The Hunt for Reality: Perspectives, Models, and Plurality in the Physical Sciences

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Declaration

This thesis is entirely the work of the author, except where indicated. No part of the thesis has been submitted for any other degree or qualification.

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Abstract

This thesis tackles the problem of realism in science by examining the analyses and insights that pluralism and perspectivism might offer. Scientific perspectivism was introduced by Giere (2006) as a way to use insights from the semantic analysis of theories to strike a middle ground between realism and anti-realism about science, which I discuss in chapter 1. The project here attempts a similar balance in the context of disagreement in specific scientific-historical contexts. It does so by suggesting we think of some forms of disagreement as taxonomic, or identity, disagreements. Scientists use perspectival taxonomies and when problems with a given taxonomy arise, rival "perspectives" emerge (hence perspectivism is a form of pluralism). Such problems can be resolved by appeal to trans-perspectival standards of assessment. This approach has the advantage of being sensitive to the historical context in which past theories were used, a virtue that anti-realist views typically have. At the same time, perspectivism does not fall into an anti-realist attitude toward science because it is compatible with stronger realist commitments to the interpretation of scientific theories.

To make this argument, I first discuss how data and data-to-phenomena inferences depend upon perspectival taxonomy (in chapter 2) and hence cannot always be used unambiguously to resolve disagreements. I next articulate the perspectival view, defend it, and situate it within the literature on scientific pluralism in chapter 3. Chapter 4 provides a perspectival interpretation of a paradigmatic case of historical disagreement: the Chemical Revolution and the debate between Lavoisier and Priestley on oxygen and phlogiston. In chapter 5 I distinguish and defend my own view against two influential alternatives (relativism and pragmatism) that also aspire to provide insightful analyses of disagreement in science.

Lay Abstract

There are two extreme attitudes we might take toward scientific products, such as theory, scientific explanations, or scientific models. Of particular interest here is the *success* of those products, where success encompasses things like good predictions, the ability to develop technology, accurate explanations, and whatever other positive results we take science to provide. The first attitude, realism, suggests the success of science is best understood in terms of truth or approximate truth, suitably defined. An implication of this attitude is that the truth or approximate truth of a theory, say, is independent of human cognition, i.e. whether a theory is true is independent of whether there is someone who can know whether it is true. The other attitude, anti-realism, suggests truth, approximate truth, and independence of human cognition are unsuitable concepts for understanding the success of science. Typically, these two attitudes form a dichotomy, but there are also a number of attempts to bridge the gap between them. One such attempt, which this thesis will elaborate, is perspectivism, so called because its proponents take human vision as a metaphor. Crudely, it is possible for us, humans, to perceive the world around us because of two preconditions; one is a world that exists independently of our perceptual organs and the other is those perceptual organs. Other views, perspectivists might argue, run into trouble by thinking that to understand scientific success, only the world is necessary (realists) or only perceptual organs are necessary (anti-realists). By allowing both, perspectivists hope to move beyond the realism/anti-realism dichotomy. This thesis argues for a perspectival interpretation of science.

To make this argument, I first discuss how data cannot be used as certain realists intend to ground their views because there is a human cognitive element to them (in chapter 2) and hence cannot always be used unambiguously to resolve disagreements. I next articulate the perspectival view, defend it, and situate it within the philosophical literature in chapter 3. Chapter 4 provides a perspectival interpretation of a paradigmatic case of historical disagreement: the Chemical Revolution and the debate between Lavoisier and Priestley on oxygen and phlogiston. In chapter 5 I distinguish and defend my own view against two influential anti-realist alternatives (relativism and pragmatism) that also aspire to provide insightful analyses of disagreement in science.

Table of Contents

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Preface

This thesis is an attempt to use perspectivism as a lens for furthering our understanding of realism in science, particularly in the face of disagreements and great conceptual change. In chapter 1, I situate and motivate perspectivism, showing how proponents of this view have attempted, and sometimes failed, to mediate between realism and anti-realism. Failure is often associated with an inability to account for the empirical side of science. The next step in my analysis is to address scientific evidence. Within science, one typically expects empirical evidence, especially in the form of data, to resolve disagreements, test theory, or justify theoretical change. However, chapter 2 argues that data and evidence are distinct. I make this distinction by arguing data are representational, but not mind-independent evidence because they depend upon the conceptual taxonomy.

Chapter 3 provides clarification on what the conceptual taxonomy is. I situate my view in the literature on epistemic pluralism. Many existing accounts approach pluralism from a pragmatic position. I suggest there are some limits on this line of thought and argue we should understand pluralism primarily as conceptual and therefore taxonomic. I draw on resources from the philosophy of language, particularly Wittgenstein, in making this claim. Two features of this account are that it takes language to be a kind of practical ability and sharing a conceptual taxonomy is a precondition for belonging to the same practice.

In chapter 4 I illustrate the perspectival view using the Chemical Revolution. In chapter 5 I contrast perspectivism with two other views—relativism and scientific pragmatism—that also purport to advance our thinking about disagreement and change. Pragmatism and relativism both struggle with accounting for the objectivity, or at least inter-subjectivity, of science. They also suppose it is possible to make sweeping claims about the variety of epistemic positions, while denying that there is such a position to make such claims. Perspectivism avoids both of these issues. It suggests disagreements arise between those with shared background. The evaluation of scientific products is consequently restricted to individual judgements, but such evaluation must have broad appeal.

I

Realism, Anti-Realism, and the Promise of Perspectives

There is no outside; outside you cannot breathe—
Where does this idea come from?
It is like a pair of glasses on our nose
through which we see whatever we look at.
It never occurs to us to take them off.
—Wittgenstein *Philosophical Investigations*

1.0 Introduction

This introductory chapter provides context and motivation for a new view of scientific disagreement and conceptual change that draws on perspectivism. Giere has recently re-articulated perspectivism as a model-based approach to analysing science that shares some commitments with both realists and non-realists. Each of these camps is susceptible to two opposing issues that any account of science should strive to avoid. On the one hand, an account of science should be sensitive to the historical and social context in which scientists work. On the other, a satisfactory account should acknowledge that science is an empirical inquiry and as such, scientists are responsive to, and answerable to, empirical constraints, whatever those might be. Realists tend to struggle with the first issue and non-realists, particularly constructivists, with providing a satisfactory explanation for science's empirical successes. I believe perspectivism as Giere articulates it also shares these difficulties with constructivists, though I hope to advance the perspectival view by clarifying its scope and applying it specifically to cases of disagreement and conceptual change.

In this chapter, I describe the realists' commitments (section 1.1) and why anti-realists might depart from them (section 1.2). I argue that some forms of realism wrongly ignore the scientific context. One way of avoiding this problem is to build an account of science using a conceptual taxonomy (section 1.3). This is difficult to do without falling into strong constructivism and ant-realism. Perspectivism offers another approach, but I suggest this view easily encounters problems similar to those faced by constructivists (section 1.4). But these problems are not endemic. I discuss where perspectivism has been successful (section 1.4.5) and how this thesis will further develop the view (section 1.5).

1.1 Realism and its Tenets

As Hacking notes (1983, chap. 1), different realists at different times have used the term "real" to elicit different contrasts and resist different "anti-realist" positions. Consequently, realism is not easily captured by a broad brush. There are, however, three commitments realists often have: a metaphysical, a semantic, and an epistemic commitment (Psillos 1999, xix). Not every realist accepts all three and some forms of realism do not explicitly endorse any.

1. The metaphysical commitment is to the existence of a mind-independent world. That commitment could take several forms such as mind-independent existence of entities (Hacking 1983), structure (Ladyman 1998), or natural kinds (Boyd 1991), or combinations of these. This is the most central commitment that realists have and it is meant to satisfy a simple intuition about the nature of the world.
2. The realist semantic commitment is to the literal interpretation of theories, i.e. the terms used in a scientific theory have referents. This is also a commitment to bivalence. One of the main purposes of this commitment is to put observable and unobservable features of the world on par, at least to the extent that we should take at face-value the descriptions of any scientific theory under consideration, regardless of whether the entities it postulates are observable or not.
3. Epistemically, realists are committed to the truth or approximate truth of scientific theories and contemporary theories have greater approximate truth than past theories. Kuhn-loss and incommensurability, phenomena that contest progress in science, are particular foils for this commitment.

