again a century from now.

**Practices should be valued for their contribution to the achievement of internal goods, especially their own. Institutions should be valued for their contribution to the achievement of internal goods.**

The autonomy of individuals has already been discussed above; it is also a part of normative individualism. Practices are autonomous in the sense that they pursue their own internal goods; specifically, they do not suffer institutional domination. This is, of course, a bit ideal: the tension between practical priority and institutional priority, and thus institutional domination, is ineliminable. This formulation of the autonomy of practices is useful because it implies that *institutional domination is an important unfreedom*, and so a (*pro tanto*) injustice. The threat of this unfreedom can never be eliminated, but we can recognize it as a wrong.

Institutional domination can also be understood as an unjust interference, that is, as a violation of negative freedom. Under institutional domination, the institution interferes with the pursuit of the internal goods of the practice: these activities are redirected in ways that are more conducive to the accumulation of wealth, for example, or are blocked entirely. With institutional domination rectified, the practice can proceed freely.<sup>162</sup>

<sup>161</sup> Neurath has some quite brilliant diagrams illustrating the agreements and disagreements among all factions of his day; see Neurath, “A System of Socialisation,” 351, 353; the critical mass is found in “Uniting the Programme of Socialisation” at the bottom of the diagram on 353; and Neurath’s unhelpful abbreviations are explained in the text starting on 350.

<sup>162</sup> Compare G.A. Cohen’s analysis of the relationship between wealth and negative free-

**Citizens have a reasonable responsibility to promote and respect the freedom, fairness, and equality of other citizens.**

As should be clear, this sense of freedom is borrowed directly from the third characterization of freedom in justice as fairness. I have modified Rawls's statement slightly in order to make the connection with the other elements of the conception more perspicuous.

**Each person has an equal right to a fully adequate scheme of equal basic liberties which is compatible with a similar scheme of liberties for all.**

Finally, the commitment to core or basic negative freedoms takes the form of the first of Rawls's two principles of justice.<sup>163</sup> However, I do not follow Rawls in giving this principle absolute or lexicographical priority over other considerations. Rawls's reasons for doing this are straightforward: he (1) takes the negative freedoms of the first principle to be incommensurable with the goods (such as wealth) covered by the second principle; and (2) thinks that a conception of justice must, to avoid an *ad hoc* or noncognitivist "intuitionism," give a principled answer to any question of relative importance or priority.<sup>164</sup> I accept the first assumption, but reject the second. Judgments of relative importance or priority can be decided in large part by concrete features of the particular case or context rather than always relying upon context-independent general principles.

### 7.7.2 Equality

Compared to his discussion of freedom, Rawls's discussion of equality as such is quite short, occupying just a few sentences in *Political Liberalism*. The following captures the gist of the conception:

We can say that a person is someone who can be a citizen, that is, a normal and fully cooperating member of society over a complete life . . . . Since we start within the tradition of democratic thought, we also think of citizens as free and equal persons. The basic idea is that in virtue of their two moral powers . . . and the powers of reason . . . , persons are free. Their having these powers to the requisite minimum degree to be fully cooperating members of society makes persons equal.<sup>165</sup>

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dom, G.A. Cohen, "Freedom and Money," chap. 8 in *On the Currency of Egalitarian Justice, and Other Essays in Political Philosophy*, ed. Michael Otsuka (Princeton: Princeton University Press, 2011), 166–192, ISBN: 9780691148717.

<sup>163</sup> Rawls, *Justice as Fairness*, 42, among many others.

<sup>164</sup> Rawls, *A Theory of Justice*, §§7-8.

<sup>165</sup> Rawls, *Political Liberalism*, 18-9.

Operationalized within the original position, the commitment to equality involves what I will call the *presumption of equality*: justice requires that citizens enjoy equal benefits and burdens until and insofar as it is in everyone's interest (as understood by maximin reasoning) to prefer an unequal distribution. For example, this is how Rawls argues for the difference principle:

The basic structure should allow organizational and economic inequalities so long as these improve everyone's situation, including that of the least advantaged, provided these inequalities are consistent with equal liberty and fair equality of opportunity. Because they start from equal shares, those who benefit least (taking equal division as the benchmark) have, so to speak, a veto. And thus the parties arrive at the difference principle. Here equal division is accepted as the benchmark because it reflects how people are situated when they are represented as free and equal moral persons.<sup>166</sup>

Note that it does not seem that the conception of equality strictly implies the presumption of equality. Instead, and to repeat my formulation above, the presumption of equality is the way in which Rawls operationalizes equality within the original position.