Taken together, these commitments would form a position that takes a realistic interpretation of each main feature of science (metaphysics, semantics, and knowledge). It is, therefore, a very committing form of realism. To be anti-realist, one need only reject some combination of these premises. There are therefore a number of different ways of rejecting realism. For example, constructive empiricists mainly reject epistemic realism (Van Fraassen 1980, chap. 1), so are mostly opposed to 3, but they also think we should literally interpret theories, making them semantic realists. Some social constructivists would accept 2 and even 3 while mainly taking issue with 1.[1]

[1] Bloor for example accepts a form of correspondence (David Bloor 1999, 89), but denies that the correspondence is mind-independent.

Endorsing all three tenets is a very strong commitment to realism, but there are also less strict forms of realism. As van Fraassen articulates it, realism is minimally committed to the following:

> Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true (1980, 8).

This suggests that, minimally, one could be a realist about science (and accept tenet 3) while remaining non-committal on the other two points. A full blown and strong anti-realism, this suggests, would need to reject only 3, as van Fraassen does.

Hacking articulates a realism that is even less committal than epistemic realism. He is a self-described entity realist (Hacking 1983, chap. 1), i.e. he takes a realistic interpretation of entities like electrons while avoiding commitment to a realistic reading of scientific theory. In defending entity realism, he suggests we take our ability to use an entity for investigating other phenomena as a criterion for its existence. This may sound like straightforward ontological realism, but there a key difference. He proposes the criterion gives us the basis for not doubting existence: famously, "if you can spray them, then they are real" (*Ibid.* 22). So, if we can use, say, an electron to investigate some other phenomenon, we have no basis for doubting that electron's existence. The key point here is that realism is not a positive claim about the existence of entities generally, it is associated with specifying under what conditions it is suitable to doubt an entity's existence. Such a realism is a realism by default (we take a realistic interpretation automatically once there is no basis for doubt). More generally we might say that Hacking's view suggests that raising the realist question is only possible in certain contexts.

Cartwright offers an even starker view of the essential limitations of realist questions: "*When* you can spray them, they exist" (1994, 325). So while Hacking suggests there are contexts where doubting existence is no longer possible, Cartwright emphasizes that the claims we take to be true are only true in very constrained and specific contexts. She is particularly interested in the truth of scientific laws, but the point applies generally to scientific claims we take to be true. Realism, this suggests, is not a general problem *about* the sciences, but a problem that sometimes arises locally *within* the sciences.

The attitude Hacking and Cartwright take toward realism is the form of realism that is most congenial for the account of perspectivism I will defend in later chapters. The realist question, I will suggest, arises in specific cases in conjunction with an identity problem, a different kind of problems that often needs to be addressed with logical priority over existence.

However, why should we not just adopt all three of the stronger realist commitments? One important reason is the insensitivity these commitments tend to show toward the epistemic context, a problem Hacking and especially Cartwright highlight. There are many other reasons as well, but this insensitivity is particularly relevant for understanding why a constrained version of realism is preferable to a strong, full blown commitment to all three realist tenets.

1.2 The Departure from Realism

Relativism and constructivism are two long and rich traditions that reject realism and part of their rejection stems from the insensitivity realism shows towards the epistemic context when evaluating the products of science. Specifically, realism tends to focus on theory and whether it is true in the abstract. In doing so, it ignores the epistemic context. I discuss what the problem is, how the realists neglect the context, and argue the problem is serious.

The insensitivity to context can emerge from an analysis of theory in the abstract and such an analysis is often a starting point for a rejection of realism. Bloor (2016), who is a part of that long tradition that rejects realism,[2] has articulated one form this rejection can take. He claims mistaken analyses of science get into trouble by treating a body of propositions as having truth-status independently of any and all historical and social contexts. This is a rejection of the possibility of evaluating beliefs or claims independently of contexts in which they feature and this is antithetical to the realist approach to science, an approach that examines the truth of theoretical claims *independently* of any historical or social contexts.[3]

Sociology is not the only field to raise these concerns about the role of context in evaluating theory. Cartwright, as mentioned above, rejects a widespread a-contextual analysis of laws that she calls fundamentalism (1994). Fundamentalism is the view that laws of nature apply not just to those cases where scientists know how to apply them (such as in experiments), but universally, even to cases where scientists cannot currently apply them. The fundamentalist is therefore committed to an unconstrained and context-insensitive interpretation of laws and where laws are true. The fundamentalist is broadly

[2] Bloor straddles several traditions ranging from sociology to relativism and constructivism. Some of these other positions I will discuss later in this chapter. I consider Bloor's brand of non-realism here in particular because of the broad range of positions for which he has relevance and because his characterization of absolutism is congenial to views I will defend later.

[3] This characterization of context-insensitivity does not only apply to realists, some non-realists are guilty of insensitivity to context as well. Just to name two, Hempel (1948) and Carnap (Carnap in Feigl and Scriven 1957) were both engaged in projects that were highly insensitive to the historical context.

similar to Bloor's foil: both are committed to the truth of scientific claims and the evaluation of truth does not require appeal to context.

Why think context important at all? Isn't it natural to think a theory is true or not regardless of what scientists believe or what methods they have available? Realists, such as Psillos (1999, xxi), are sometimes explicit about this. He argues truth is not epistemic, just regulative. Regulative truth provides constraints on scientific theorizing, but offers no epistemic constraints; i.e. a claim is true regardless of whether we could ever recognize it as true. Therefore a theory is true, or not, independently of the epistemic positions that we could occupy.

Part of the problem with this picture is that we could never be in a position to judge the truth of theory (because truth is not epistemic); we could only be in a position to infer the truth of a theory based on its predictive or explanatory successes. There is therefore a gap between success and truth *and* a gap between what is epistemically available to us and what the world is like. We now have a clash in intuitions: on the one hand it seems whether a theory is true should be independent of the human position we occupy, but it also seems like the world is available to us to know and make judgements about.

This tension leads anti-realists to an alternative to the regulative conception of truth: the epistemically constrained conception. This second view says that for a claim to be true, it must be possible to be in a position to judge that it is true (at least in principle). This is a context sensitive position. It is context sensitive because for a claim to have a truth-value, there must be a context from which we could judge its truth. So when evaluating a claim, it is necessary to consider the context which we would need to occupy to judge its truth. Cartwright, for example, might say that a law applies in those positions where we could judge the suitability of its application, i.e. a theory is true in those contexts where we can apply it.

General strategies for showing more sensitivity to the human epistemic position can include arguing for mind-dependence in science. Such mind dependence might be metaphysical (Knorr-Cetina 1983; Cetina 1993; Latour and Woolgar 2013). This is the strongest anti-realist position. Scientists, by this view, "construct" facts. This has the advantage of closing the gap between truth and what we can know. Truth, what the world is like, is accessible and context sensitive because it is the product of our cognition. However, this strategy has the unfortunate consequence of entailing that what the world is like is determined, in a strong sense, by human cognition. This is quite radical and does not sit well with science as an empirical inquiry.

If the extremes of realism and constructivism are unsatisfactory, presumably some middle position could satisfy us by being both context sensitive *and* accepting the empirical nature of science. One strategy involves appealing to a form of mind dependence in conceptual taxonomy. This is a promising option, and ultimately this thesis's approach, because it promises to preserve the intuition that what the world is like does not depend upon human cognition. However, many of the existing strategies for developing conceptual taxonomies are too relativistic, or so I will argue in the next section. Goodman and Hacking are the foils I will use in making this argument.

1.3 Being in the Middle

Some attempts at avoiding realism involve locating mind dependence at the level of taxonomy. The advantage to this strategy is that we avoid making claims about metaphysics without first clarifying what the epistemic grounds are, in this case the taxonomy. It also shows sensitivity to the epistemic context because taxonomies change and are intimately connected to the historical period and the inquiries in which they feature. Despite these advantages, some of existing taxonomic accounts are too constructivist and consequently fail to properly account for the empirical character of science. I will show this below using accounts from Goodman and Hacking. I use these authors because they provide two radically different taxonomic options, but despite their differences, they face similar problems. I will ultimately be following a broadly similar strategy in appealing to taxonomy, but I will combine it with perspectivism and avoid their problems.

1.3.3 Philosophical Grounds for Construction: Goodman

Goodman's view is motivated by considering the grouping of kinds an activity. This active process applies across the board: to everyday life, to the subjects studied by science, religion, and art. The construction begins with categorization and because Goodman claims we cannot separate form and content very neatly, his view is strongly constructivist[4]. Metaphysics depends upon the categories and if the categories are constructed—which they are for Goodman—then the ontology is constructed as well. I will show this in more detail.

Goodman will claim that there is more than one right way to group objects, that is, more than one right way of sorting experience into categories. He discusses this in the

[4] See especially chapters 1, 4, and 5 in McCormick 1996.

context of worlds, where a world is a system of classification and two worlds reflect two different systems of classification.

> Much but by no means all world making consists of taking apart and putting together, often conjointly: on the one hand, of dividing wholes into parts and partitioning kinds into subspecies, analyzing complexes into component features, drawing distinctions; on the other hand, of composing wholes and kinds out of parts and members and subclasses, combining features into complexes, and making connections.[5]

Goodman in this passage makes repeated reference to classification. It is on the basis of these classifications that worlds are made and distinguished from one another. We construct classifications by groupings, which is what he means when he writes of constructing wholes and kinds out of parts. And we also take those wholes and kinds and make finer distinctions.

There is very little discussion of what constraints, if any, there are on our classifications. If there are no constraints on classification and if ontology depends upon classification, then it looks like Goodman's view is a very strong form of construction. Not only are taxonomies and classifications constructed, objects, facts, everything is constructed.

What does it mean for the world to be constructed? Two worlds are the same worlds when they have the same objects and whether the objects are the same is determined by whether they are classified in the same way. If worlds conflict in their ontology, then they are different. Therefore, it is not the case that people in different worlds can fail to interact with one another, but despite their ability to interact, they can live in separate worlds, just in the sense that the categories each uses differ. If the categories differ, so do the objects. These categories, therefore, are really what drive the Goodman notion of multiple worlds.

The language Goodman uses is quite dramatic—terms such as "star making" feature prominently in his work—and it applies across the board, including to human projects we traditionally think of as empirical, namely science. However, because Goodman offers no constraints on this construction, it is not only unclear what kind of role the world plays in science, it is not clear that anything in the world could play such a role. Consequently, it is unclear how science would differ from other forms of human inquiry because a key feature of science, that it is empirically informed or constrained, is missing.

[5] Goodman in McCormick 1996, p. 65.

1.3.4 A Milder Form of Constructivism

Even though constructivism seems antithetical to treating science as empirical, there are milder forms that are not so obviously incompatible. Hacking provides a paradigmatic example of a mild constructivist position that is also, he claims, realist. The position is mild because it is not committed to strong contingency. In discussing certain social kinds, like gender, child abuse, and mental illness[6], Hacking accepts contingency and constructivism within certain domains. Here constructivism means that the individual people are not constructed, but the act of putting them into categories—such as the category of schizophrenia—is contingent and dependent on the activity of classifying.

Hacking's attitude toward construction here may face difficulties not so different from Goodman's. Hacking does claim that science is contingent in its form, but not its content. To make this claim, he argues that the course of science is shaped by the questions posed by scientists.[7] Questions constitute a kind of framework or form in science and that the answers constitute the content. Questions are contingent, answers to them are not. Therefore, scientists could have chosen to not study nuclear fusion, in which case no one would know anything about nuclear fusion; there would be no science of nuclear physics. But any scientists choosing to study nuclear fusion would inevitably learn the same things about it, acquire the same knowledge, regardless of any other social phenomena. The consequence of this view, we might say, is that the things scientists know are contingent because they might have chosen to learn other things, but given the choices they did make, they inevitably learned what they did. Given that content is stable, the world is, in some sense, stable. We might phrase this in the following way:

> *Weak Contingency:* Which content that scientists consider is determined by the scientific inquiry, but the nature of that content is independent of any inquiry.

There are two issues I will discuss with being committed to weak contingency of this kind. The first is, can we be committed to the contingency of the form of science without the content as well? If the content is in some sense determined by the question and the question is contingent, does that not entail that the content is contingent as well? This stronger form of contingency can be expressed as follows:

[6] See Hacking 1999, p. 165
[7] Hacking 1999, p. 165.

> *Strong Contingency*: A scientific inquiry determines not only what content is under consideration, but what content there is.

The problem this question raises is what does asking a question do? Does it construct content, content that would not be there had the question not been posed? This is a strong reading. I take it that Hacking wants to offer a weaker view, which might be this: there is content and scientists are interested in some content, but not all content—and so their questions reflect their interest in that they study only a subset of the possible content. This weaker reading does not claim that a question creates content.

I'm not sure, however, that this weaker version is stable. It may collapse into stronger versions. Historical cases in particular are difficult to interpret as Hacking would like to. Consider the historical substance phlogiston, a substance we now do not believe exists. And yet when scientists in the past asked questions about phlogiston, inevitably their answers would involve phlogiston. The fact that the answer to our question involves phlogiston is determined by introducing phlogiston into the question.

And this suggests that we cannot avoid ascribing contingency to content: if we had not asked about phlogiston—which seems possible—then we would get no answer about it. If we are committed to the contingency of the question, we are also committed to the contingency of content. Hacking, this suggests, cannot maintain contingency about form without having the same view about content, which is precisely the point that Goodman was at pains to defend.

1.3.5 The Construction Approach: Challenges and Scope

There are two general problems I would like to suggest with the way these taxonomic accounts characterize science. The first is over generalization and the second is the paradigmatic type of activity they have chosen for their analysis. Both problems are closely related because they preclude the possibility for science to be affected by the world.

The first problem is over generalization. The idea of a construction suggests taking materials and putting them together to make something new that is more than the sum of the original parts. Machinery, equipment, entire laboratories, and thermometers are intuitive cases of constructed and thus artificial objects. If we had not made them, they would not exist. If we ceased to exist, they would carry on as they are until nature did its work. For any of these kinds of object, it is quite clear what is meant when we say they are constructed. Even if we do not understand the process or the parts involved, some expert does and we can understand how, in a general way, the object is constructed.

Now the constructivist, of a certain kind, wants to extend this idea of construction beyond the normal set of objects and apply it to things that are not straightforwardly constructed. Such objects as stars, rocks, electrons, etc. These other kinds of objects are not things that we put together using various parts and at first blush it is not obvious what the constructivist might mean when claiming they are constructed. The problem seems to be one of over generalization: taking the term construction and applying it too broadly. It is too broad an application because natural objects fail to meet the criteria for construction.

The constructivist might say that of course we do not make natural objects in just the same way we make artificial objects, but we have constructed them in a different sense. We, for example, have built laboratories and very artificial environments in which to conduct experiments and the objects we investigate in them are, by extension, also constructed. Alternatively, our perceptual faculties build the objects we perceive, so even stars and objects not within the confines of laboratories are constructed. We each take different features of the environment and in some way put them together to construct objects.

Firstly, the problem with laboratory constructions is that it does not generalize. Of course we can only make certain experiments and observations in constructed environments and of course these are different from natural environments. This does mean that there is an open question about how far we can extend what we learn in a laboratory into the rest of the world, but this concession does not imply the broad scope of construction that the constructivist wants. Putting a goldfish in a tank involves creating an artificial environment, but the goldfish is not constructed just because the tank is constructed.[8]

There is a second, more subtle trouble with the constructivist line of thought. It is that the criteria for a construction are still not met, or are at least not met in the way the constructivist imagines, even if we were to accept that science is inherently an activity. Let's examine Goodman's stereo case to illustrate this, but the point applies more generally.

> I sit in a cluttered waiting room, unaware of any stereo system. Gradually I make out two speakers built into the bookcase, a receiver and turn table in a corner cabinet, and a remote-control switch on the mantel. I find a system that was already there. But see what this finding involves: distinguishing the several components from the surroundings, categorizing them by function, and uniting them into a single whole. A good deal of making, with complex conceptual equipment, has

[8] I imagine this criticism applies in particular to forms of construction that focus on laboratory work, such as those defended by Knorr Cetina (2013; 2009, 1993; 1983), Latour and Woolgar (Latour and Woolgar 2013).

> gone into finding what is already there. Another visitor, fresh from a lifetime in the deepest jungle, will not find, because he has not the means of making, any stereo system in that room.[9]

One way to describe the scenario is this: the tribal member does not recognize stereos as devices for listening to sound, but I do. We might then wonder what is the difference between us? The difference is that I have learned how to identify, how to observe, and use stereos. The use of "identify" suggests action, but what sort of action is involved here? Is it, as Goodman suggests, a constructive sort of action? The kind of action associated with building. I think Goodman wants to suggest here that the mere act of identifying is constructive. But we might wonder why identification should be associated with constructive action?