The first aspect of equality — the assumption that citizens have certain cognitive powers to “the requisite minimum degree” — has proved controversial. It seems to imply that non-human animals, young children, and adult humans with severe cognitive disabilities are not subjects of justice, even as they may be objects of justice.<sup>167</sup> That is, while the conception of justice may incorporate some protections for these individuals, they do not have a role to play in formulating or endorsing the conception of justice. Furthermore, it seems to imply that these individuals or their interests are inferior or subordinate to cognitively-normal adult humans or their interests.

While the conception of practice cannot completely avoid these worries, its more expansive conception of the basic structure is an improvement over Rawls.

**equality:** The presumption of equality applies to all individual agents who are able to participate as agents in at least one of the practices or institutions of the basic structure.

First, note that this formulation explicitly incorporates the presumption of equality; the presumption does not come later in the conception, when we operationalize it. I take this to be an improvement in perspicuity, but only in perspicuity. More substantively, many individual agents who are unable to participate in some basic practices are able to participate in others, and thus enjoy the presumption of equality in at least some respects. For example, dogs and cats and humans with severe cognitive

<sup>166</sup> Rawls, *Political Liberalism*, 282.

<sup>167</sup> Nussbaum, *Frontiers of Justice*.

disabilities are unable to participate in public political discourse or scientific inquiry (at least, as agents), but they are able to participate in affective practices, such as families. Hence these individual agents, who do not seem to enjoy the presumption of equality under Rawls's conception, seem to enjoy this presumption under the conception of practice. This does not mean that dogs and cats should have the right to vote; but it might mean that dogs and cats should have standing as members of families, and similar to the standing that humans (especially small children) have as members of families.

How far does this presumption extend? As a first pass, until and insofar as we have good reason to go with inequality instead. Or, we might say, inequality suffers the burden of proof. Of course, this answer is unsatisfying and difficult to operationalize. Consider a proposal that employers be required to offer additional break periods throughout the working day and adequate facilities for lactating mothers to nurse or pump milk. Is this equal or unequal treatment? There are *pro tanto* arguments either way.

### 7.7.3 Fairness

And so we come to the final commitment of political liberalism, the commitment to society as a fair system of cooperation over time. Rawls explicitly characterizes fairness in terms of three “elements”:

**legitimacy:** There are “publicly recognized rules and procedures that those cooperating accept and regard as properly regulating their conduct.”

**reciprocity:** “All who are engaged in cooperation and who do their part as the rules and procedures require, are to benefit in an appropriate way as assessed by a suitable benchmark of comparison.”

**thin theory of the good:** “An idea of each participant’s rational advantage, or good,” that is, what it is that “those who are engaged in cooperation . . . are trying to achieve, when the scheme is viewed from their own standpoint.”<sup>168</sup>

Roughly, legitimacy is fair insofar as it is not fair for citizens’ lives to be governed by rules and procedures that they do not or cannot accept. Fairness requires that the terms of social cooperation be acceptable to all. I think that this understanding of fairness and legitimacy is among Rawls’s reasons for taking a social contract approach in justice as fairness.

Rawls characterizes reciprocity as “between” altruistic impartiality — the sort of universal egalitarian concern for all sentient beings based on sympathy that is

<sup>168</sup> Rawls, *Political Liberalism*, 16. Note that “legitimacy” is my term, not Rawls’s; his discussion of legitimacy is similar, which is why I am borrowing the term, but distinct. See *ibid.*, 137ff.



common in the utilitarian tradition — and “the idea of mutual advantage understood as everyone’s being advantaged with respect to each person’s present or expected future situation as things are.”<sup>169</sup> Furthermore, the two principles of justice, especially the difference principle, “formulate an idea of reciprocity between citizens.”<sup>170</sup> In light of the contrast between the preferred interpretation of the difference principle and Pareto optimality in *Theory*,<sup>171</sup> I believe that it is reasonable to read “mutual advantage” as Pareto optimality. That is, an allocation is mutually advantageous if and only if it is Pareto superior to the status quo.<sup>172</sup> Hence Rawlsian reciprocity might be Pareto inferior to the status quo. As he says in another context,

It is a mistake to believe that a just and good society must wait upon a high material standard of life. What [people] want is meaningful work in free association with others .... To achieve this state of things great wealth is not necessary. In fact, beyond some point it is more likely to be a positive hindrance, a meaningless distraction at best if not a temptation to indulgence and emptiness.<sup>173</sup>

In particular, straightforward readings of the difference principle require a transfer of wealth (using coercive force or otherwise) from the wealthiest members of our society to the least advantaged.