A more natural emphasis in this case is to place more weight on how passive I am, or the way in which I am passive, when observing the stereo. Once I have learned what it is, there is no further learning or thing I must do in order to observe it, to pick it out, or use it to listen to recorded sounds. This way of characterizing the scenario is, I think, innocuous. The fact that I need to know what stereos are to use them as stereos should not undermine my confidence in the existence of stereos, in how to use them, or in the fact that not everyone knows what stereos are. But it also does not prevent the tribal member from taking it apart, picking it up, using it as a seat, and failing to interact with it as a stereo. We are going to use and interact with this object in very different ways, but this does not commit us to a strong dependence relation between stereo and my activity.

However, the constructivist may come back and claim identification is an activity, observation is an activity, and any activity has, inherently, an element that is not passive. I must, when observing a stereo, look at it, notice certain features, etc. These are activities. We can, therefore, characterize science as constructed, or based on constructions, because the act of identifying something is active and best construed as construction.

The right response here to distinguish between activities that construct and those that do not. Identification is a form of activity that does not result in the construction of something. So we might say that the constructivists are right to emphasize the activity in science, but incorrectly take construction as the paradigmatic activity. If we wanted to take the constructivist insight seriously, it would be to acknowledge that there is an active element inherent in certain human activities that seem passive at first blush. But the trouble with using this language is that it equivocates between activity and construction. There are all sorts of activities and not all of them involve building. The mistake is to conclude that something is constructed on the basis that an activity was involved in

[9] Goodman in McCormick 1996, p. 155.

identifying that object. But, contra the constructivist, we can say science involves activity without committing to a construction of facts, objects, etc.

In attempting to do justice to the human element inherent in science, these thinkers have appealed to taxonomy, but characterized taxonomy in such a way that it is unclear how the world could place any constraints on our classificatory practices. This lack of empirical constraint will also prove a problem for some perspectival accounts.

1.4 Perspectives: History and Trajectory

What we have so far seen is taxonomy-based accounts attempting to show sensitivity to both empiricism and context in evaluating science. However, they are too strongly constructivist. Perspectivism is another such strategy that seeks middle ground. The existing literature shows the view has challenges and promise. One challenge is to limit the scope of perspectivism such that one is not making overly general claims about knowledge. This is a challenge with the metaphor. The second challenge is how to account for data, the supposed empirical evidence on which scientists rely. Giere and Teller struggle to provide accounts that avoid these two problems, as we shall see in 1.4.3. The promise, on which Mitchell delivers, is to acknowledge the connections and relevance our different methods and forms of knowledge have for one another, i.e. science is not fully unified, but it is also not fully disunified either. Science can be, on occasion subject to integration, despite the great disunity we often see. I examine how the general idea of a perspective is a difficult metaphor to apply generally to knowledge, then examine more specifically how perspectivism has fared when applied to science.

1.4.1 Perspective as Metaphor

Perspectivism takes as a knowledge metaphor the case of perception, which is not a new approach to epistemic problems. Just as the approach is old, so is the tension in the thinking that underlies it, a tension we can find in the history of perspectivism. Conant (2005, 2006) has done some of this historical research in the context of Nietzsche's work. The latter took vision seriously as a metaphor for a philosophical view of knowledge, a metaphor that Giere has also adopted, albeit more superficially, as we will see below. Conant raises what I see as a challenge to thinking that perception provides guidance in thinking about non-perceptual epistemic problems: to determine where and to what extent the metaphor of a perspective provides an illumination of (human) knowledge. This challenge is deep and pernicious because the metaphor itself can, and often does, lead to a confused view of our (humanity's) epistemic situation, which it has in Giere's case.

However, I think it can be overcome, as it has in the context of Mitchell's integrative pluralism. The key is using the metaphor in specific contexts to resolve particular problems. The application and suitability of perspective-as-metaphor is, consequently, highly constrained to specific kinds of cases and does not provide an account of knowledge generally.

Perspectivism can easily provide a misleading picture of human knowledge. It is misleading because it entices us to suppose that it is possible to distinguish those contributions to human knowledge that come from the world and those which come from our faculties, perceptual or cognitive. That is, it suggests it is possible to stand apart from our epistemic context and make independent claims about it, even though we of course cannot set aside our faculties when we make those claims.

This picture is suggested by the appeal to a perspective. To say the world appears to be such-and-such *from this perspective*, is to make two claims. One is that the world would appear differently, if only we occupied a *different* perspective, or if we could only look at the world from another point. That is, there is the claim of plurality and thus of an alternative. The second claim is that there is something about being in one position rather than another—something about each perspective—that in some way affects what it is the world is like.

There is a logical problem to the second claim. How could our faculties, cognitive or perceptual, allow us to evaluate our faculties independently? This seems impossible because any evaluation we offer draws on our faculties, so how could we know what kind of evaluation we would give had we different faculties, faculties we cannot possess? It is not clear that our faculties could in principle give us the capacity to evaluate the world or ourselves independently of these faculties. This is the God's-eye-view problem.

The first claim, that of a genuine alternative, trades on the second. In thinking that a particular position offers a tainted view, one can make the small philosophical step toward thinking that there are other positions that are tainted differently and so by occupying different perspectives, we may come to infer more general claims about our cognition, which also supposes a strong distinction can always be maintained between what the world and our minds contribute to knowledge.

Thus far the discussion of perspectives is quite general and metaphorical. There are more precise formulations available. Conant (2005) discusses three and implies that all three are dubious. I will discuss what these formulations are and offer some reasons for thinking that, even if these philosophical positions are not stable positions, the metaphor of perspective is nevertheless helpful, but in specific cases. The three formulations of perspectivism as Conant describes them are, in decreasing realist commitment, (1) primary quality realism, (2) hidden world realism, and (3) pseudo-Kantianism.

(1) is the mild claim that some of the things we perceive are properties of objects (the primary qualities), while others are at least partly the product of our capacities as perceivers (the secondary qualities). This interpretation of perspectivism stays close to the original metaphor by taking the objects of perception, especially vision, as the subject of the perspective. This is a very restricted view because only some properties are the product of our perceptual system, many properties are not.

Slightly more distant from the original metaphor is (2), which claims that some perspectives, roughly equated with ways of knowing, are privileged because they are more accurate, true, or correspond more closely with reality. There is a sense in which this version of perspectivism is not fully stable because inferior perspectives should give way to those that are better. If perspectives can actually be ranked, then scientists (or knowers more generally) would tend to abandon the inferior perspectives in favour of those which are superior.

Least realist is (3), which Conant calls pseudo-Kantian because all human knowledge is in some way tainted or the product of our faculties, which precludes any definite knowledge of truth or mind-independent states of affairs. Crucially, this view assumes it is possible to posit mind-independent states of affairs while at the same time denying the possibility of cognizing those states. Giere's view falls squarely within this version of perspectivism because all knowledge is restricted to models and there is no way to evaluate or compare models, as we will see below.

The least committing versions of perspectivism (1) seem quite innocent, but there is a difficulty with pressing the metaphor of a perspective too far. With this pressing there emerges a tension between making a claim about knowledge generally, while at the same time denying that there is a privileged position from which one could make general claims about our epistemic position. Who could say whether knowledge is restricted or not about a mind-independent world? To make this claim, one would need to compare what we know with what the world is like and such a comparison is in principle ruled out when a strong perspectivist claims that knowledge falls short of telling us what the world is really like.

However, when used in a more restrictive sense, the perspectivist view may be insightful. Sometimes this insight has proved helpful in modelling. For example, it shows how acquiring knowledge requires appealing to very different methods. Similarly, there is scope for thinking that perspectivism may, when suitably restricted and articulated, provide insightful analysis of other areas of science that show some kind of plurality. In particular, it may help shed light on how we should think about disagreement and dramatic conceptual change. These topics, like modelling, also involve a kind of plurality, especially taxonomic plurality. Crucially, taxonomic plurality in these cases involve restricted cases where there are alternatives, i.e. where there are two or more taxonomic options, one has a choice of which to pursue, and it may not be obvious which taxonomic choice is best. Before exploring how I will approach this problem in 1.4.5, let's see how others have applied perspectives to science.