Now, in what sense of “fairness” is such a transfer required by fairness? Recall that Rawls assumes that the well-ordered society that he is trying to describe is closed: citizens do not choose to be born into it, and have no possibility to emigrate.<sup>174</sup> So the least advantaged members of society cannot leave for greener pastures. Furthermore, the status or level of citizens is defined over their entire lives. In particular, the least advantaged are defined as those individuals who enjoy the smallest bundle of social primary goods over their entire lives. Those individuals who are born among the least advantaged but gradually improve their status, *au Horatio Alger*, are as such not among the least advantaged. All together, the least advantaged as such have only very limited opportunities to improve their status and pursue their comprehensive conceptions, especially compared to the more advantaged. Any distribution that does not satisfy the difference principle — the leaves the least advantaged with less than they would have under some other distribution — can thus be seen as requiring the least advantaged to sacrifice for the benefit of the most advantaged: the rich are richer in the non-difference principle society, but the poor are poorer. And this is unfair. Indeed, it is not far from certain conceptions of exploitation.

Finally, the line of thought of the previous paragraph presumes some way of measuring and comparing well-being. We need to be able to say how well individuals

<sup>169</sup> Rawls, *Political Liberalism*, 16-7.

<sup>170</sup> *Ibid.*, 17.

<sup>171</sup> Rawls, *A Theory of Justice*, §§12-13.

<sup>172</sup> Compare Narveson, *The Libertarian Idea*, 175-84.

<sup>173</sup> Rawls, *A Theory of Justice*, 257-8.

<sup>174</sup> See, among several other places, Rawls, *Political Liberalism*, 12.

$X$  and  $Y$  are doing, and ask whether  $X$  is doing better than  $Y$ . This is done using what Rawls calls the “thin theory of the good” or, in other contexts, an “index of social primary goods,” and it is given by the third element of the conception of fairness. More generally, we need what contemporary political philosophers call a “metric of justice.”<sup>175</sup> I suggest reading the metric not as part of the content of fairness — as with the first two elements — but instead as a presupposition for discussing fairness, in the same way that a discussion of the height of various buildings presupposes a standard unit of length.

It seems to me that we also need the metric in exactly the same way for substantive discussions of equality, and perhaps also freedom, and so we might wonder why Rawls has included his metric here but not in the other two commitments. I do not have a good answer to this wonder, and raise it as a point for consideration in future work.

I am happy to accept legitimacy and reciprocity, though not as they are operationalized by the original position. Let me simply note that, on the conception of practice, issues we have encountered before — the recognition claim, the importance of joint practice, and the threat of institutional domination — appear once again. Public recognition of the conception of justice requires overcoming the obstacles presented by the recognition claim and concerns about the threat of domination, and these require joint practice. Failures of reciprocity include institutional domination — the institution is not cooperating, in the right way, with its practices — and the misperceptions and misunderstandings that follow from the recognition claim, as in the connection argument — it is unfair to treat a practice as though it were an institution.

Finally, on the conception of practice, the appropriate metric of justice would include not (or not only) the all-purpose social primary goods of Rawls’s justice as fairness, but rather both internal goods and external goods and considerations of their relations, such as institutional domination. But this is an argument to be made elsewhere. I mention it here because it gives us yet another way in which, on this political conception, the institutional domination of scientific inquiry can count as an injustice.