1.4.2 Modelling and Perspectives

Modelling, especially recent literature on perspectival modelling, promises to pave a middle road between realism and antirealism. Although there is this promise, some of the way's authors have developed the view fail to follow the middle road by encountering the difficulties associated with the perspectival metaphor, which I discussed in the preceding section, and with constructivism. I first examine why models are of philosophical interest and then how perspectival modelling tries, and often, but not always, fails to be the middle road.

Models are constructions: scientists build them. Once built, a model can be used for a variety of purposes, some of which may not be anticipated by those who built it (Morrison and Morgan 1999). Philosophers since the Semantic Turn have also supposed that models provide a mediating role between theories and more empirical elements of science (Suppes 1966). Science, by this view, does not aim to provide explanations for what we can observe, so called saving the phenomena—a thesis empiricists sometimes defend. Rather, science is concerned with building theories that are sets of representations, in the form of models. What is and is not observable drops out as a central feature of science, at least according to the semantic philosopher of science.

1.4.3 Perspectival Risks

This section has two tasks; first, to defend the idea that some modelling accounts, especially Giere's, presuppose the normative and conceptual aspects of data. In so doing, these accounts face similar issues to those facing constructivism. I will defend two points: 1) that Giere's framework gives no resources for talking about what data or other model inputs are; 2) modelling practices can be subject to some of the same criticisms that constructivists face. Because of these two points, Giere's account has an odd tension: on the one hand it is a form of unfounded empiricism, but on the other a form of strong constructivism. Teller's version of perspectivism has similar difficulties. The second task is to suggest that model perspectivism is innocuous, if we do not subscribe to model relativism. Mitchell's interpretation of perspectival modelling provides this kind of escape route (Mitchell in Massimi and McCoy 2019). Let's first examine Giere's account and the problems facing it.

Giere builds his perspectival account of science with observation as his starting point. Observation, he argues, is perspectival. Human vision provides an illustrative example (2006, chap. 2). He then extends the metaphor of vision to scientific instruments and theories (2006, chap. 3). The assumption here is that science is built up from information given to us by observation and instruments. And like vision, scientific models are sensitive to different stimuli and provide different "perspectives" on what the world is like. It is not possible to "see" the world or offer theories of it except through some particular and partial perspective. This is the 3rd and pseudo-Kantian version of perspectivism that Conant lays out.

Observation and instruments, so this story goes, are partial. They are only sensitive to a limited range of environmental stimulus. In the case of vision, our light receptors are only sensitive to a specific range of radiation. Because these receptors are partial, vision which depends upon these receptors is also partial. The same applies to scientific instruments. Different instruments are sensitive to different kinds of input from the world and are consequently partial in different ways. This partiality allows models to be representational, but at the same time very different. Two models can both represent the same target phenomenon, but maintain very different commitments.

Giere distinguishes two stages of scientific activity, a passive reception and an active model building stage. We can re-articulate these stages in realist terms. There is a mind-independent and worldly contribution that our receptors and instruments are sensitive to: this contribution comes from data. There is a mind-dependent element that takes the form of model construction. Model building is mind dependent because models are built for particular purposes using a partial selection of possible worldly inputs. Giere extends this to knowledge generally. Statements are true only in the context of particular perspectival representations (Giere 2006, 57–58) and truth does not generalize to the world, i.e. we cannot generalize a claim beyond the particular model in which it appears. This is the sense in which knowledge is perspectival. This is because the only access we have to the "world" is mediated by selective and constructed models and only in the context of a particular model can a claim be true or not.

This discussion shows that Giere's view assumes an unfounded empiricism, i.e. that he takes attempts to ground his account in mind-independent worldly contributions to knowledge, but in so doing takes for granted many other forms of knowledge that are necessary for his examples. Supposedly there is a worldly contribution to scientific modelling, but claims should only be understood in the context of models. How, then, are we supposed to know anything about the data that form the basis of any given model? Giere's own examples illustrate this problem. He discusses at length the functioning of Computer Assisted Tomography (CAT) (Giere 2006, 50–51), a type of X-ray scan that models the structure of the brain. During a CAT scan, an emitter positioned near a patient's head produces X-rays that pass through the brain before striking an X-ray detector. The intensity of the X-rays that strike the detector yield information about the structure of the brain tissue that the rays passed through. A computer then takes the information and builds a structural image of the brain. It is important for Giere's point that the image tells us something about the brain's structure because he then goes on to discuss another type of imaging method that captures brain function rather than brain structure.

This example, as Giere describes it, does not actually illustrate the perspectival view he is defending. It fails to do so because he omits critical information about the knowledge necessary for making CAT scans work. Scientists do not just build a machine that selectively represents part of the human brain, they build such a machine using an extensive body of knowledge, skill, and understanding of nuclear physics, mechanics, neurobiology, and many other fields. Knowledge is not restricted to what the model tells us. We have to know a lot about the world and about what data it is possible to collect; and we must know all this before we can even dream of building anything that captures the structure of the brain.

Now perhaps Giere could respond that the knowledge upon which CAT scans depend takes the form of other models. This would preserve the hierarchy of models approach. This response cannot be carried very far, however. Scientist need to know lot about X-rays in order to use them to create images of the brain. That is, they need to know something about the world and this knowledge is independent of any particular model and it is necessary to have this knowledge *before* data collection. One does not blindly collect data and only acquire knowledge after one has a model of the data.

This discussion suggests that models, and anything we learn from a model, is situated within a broader context of scientific knowledge, some of which is not constrained to particular models; understanding the function of a model is impossible without attending to this broader context. Because Giere takes this knowledge for granted and lumps it into a cursory contribution from the world, his view assumes an unfounded empiricism. It is unfounded because there is no justification for thinking that the world makes a contribution in the way he assumes, i.e. that there is an unmediated contribution in the form of data. Left unanswered are questions like: what are data? How do scientists choose which data to collect? How is a data model built from data? I will address some of these topics more fully in chapter 2.

Unfounded empiricism is not the only problem facing the hierarchy-of-models view and Giere's view in particular. There is also an element of strong constructivism in model thinking. This construction emerges from Giere's views on representation in particular, but could emerge in principle from any account that takes an agent-based view of representation without constraints.[10] Other views, such as isomorphism, may not face this problem, though they still face considerable difficulty on other fronts. Let's look at Giere's account for simplicity. His agent-based account of representation consists in the following:

> Agents (1) intend; (2) to use model, M; (3) to represent a part of the world, W; (4) for some purpose, P (2010, 274).

We can see that representation is a highly constructive process, and not only in the trivial sense that models must be built to exist and to be used. Human intentions play a constitutive role in a number of places, not only in model construction and use, but also in model evaluation. A model "works" or is in some way a good model if it satisfies our purposes. Parker (2010) also emphasizes this point about model evaluation.

This agent-based approach to modelling is not, on its own, a particular problem. Indeed this approach avoids several problems that other views towards representation have, views such as isomorphism or partial morphism because it stipulates why a particular model represents a particular target: because a scientist uses it as such to further some purpose. But it becomes a problem when the philosophical approach to science takes models to be constitutive of scientific practice and knowledge. Under those conditions, it is difficult to see how scientific knowledge could be anything other than constructed.

Consider once again the CAT scan and let's consider it using Giere's framework. By the agent-based representation view, we should positively evaluate the models it yields not based on the accuracy or truth of its representations, but on the basis of how well it furthers the agents ends, which in this case might be diagnosing certain brain diseases or brain damage. Now we might further wonder how it is possible for the CAT scan to yield such useful models. The explanation for this success can appeal to the model inputs, in

[10] These criticisms could in principle apply to inferential accounts—such as that which Suarez defends (2004; 2010)—or any account that is strongly intentional. Whether these criticisms are a problem will depend upon whether the account then relativizes knowledge to models. The following discussion on Giere's view illustrates this.

this case the information from the X-rays striking the detector. But Giere thinks the inferences we can draw from success stop there. We cannot, for instance, infer what the true structure of the brain is because all we have at our disposal is the structure of the model. Giere is explicit about this:

> To say a model is "true of" a particular real system in the world is to say no more than it "fits" that system or "applies to" that system (2006, 65).

As well as rejecting claims about truth, Giere's account also precludes comparing models directly with anything in the world. Models are compared with data models or other models (2006, 89). There is no direct comparison with data. But more generally, a model is only answerable to other models: there is no further empirical constraint. Does this seem correct?