## 7.8 Conclusion

In this chapter, I have provided a sketch of a liberal conception of justice that incorporates my conception of practice and that supports the claim that institutional domination is unjust. I have generally worked within the framework of Rawls’s political liberalism, though at the level of detail it would be quite misleading to call the

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<sup>175</sup> For an early discussion in these terms, see G.A. Cohen, “On the Currency of Egalitarian Justice,” chap. 1 in *On the Currency of Egalitarian Justice, and Other Essays in Political Philosophy*, ed. Michael Otsuka (Princeton: Princeton University Press, 2011), ISBN: 9780691148717, 3–43.

conception of justice Rawlsian. I find this framework attractive because, I believe, it offers a real possibility to synthesize the liberal and radical/socialist/communitarian intellectual traditions. For the past two centuries, these two traditions have been deeply mutually antagonistic. If my sketch can be developed further, this antagonism is unnecessary: there is a coherent, perhaps even defensible, position in political philosophy that can incorporate both the liberal emphasis on individualism and persistent disagreement and the radical emphasis on interdependence and solidarity.

In addition, this position offers significant resources for analyzing and responding to a significant problem in contemporary US society: institutional domination. The opening section — and quite a lot of the proceeding chapters — give some idea of just how significant this problem is. And it should now be clear that the problem of institutional domination is closely related to the three vignettes with which this dissertation began: the persistent disagreement and profound mistrust over such issues as (purported) sexist biology, anti-capitalist climate science, and anti-environmentalist genetic engineering. In the brief conclusion that starts on the next page, I return to these vignettes, using them to provide a summary overview of my analysis.

# Chapter 8

## Conclusion

I opened this dissertation with three vignettes, sketching three different cases in which ethical and political values (and other values as well) appear to influence scientific inquiry. These vignettes led to the puzzle that, in turn, led to the rest of the dissertation: How do we distinguish the seemingly-beneficial cases of such influence — the feminist scientists, successfully challenging the sexist assumptions of their colleagues — from the seemingly-illegitimate cases — the scientists who are little more than corporate shills? In these final few pages, I show how the conceptual tools that I have developed *ad nauseum* in the intervening several hundred pages can be applied to this problem. This also provides an opportunity to get an overview of these tools.

Chapters 2 and 7 set out the basic conceptual framework: the distinction between internal goods and external goods; the distinction between practices and institutions; their fraught relations, culminating in the generic injustice of institutional domination; its specific forms of commercialization and politicization; and the role of the recognition claim in fostering misunderstanding and miscommunication. In chapter 3, I showed how scientific inquiry could be seen as a practice; this allowed me, in chapters 4, 5, and 6, to reconstruct the problem of “outside influences” on scientific inquiry. Where it is standardly taken to be an epistemological issue, I see it as a “sociological” issue of the relationship between scientific inquiry and other practices and institutions.

In this light, we can address the cases presented in the vignettes — and other cases — by asking two questions:

- (1) Does this interaction lead — or threaten to lead — to the institutional domination of scientific inquiry?
- (2) Does this interaction lead to — or promote — partly external progress in scientific inquiry?

If the answer to the first question is “yes,” then we have good reason to be wary of this interaction, or even claim that it promotes injustice. If the answer to the

second question is “yes,” then we have good reason to support this interaction, at least tentatively, or even claim that it promotes justice.

In the feminist case — as we saw in more detail in §5.2 — the answer to the first question is “no” and the answer to the second question is “yes.” By challenging sexist science, feminist influences on science have and continue to promote justice. Despite isolationist worries, the influence of feminism on science does not seem to have led to rubber-stamping feminist prejudices (whether of real or imaginary feminists) or reducing science to a cudgel in the culture wars. Indeed, the incorporation of feminist methods of gender criticism has improved several fields of scientific inquiry, especially biology, ethology, and social sciences. Notably, this has occurred even in fields where not all scientists identify as feminists. Recall the example of primatology discussed in §5.2: According to Linda Fedigan, primatologists abandoned the extremely *epistemologically* problematic method of ad lib sampling — recording only those behaviors that caught the observer’s attention — primarily on epistemological grounds, yet the criticisms of these methods were driven in important ways by feminist commitments. Even a few decades after incorporating these criticisms and developing new methods, primatologists reject the characterization of their discipline as a feminist one. Sarah Richardson’s analysis of the history of gender criticism in research on sex determination is similar:

First, cultural change in and around the field of sex determination genetics created the conditions for receptivity to gender criticism, including early feminist criticism from outside the field. Second, a respected female scientist in the field, Jennifer Graves, began to employ an explicitly feminist framework in her work . . . . Third, over time, members of the larger sex determination research community came to see gender criticism as useful to their own thinking, incorporating feminist insights even while often not explicitly articulating them as such.<sup>1</sup>