It does not seem correct. The analysis is deficient in two ways, both concerned with mind independence. First, there is no possibility of making claims about the world, only claims relative to a model (a perspective). This is puzzling because one very minimal requirement of science is that it tell us about the world. Second, models can only be compared with one another, not directly with data. Consequently, we cannot make any inferences about what contribution the world is making to our models, or it is at least unclear how it would be possible to know anything about how the world is affecting our models if any evaluation we can give is internal to a model. And I take Giere to be committed to this relativistic idealism, as exemplified when he writes this:

> That scientific observational or theoretical claims should in general be relativized to a perspective is, if anything, easier to accept (Giere 2006, 82).

Without an ability to make claims about the worldly inputs, the hierarchy of model view, and Giere's perspectivism in particular, becomes a misleading description of science and exceedingly difficult to accept. Scientists, by only comparing models to other models, would not be engaged in an empirical enterprise; science would be reduced to a practice little different from that of comparing train engines. Although interesting, comparing human constructions is not the project of science.

Once again it is important to emphasize that modelling practices are situated in a broader scientific context and modelling cannot be solely constitutive of that context. If we take models to be solely constitutive of that context, there is very little work that empirical support provides and what we are left with is a highly constructivist view of science. It is worth comparing more directly strong constructivist commitments with Giere's perspectivism, which will show the striking similarities. I take the main commitment for the constructivist to be the following:

Constructivist commitment: There are different ways to conceptualize the world and there is no best conceptualization. What the world is like (ontology) depends upon how it is conceptualized. Because conceptualization is a contingent action, ontology is constructed.

Is Giere also committed to this view? Each model offers a different perspective on the world. Among other things, a perspective supplies a taxonomy. In order to be a form of construction, this taxonomy would have to be mind dependent and it would have to determine ontology. Does Giere's perspectivism do this? I think it does. Consider the following:

> The kinds are defined relative to the theory. Determining the empirical counterparts of theoretical kinds is another matter altogether. [...] What we count as being empirical members of the corresponding set of real systems depends on how good a match we require and in what respects. These judgments cannot help but be interest relative (Giere 2006, 87).

Giere claims quite clearly that kinds—qua the potential basis of a taxonomy—are relative to theory, which is for Giere just an abstract form of model. Because models and perspectives are equivalent, kinds depend upon perspectives. Taxonomy therefore depends upon perspective. Because models are built to fulfil specific purposes, it looks like taxonomy is also mind dependent and constructed, just as it is for the strong constructivist.

Does, however, this construction of taxonomy also mean ontology is constructed, which it must be if Giere's perspectivism is indeed a form of strong constructivism? Giere is not explicit. He does reject what he calls an objectivist metaphysics (2006, 81), i.e. a mind-independent structure that is the world. This certainly leaves room for a constructivist view. Whether it fully deserves this label may depend upon whether data or data models can provide a kind of worldly anchor that would preclude full mind-dependent ontology. I suggested above that data provide a very weak or non-existent anchor for Giere, so the connection between model perspectivism and strong constructivism is probably a powerful connection.

1.4.4 Perspectivism and Realism

Giere's view fails to find a middle path and, though it is a stronger attempt, Teller's characterization of perspectivism is also not in the middle. Teller attempts to bring perspectivism more directly in line with realism by rejecting some realist tenets and explicitly accepting others. His work suggests there could be stronger links between realism and perspectivism than Giere makes out. If such links do exist, however, Teller has not established them. Let's see which commitments he endorses, why, and how his view does not make strong advances over Giere's.

Teller rejects traditional scientific realism because it fails to accurately explain reference and existence, commitments 2 and 3 listed at the beginning of this chapter. He argues that reference cannot work the way the realist intends because to fix the reference of a term, one needs some kind of reference fixing tool to connect a term with its extension (Teller in Massimi and McCoy 2019, 51–52). However, Teller argues that the world is too complex for scientific terms to really work in this way. How does complexity interfere with reference? By supplying too many potential entities for the extension. For example, a simple realist semantic analysis of "atom" gives us tools for fixing the extension and, so the realist hopes, these tools give us a unique referent. So "atom" refers to all and only atoms. However, if we then use those tools, we do not get a unique referent, but too many referents, or so Teller contends.

If realist reference fails because the world is too complex, what kind of knowledge, if any, do we have, and what are we to make of the truth of scientific claims? Teller reaffirms the perception metaphor (*Ibid,* 57); we have inexact and partial knowledge afforded us by our imperfect representations. This is how we are to understand knowledge with an emphasis on context. For scientific claims to be true, Teller cannot appeal to the traditional realist views because he has rejected traditional realism about reference. That is, if scientific terms do not refer, then what makes them true is presumably not their referents. Instead, Teller argues that we think of truth as approximate and contextual. Claims are true or false within particular idealised and abstracted contexts given to us by a model or perspective. They are approximately true, i.e. true enough, depending on our purposes.

There are two worries I have with Teller's defence of perspectivism. First worry: how are we to know that our knowledge is inexact? To make such a claim, one would need to compare our state of knowledge with some less inexact variety. But such a comparison is impossible because, as a truism, the only knowledge we can have is human knowledge, i.e. what Teller calls inexact. An understanding or grasp of a more exact knowledge is beyond our ken and so a comparison between what we know and what the more exact knower knows is impossible.

Second worry: there is a truth gap. Teller wants to say that truth is context-sensitive and models or perspectives provide that context. But how then are we to generalise or extend what is true in a model to the world? This is a peculiar worry because Teller may (and Giere definitely will) want to reject idea that truth can be evaluated independently of a perspective. But their rhetoric does not support his rejection. They have set up a distinction between models (imperfect representations that we construct) and the world. They then proceed to contextualise claims to the models. If that is correct, then our knowledge is only knowledge of our imperfect, constructed representations. Where does the world factor into our knowledge? In what sense is human knowledge about the world, perfect, imperfect, inexact, or otherwise?

1.4.5 Perspectives and Integrative Pluralism

The general problem with Giere's and Teller's defence of perspectivism is a deep one, as I suggested in the brief discussion of Nietzsche and adopting the perception metaphor for knowledge generally. However, the problem does not suggest perspectivism cannot provide a useful tool for addressing epistemic and metaphysical problems in the philosophy of science. In particular, there is hope that perspectives can offer a way of thinking about disunity in science that is not relativistic and that does not prevent the fruitful exchange and relevance different elements of science enjoy. Mitchell has provided an exploration of perspectivism in the context of epistemic and integrative pluralism that avoids the pitfalls other brands of perspectivism face.

Mitchell defends a version of model perspectivism that is methodological (Mitchell in Massimi and McCoy 2019, 178). Like Giere, Mitchell takes perspectives to be intimately connect to models and what makes a model perspectival is its partiality: through abstraction and selection, a model only represents a target imperfectly and incompletely. However, this view is much less divisive, and it is much more sensitive to the empirical context in which scientists work. Mitchell is primarily resisting a classical view about ontological reduction in the sciences.

Her view is less divisive because there is the possibility of integrating the results from different models. Her (2003, 2002, 2009), and other works, explore the varying ways that biological inquiries use multiple models and approaches to investigate the same phenomena, such as social insect behaviour or depression. Crucially, the results of these different inquires can be integrated because different approaches select and abstract differently.

This account is more sensitive to the empirical context than Giere's because the evaluation of a model is not solely based on human interests. Rather, scientists are faced with a number of modelling options that vary in their levels of abstraction. Which model they use depends upon their interests and upon the inquiry, but the accuracy of any given model depends upon the world (2009, 115).

There are consequently two senses in which scientists evaluate models. As Mitchell writes: "empirical confirmation warrants correctness, while pragmatic concerns decide adequacy" (Mitchell in Massimi and McCoy 2019, 188). To be useful in developing our knowledge and understanding, models and perspectives need to be representations that scientists can integrate as well as use to inform and constrain one another. But to provide this function, models must be accurate and accuracy is not decided by pragmatics.[11] Knowledge, by this view, is not relativistic and fraught with idealism because perspectives are not the constraint on knowledge. Perspectives rather concern the methods whereby scientists develop and further our understanding of the world.

This view has two advantages. It approaches the perspectival element of science through the methods scientists use, which is perfectly sensible because the view concerns representations especially models, which are a part of scientific methods. Because of this emphasis on methods—i.e. on how scientists investigate phenomena—and not on what knowledge consists in, Mitchell's view is not antirealist in the way Giere's is. Second, Mitchell's view allows for models to be integrated. Claims are not, consequently, relativized to a model. Multiple models can be informative in different ways about different features of the same target phenomenon. There is extensive case study evidence for thinking models can be integrated in this way (see Mitchell 2002).