These are cases in which feminism successfully stimulated partly external progress in scientific inquiry. And so it seems there is good reason to encourage further interactions between scientific inquiry and feminism

In the case of the climate change skeptics, *prima facie* the answer to the first question is, generally and for the most part, “yes” and the answer to the second question is, generally and for the most part, “no.” While some climate change skeptics have raised valuable methodological points, and thereby led climate scientists to produce even more excellent representations of climate systems, many more of these criticisms are otiose and irrelevant for public policy at best, disingenuous and misleading at worst, and overall serve only to delay actions to reduce greenhouse gas emissions and

<sup>1</sup> Sarah Richardson, “When Gender Criticism Becomes Standard Scientific Practice: The Case of Sex Determination Genetics,” chap. 2 in *Gendered Innovations in Science and Engineering*, ed. Londa Schiebinger (Stanford: Stanford University Press, 2008), 23, ISBN: 9780804758154.

adapt to climate change, to the financial benefit of the fossil fuels industry.<sup>2</sup> Hence, generally and for the most part, climate change skepticism threatens (and, indeed, has brought about) injustice.

However, the sorts of complications that were raised in the discussion of Nazi science in §5.5 can also be raised here. At least some prominent anti-environmentalist scientists see themselves as defenders of scientific inquiry, technological progress, and individual freedom from anti-scientific, “Luddite,” authoritarian environmentalists:

It’s not difficult to understand some of the motivations behind the drive to regulate CFCs out of existence. For scientists: prestige, more grants for research, press conferences, and newspaper stories. Also the feeling that maybe they are saving the world for future generations . . . .

I have left environmental activists till last. There are well-intentioned individuals who are sincerely concerned about what they perceive as a critical danger to humanity. But many of the professionals share the same incentives as government bureaucrats: status, salaries, perks, and power. And then there are probably those with hidden agendas of their own — not just to ‘save the environment’ but to change our economic system. The telltale signs are the attack on the corporation, the profit motive, and the new technologies.

Some of these ‘coercive utopians’ are socialists, some are technology-hating Luddites; most have a great desire to regulate — on as large a scale as possible . . . .

I tried to explain all this in a letter to the editor of *Issues [in Science and Technology]*, but he turned it down. Twice, in fact. So much for open discussion of important scientific and public-policy issues.<sup>3</sup>

In short, these anti-environmentalists (for lack of a better, more positive term) might see themselves as participants of some sort of practice — allied with, but still distinct from, scientific inquiry — that is actively opposing the threat of institutional domination posed by environmentalists, and promoting continued technological progress and, consequently, improvements in human well-being.

The threat of institutional domination posed by environmentalists can be seen in the way that they refuse to respond to objections (which requires repeating them incessantly) and are willing to exclude their critics coercively from fora for scientific inquiry and public policy deliberation. Indeed, during the Climategate scandal — the publication of emails from the Climatic Research Unit at the University of East Anglia in November 2009, and the ensuing investigation and controversy — it was revealed that climate scientist Michael Mann had apparently coordinated a retaliation

<sup>2</sup> Oreskes and Conway, *Merchants of Doubt*, ch. 6.

<sup>3</sup> S. Fred Singer, “My Adventures in the Ozone Layer,” *National Review*, June 30, 1989, 36-7. For a review of Singer’s anti-environmentalist work, see Oreskes and Conway, *Merchants of Doubt*.

and blacklist campaign against the peer-reviewed journal *Climate Research*, after it published a criticism of Mann’s work by of Harvard astrophysicists Willie Soon and Sallie Baliunas in 2003.<sup>4</sup> Mann and a group of colleagues wrote a response to the Soon and Baliunas paper, challenging their analysis and arguing that it relied upon “just one-off scattered warm events.”<sup>5</sup> When the publisher of *Climate Research* stood by the Soon and Baliunas paper, Mann proposed the following:

I think we have to stop considering *Climate Research* as a legitimate peer-reviewed journal. Perhaps we should encourage our colleagues . . . to no longer submit to, or cite papers in, this journal. We would also need to consider what we tell or request of our more reasonable colleagues who currently sit on the editorial board.<sup>6</sup>

In the wake of this, one of the journal’s editors, Hans von Storch, resigned; “sceptical climatologist” and Cato Institute fellow Pat Michaels has alleged that von Storch’s resignation was the work of Mann.<sup>7</sup> Then, in 2004, climatologist Phil Jones wrote the following in an email to Mann:

I can’t see either of these papers [including the Soon and Baliunas paper] being in the next [Fourth] IPCC [Assessment] report. Kevin [Trenberth] and I will keep them out somehow — even if we have to redefine what the peer review literature is!<sup>8</sup>

Jones and Trenberth were joint lead authors for chapter 3 of Working Group I’s contribution to the Fourth Assessment Report, which dealt with observations of warming over the past 150 years or so. Soon and Baliunas’s work was discussed in chapter 6, which dealt with paleoclimate research. If environmentalist scientists treat those who disagree with them in this way, think of what environmentalist politicians would be willing to do!

In light of the recognition claim and the connection argument, our (here referring to environmentalists, or perhaps just me) lack of familiarity with anti-environmentalism may be leading us to perceive, erroneously, that anti-environmentalism is an institution — propaganda for the fossil fuels industry — rather than a practice — the pursuit of such internal goods as technological progress and individual freedom. So, first, we must consider more carefully the products of anti-environmentalism — whether, generally and for the most part, it has produced institutional domination and not

<sup>4</sup> Fred Pearce, “Climate Change Emails between Scientists Reveal Flaws in Peer Review,” *The Guardian* (February 2, 2010): 12, <http://www.guardian.co.uk/environment/2010/feb/02/hacked-climate-emails-flaws-peer-review> (accessed March 2, 2012).

<sup>5</sup> *Ibid.*

<sup>6</sup> *Ibid.*, quoting an email by Mann, ellipses in Pearce.

<sup>7</sup> *Ibid.*

<sup>8</sup> *Ibid.*, quoting an email by Jones, my brackets.



stimulated partly external progress. But, second, if this more careful consideration of anti-environmentalism gives even a small indication of internal goods, it may be necessary to pursue the possibility of joint practice with anti-environmentalists. The weak recognition claim suggests that this is the only way we will be able to recognize the practice, if it is indeed one. Such a pursuit should be done with immense care and deliberation, of course, but this does not imply that it should not be done.

This seems like an appropriate place to recall one of the major caveats from the introduction: this dissertation has been written from the perspective of a particular set of social and political commitments and experiences. Like any such perspective, it holds significant resources for reflection and analysis — familiarity with feminism, for example — but suffers weaknesses and deprivations in other respects — such as a lack of familiarity with conservative and pro-capitalist movements. This limitation needn't preclude us from answering the two questions about the influence of anti-environmentalism on scientific inquiry. But it does preclude taking these answers to be certain and incorrigible. Anti-environmentalists are not Nazis; they are not so spectacularly institutionally dominated that we (again, meaning environmentalists) should avoid interactions with them. The recommended course of action would thus seem to be cautious engagement. Could a special conference or series of projects promote better mutual understanding between environmentalists and anti-environmentalists?

Finally we have the vignette of environmentalist critics of transgenic crop research. The most widely cultivated transgenic crops are varieties of corn, soybeans, and cotton that either (a) are resistant to glyphosate, an herbicide produced by chemical company Monsanto (which also owns the patents to these glyphosate-resistant crops), or (b) produce the same insecticidal toxin as the bacterium *Bacillus thuringiensis*. It is not clear whether these crops are more profitable to farmers, are more productive (as measured by bushels per hectare), or have milder environmental impacts (because they require fewer or less environmentally hazardous pesticides) than non-transgenic varieties.<sup>9</sup> And the most high-profile ongoing research in transgenic crops deals with creating varieties of other major commodity crops with either or both of these two characters.<sup>10</sup> Thus it seems that the development and widespread adoption of transgenic crops is best explained by their profitability for patentholders. This should at

<sup>9</sup> Compare Maurizio Paoletti and David Pimentel, “Environmental Risks of Pesticides versus Genetic Engineering for Agricultural Pest Control,” *Journal of Agricultural and Environmental Ethics* 12 (2000): 279–303; Martin Qaim, “The Economics of Genetically Modified Crops,” *Annual Review of Resources Economics* 1 (June 2009): 665–94, doi:10.1146/annurev.resource.050708.144203.