This discussion shows that a perspectivism restricted to particular parts of inquiry, such as Mitchell's discussion of modelling practices, can be insightful. But at the same time, when perspectives are used more generally to provide an account of knowledge, perspectivist views face challenges similar to those that constructivist views face. I showed this in the context of Giere's hierarchy of models account, but even divorced from modelling, perspectivism as a general view about knowledge faces difficulties, some of which we can see from a look at part of its history.

This discussion of perspectivism suggests two things. First, data have been neglected and must be addressed more directly. Without an account of data and with knowledge relativized to models, we have a strong anti-realist account of science. As those accounts stand, they do not provide a stronger middle ground compared with the

[11] Mitchell notes that not all models can be integrated, but there is the possibility of doing so in some contexts (2009, 117–18).

constructivist views discussed earlier. Second, despite these difficulties perspectivism can provide a fruitful way of thinking about unity and disunity in science.

1.5 The Promise of Perspectives

The rest of this thesis will develop, clarify, and defend a perspectival view of science. There are four parts to this task. First, I will develop a stronger and perspectival account of data that avoids unfounded empiricism. In doing so, I suggest data depend in a strong sense on the epistemic context. Second, I clarify what this context consists in and how it is a form of scientific pluralism. Third, I will illustrate this view through an analysis of a case from the history of chemistry. Fourth, I defend the view against two related positions with very different commitments. Linking these tasks together, and the basis of my appeal to perspectives, is taxonomy. In 1.5.1 I discuss why taxonomy, despite issues with construction, is a fruitful starting point. I then show how I will develop the four parts of my defence of perspectivism in the sciences (section 1.5.2).

1.5.1 Conceptual Taxonomy

Starting with taxonomy is not a new strategy for understanding realism. Dupré's promiscuous realism is primarily a taxonomic account and the same could be said of Chakravartty's sociability of kinds. There are, however, important differences both with the content and with the problems that need to be addressed. Dupré is primarily concerned with reduction, ontology, and natural kinds; his view is tailored accordingly. His pluralism is also primarily based upon the varying human interests associated with our inquiries. I, on the other hand, am interested in constraining the realist question of existence and I am not basing perspectival pluralism upon human interests so directly. I also mean taxonomy in a more general and abstract way than Dupré, who mostly discusses natural kinds in biology.

In what way does taxonomy help us address realist questions? Like Teller, I will accept and reject some of the realist tenets listed at the beginning of this section 1.1. Tenet 2, semantics, is likely to be in conflict, depending on how we interpret it. If we follow van Fraassen and insist just in the literal interpretation of theories, then this is fine. If we go further and think 2 should be about bivalence and correspondence theories of truth, this will likely conflict with the view I will defend. The conflict is likely to arise because of the inflexibility a strong reading of 2 ascribes to scientific terms. The mastery of taxonomies in the sciences is not easily understood in terms of correspondence and the meaning of terms change in ways that fit unnaturally this strong reading.

Tenet 3, the epistemic commitment to truth or partial truth of scientific theories, should not be problem. It could be if we read a strong metaphysics into truth, but this is not necessary. We can just read 3 as the claim that what the world is like is not determined by our cognition, i.e. the world is resistant to our interests, desires, construction, and classificatory practices.

Tenet 1 is not straightforward. The general attitude I defend is that 1 does not arise in the way realists intend, with the result that both realists and antirealists who endorse or contest this commitment are wrong to do so in many cases. In essence, I will suggest a worry about existence, which is the concern of 1, is a misplaced worry because it can be understood instead as a problem about identity, not existence. I am in agreement with realists to the extent that this commitment cannot be straightforwardly rejected, but also in agreement with antirealists to the extent that it also cannot be straightforwardly endorsed. An examination of conceptual taxonomy allows for this more nuanced attitude toward 1. My view is essentially in line with Hacking and especially Cartwright as I described their realist commitments in section 1.1.

This nuanced attitude toward realism raises the question: does this perspectivist view count as realist? Typically, to be realist, one would need to be committed to all three realist commitments. The perspectivist view I will defend does not do this—though it does share some of the realist's commitments—and so whether it can be legitimately considered a form of realism is open to question. There are a number of different ways perspectivism can be cached out, some of which I discuss later in this chapter. The realist vein in these views is as broken and varied as the views themselves.

However, I contend the perspectivist view, as I will defend it, can be thought of as realist to the extent it is committed to 3 and to the extent it does not offer an outright rejection of 1, but it is antirealist to the extent that it rejects 2 and does not accept 1. I presume that this straddling of realist and antirealist commitments is sufficient for calling perspectivism a middle ground. However, I'm not sure much hangs on whether perspectivism can legitimately be called a form of scientific realism, provided the view is defensible and gives insight into philosophical problems associated with scientific disagreement. I believe it satisfies both these requirements; perspectivism is compatible with an optimistic view of scientific progress and with a serious commitment to the products of science.

1.5.2 The Thesis Structure

My view will emerge over the next three chapters. I will show in chapter 2 how empirical evidence, data, are actually unable to provide the kind of unequivocal empirical arbitration we might expect when we face taxonomic problems, i.e. conceptual problems. We find this difficulty with evidence because data actually depend upon the conceptual taxonomy in which they feature. I argue data sit mid-way between traditional representational accounts which are strongly empiricist (such as Bogen and Woodward's (1988) account), and more mind-dependent relational accounts, most recently defended by Leonelli (2016). Data are representational, I will argue, but still depend upon the epistemic context because they are the product of selective collection and in choosing what to collect, scientists conform to a conceptual taxonomy.

Chapter 3 explores in what way the perspectivist account I defend is pluralist and what it means to use a conceptual taxonomy. It does so by showing the connection between scientific practice and taxonomy. One common approach to epistemic pluralism is to be pluralist about practice and to distinguish practices by epistemic aims. Drawing on a Wittgensteinian approach to practices, I argue that epistemic engagement, rather than epistemic aims, should be the basis for distinguishing practices. The question then arises what preconditions are necessary for epistemic engagement? The answer, I will argue, is a conceptual taxonomy. I clarify what such a taxonomy consists in. This is a crucial clarification because there is a danger of falling into conceptual relativity, as Giere does.

There are two reasons my account is not best characterized as conceptual relativity. First and most importantly, I do not mean taxonomy in a rigid sense. A taxonomy is rigid if it consists in a finite set of terms with definitions. I think this is Kuhn's view of lexicons, for example (1990). The difficulty with this kind of taxonomy is that it suggests a taxonomy has autonomy and isolation from other taxonomies. One can just switch from one taxonomy to another by merely replacing one finite list with another. This is a very different kind of taxonomy from the one I am interested in. A conceptual taxonomy is better characterized as fluid with unfixed edges that only become sharp and distinct in cases where there is conceptual disagreement or abrupt change. The consequence of this is that one cannot just replace an entire taxonomy with another because it is not a finite set that can be replaced with another finite set.

The second reason this kind of taxonomy does not fall into conceptual relativity concerns epistemic standards. These standards provide norms for evaluating taxonomic choices and are not fully internal to the conceptual context in which scientists work. Standards as I define them follow a middle path between rigid determiners of rational choice (Boghossian 2007) and scepticism of standards (Kusch 2004).

In chapter 4 I illustrate the perspectival view by examining a classic case of conceptual disagreement and theoretical change: The Chemical Revolution. A perspectival analysis is in tension with traditional views. A traditional view of the Chemical Revolution treats it as a period of, unsurprisingly, revolution, specifically theoretical revolution: oxygen theory replaced phlogiston theory. One of the main issues at stake, so the traditional account goes, is the existence of a theoretical substance, phlogiston. This view struggles to make sense of continuity and also struggles to make sense of the appeal phlogiston theory may have had. The perspectival analysis has traction with both of these problems. I argue what really was at stake was a taxonomic problem: how to classify several different reactions and substances. This analysis fits quite naturally with several historical experiments and shows greater continuity, while at the same time accommodating and specifying the intuition that there was indeed dramatic change during this period. The dramatic change was conceptual, and therefore taxonomic, change. This analysis strikes a balance between thinking of science as unified and thinking of science as disunified, hence delivering on the promise of perspectivism I suggested was possible in my discussion of Mitchell's views.