<sup>10</sup> For example, in January 2011 the USDA approved a version of alfalfa that is resistant to glyphosate. See “USDA Announces Decision to Fully Deregulate Roundup Ready Alfalfa,” United States Department of Agriculture, January 27, 2011, <http://www.usda.gov/wps/portal/usda/usdahome?contentidonly=true&contentid=2011/01/0035.xml> (accessed March 2, 2012).

least make us sensitive to the possibility of institutional domination.

But there is other research on transgenic crops that does not seem to involve any institutional domination whatsoever, and also seems to promote the ethical and political values of environmentalist and peasant movements. For example, one of the major non-chemical methods of weed control in rice cultivation is submergence — flooding the paddies so that other plants cannot grow. However, many varieties of rice cannot survive submergence as sprouts, and so weed control requires either labor- or chemical-intensive methods until some time into the growing season. The geneticist and plant pathologist Pamela Ronald has identified the genes that make a certain variety of rice submergent-tolerant, and used transgenic techniques to develop new versions of widely grown varieties with these same genes.<sup>11</sup> This work was funded by the USDA. Transgenic crops could also be developed to be more productive in areas of drought and desertification or high salinity.

There does not seem to be any strong evidence of health and safety or environmental issues with transgenic crops in general — there are particular issues, as when a potentially allergenic or toxic protein is introduced — though, obviously, there have been no low-dose-long-term exposure studies as of yet. Thus, some observers argue that the ongoing dispute over transgenics is not actually about health, safety, and environmental issues that can be analyzed using conventional risk measures; rather, they argue, the dispute is over issues of public participation in technology governance and commercial control through the intellectual property system.<sup>12</sup> If this analysis is correct, then our answers to the two questions will be more complicated than those given by many critics of transgenic crops.<sup>13</sup> As they have been developed so far, transgenic crops may involve objectionable institutional domination. On the other hand, proponents of transgenics would seem to be correct when they argue that transgenics can *potentially* help adapt to climate change, make peasant agriculture more productive and nutritious, and so on. The weakness of this argument is that transgenic research has not *actually* been developed in this direction, or that the products of such research have not been widely adopted.

The best course of action for environmentalists and peasant movement activists, I suggest, is to pursue joint practice with molecular biologists (and vice versa). Not

<sup>11</sup> Kenong Xu et al., “*Sub1A* is an Ethylene-Response-Factor-Like Gene that Confers Submergence Tolerance to Rice,” *Nature* 442 (August 10, 2006): 705–8, doi:[10.1038/nature0492](https://doi.org/10.1038/nature0492); Pamela Ronald and Raoul Adamchak, *Tomorrow’s Table: Organic Farming, Genetics, and the Future of Food* (Oxford and New York: Oxford University Press, 2008), ISBN: 9780195301755.

<sup>12</sup> See, for example, Ambuj Sagar, Arthur Daemmrich, and Mona Ashiya, “The Tragedy of the Commoners: Biotechnology and Its Publics,” *Nature Biotechnology* 18 (January 2000): 2–4, doi:[10.1038/71861](https://doi.org/10.1038/71861).

<sup>13</sup> Compare Vandana Shiva, *Biopiracy: The Plunder of Nature and Knowledge* (Cambridge, MA: South End Press Collective, 1997), ISBN: 0896085554; Altieri, “[Multifunctional Dimensions of Ecologically-Based Agriculture in Latin America.](#)”

only will this improve mutual understanding of the possibilities (and dangers) of this technology; it is also, I think, the only way that research on transgenics is liable to be directed towards the promotion of the ethical and political values of environmentalists and peasant movements. One major task for such joint practitioners will be the development of alternative (that is, non-commercial) systems of funding and institutional support for this research. As the cost of DNA sequencing drops, transgenic research becomes increasingly feasible outside of the familiar institutions of “big science.” Could a small farming community — a town, a county, a region — directly support their own genetic engineers to develop crops that meet their particular needs? If so, there may be an important alternative to institutionally dominated “big genetics.”

The analysis of these three vignettes has been quick and suggestive rather than careful and thorough. In each case, I have given answers to the two questions, but also indicated complications and points where further empirical investigation is required for a satisfying or conclusive answer. My aim here has not been to give such conclusive answers, but instead to show how the tools that I have developed in this dissertation can be utilized. In each case, it is clear that merely thinking through the problem is insufficient to resolve it. A practical — effective — resolution requires practical — material, active — engagement with the problem. And, so often, the required practice is joint practice.



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