Chapter 5 brings out explicitly the differences between the perspectival view I defend and two other views that a) philosophers have used to try and understand the Chemical Revolution and that b) strive to address some of the same problems perspectivism does. The two views are a form of scientific pragmatism and relativism. The main problem with the other accounts is that they are too divisive: they place too much emphasis on disunity by allocating disagreeing scientists to different epistemic contexts and in so doing, they fail to account for the fact those who disagree have a lot of shared epistemic background. The challenge with conceptual change and disagreement is offering a characterization that brings those who disagree close enough together epistemically so that they can disagree, but not so close that there is no basis for disagreeing. Perspectivism strikes this balance better.

1.6 Conclusion

This chapter has laid some of the groundwork for a perspectival view of scientific disagreement. It has done so by showing how traditional strong realist views run into trouble with accounting for the context in which scientists work and in which theories are developed and used. Constructivist views of science, in resisting these issues with realism, have run into a different kind of problem: accounting for the empirical nature of scientific enterprises, a constraint realist views tend to accommodate. Taxonomic pluralism has been one strategy for resisting the lack of context realism provides while also avoiding metaphysical antirealism. Some such views struggle to separate themselves from constructivists.

This is part of the promise of perspectivism: to provide an account of science that is sensitive to those constraints and in so doing to offer a middle ground. However, some of the existing characterizations of perspectivism take the view to apply too broadly, in the process running into some of the same problems constructivism faces. I suggested a more constrained version, restricted to disagreements and conceptual change, might offer more promise. I laid out the view I will defend, which also makes appeal to taxonomy, but by also using perspectivism, it strikes a balance between realism and antirealism and between emphasizing unity and emphasizing disunity in the sciences. In so doing, perspectivism specifies and constrains realism.

2
Data and Perspectives

Abstract
This chapter defends a novel perspectival account of data that overcomes issues facing two existing views. It aims to specify what data are and how it is possible for them to be evidentially useful, issues Giere's perspectivism does not address. Those existing views of data, the relational and representational views, also do not fully capture what data are and how they function in science. The representational view is insensitive to the scientific context in which data are used. The relational account does not fully account for the empirical nature of data and how it is possible for data to be evidentially useful. The account I defend here surmounts these problems by accommodating a representational element to data. At the same time, data depend upon the epistemic context because they are the product of situated and informed judgements. These judgements emerge from within conceptual taxonomies. By explicating the connection between data and human judgement, this chapter sets the scene for a more detailed examination of the epistemic context, of judgement, and of taxonomy in chapter 3.

2.0 Introduction

What are data? It is important that my account start with this question because data are closely associated with empirical evidence. Understanding in what way science is an empirical inquiry—and the limits of that empiricism—therefore requires starting with an examination of data and of evidence.

There are two main answers to the question of what data are. One influential answer, first defended by Bogen and Woodward (Bogen and Woodward 1988), is that data are representational. They represent in virtue of being records produced by reliable experiments. Data provide empirical evidence and, as such, are mind-independent and free from theoretical assumptions. They are also stable, meaning their identity does not change even if other features of an epistemic context change, including theoretical and conceptual change.

Another answer, more recently defended, is that data are relational (Leonelli 2016). Data are defined principally by their use as evidence. Consequently data identity depends upon the particular inquiry in which they feature and their identity changes as the inquiry changes. Different inquiries put different demands upon data and, to meet these demands, data change identity. Because these demands are many and varied, data identity changes often and data are very unstable.

Given these two seemingly incompatible options, how should we define data? Do data have a changing identity and, if so, what precipitates identity change? In trying to clarify the role of data in science, this chapter will strike a middle option between the representational and relational accounts. I will call this third view a perspectival account. I defend two claims: (1) that data identity is much more stable than the relational account allows because data are representational; and (2) data identity is not completely stable because data are relative to the epistemic context, in a way I will clarify. This dependence can be helpfully understood by appeal to perspectivism. This account will help develop what it means for data to be representational and consequently how data are both empirical constraints, but also with an identity that depends upon the epistemic context. With these results in place, we will avoid the kind of problematic empiricism to which perspectival accounts like Giere's are susceptible.

My strategy for defending the perspectival account has two main parts. In section 2.1 I discuss the relational account of data and why it provides a foil to explicating a view with stronger representational commitments in section 2.2. To develop these commitments, I argue in 2.2.1 that, despite this representational element, data still do depend on the epistemic context. Section 2.2.2 clarifies this dependence by introducing the notion of a perspective and section 2.2.3 illustrates the view with an example. Section 2.3 concludes this chapter.

2.1 Representational and Relational Accounts of Data

The representational view of data (Latour 1999; Rheinberger 2011; Bogen and Woodward 1988; Bogen and Woodword 2003) is the view that knowledge claims are grounded on a largely theory-free and empirical contribution from data. Scientists record data and, once recorded, the data serve as unchanging evidence against which theories and models can be compared, or upon which theories and models are built. Data serve as the empirical arbitrator by being evidence that supports more theoretical claims, though the data themselves are theory-free.[12] In using data in this way, scientists can justify claims about phenomena, phenomena such as the melting point of lead, neutrinos, black holes, aggressive behaviour, or evolutionary traits (Bogen and Woodward 1988).

Leonelli (2009, 2016, 2015) rejects the representational view and defends a relational account. This account claims data are material artefacts whose identity is determined by their evidential use. After describing how this view is motivated, I will argue that accounts that do not treat data as at least partly representational—such as relational accounts—face two issues: the problem of circularity and the problem of data stability.

Leonelli has documented how data travel and how they are used evidentially. She argues that these two considerations require substantive identity changes in data (Leonelli, 2012, 2009, 2013, 2016a, 2016b). Here is an excerpt from Leonelli's work where she discusses this change in identity, couched in terms of stability:

> What I do not share [with the representational view of data] is the emphasis on stability. When traveling from their original context of production to a database, and from there to a new context of inquiry, biological data are anything but stable objects. (Leonelli 2016, p. 5).

Data must be transported to be used: they are not used right at the time and location of collection. To make data suitable for movement and use, they must be formatted, classified, organized with meta-data, and filed for later use. Scientists make these material changes so the data can be put to new and different evidential uses. Leonelli also writes:

> Within this framework, it is meaningless to ask what objects count as data in the abstract, because data are defined in terms of their function within specific processes of inquiry (*Ibid*. p. 7).

This passage suggests that data are defined by their use, or the "role they are made to play" (Leonelli 2016, 78). Leonelli's account is relational because it rejects the consideration of a datum independently of the context in which it is used. Consequently, what are data to one scientist in one context of inquiry may be different data to a scientist in a different context. This is the particular feature of relational accounts, namely that data are "defined in terms of their function within specific processes of inquiry" that I want to critically engage with in the rest of this chapter and somehow mitigate by arguing that the representational view might in fact have some important insights, even though we do not want to reject the importance of examining data within the epistemic contexts in which

[12] See, for a well-articulated view of this position, Bogen and Woodward (2003).

they are used. Let us then consider in more detail what data are according to Leonelli's account:

> [...] any object can be considered as a datum as long as (1) it is treated as potential evidence for one or more claims about phenomena, and (2) it is possible to circulate it among individuals" (2015, 2).

A set of records are data when those objects function as evidence and when the set can be transported. Data are defined by their functioning as potential evidence *and* in virtue of their physical form. Because data are defined by these two criteria, a set of data does not merely acquire or lose properties or characteristics when its use or form changes, the set of data actually ceases to be data or becomes different data. *The identity of the data set has changed.* Data, this view seems to suggest, are unstable because what data are depend on purposes of specific agents that want to make evidential claims about some phenomena. As soon as different epistemic communities have different purposes in mind, what used to be a data set might no longer count as such.

My purpose in what follows is to address the problem of data instability by suggesting that relational accounts leave room for a further discussion concerning what data are in addition to their evidential role; that other role is representational. There are two issues to address concerning the relational account:

(I) Is use-as-evidence sufficient in identifying data? and
(II) Is the materiality of the data important in establishing identity?

I here suggest (I) is problematic for two reasons. First, data are not the only source of potential evidence in science. A model or simulation, for example, might provide evidence that a hurricane will strike a particular place at a particular time, but both these forms of scientific evidence are distinct from data. This suggests data are not the *only* evidence. How then should they be distinguished from other forms of scientific evidence (say simulated evidence)? Or, differently put, can simulated evidence count as evidence under the relational account? And if so, is not there a risk that a net with mesh too large is used to capture what count as data?

Second, it is unclear how data can be used as potential evidence. To use data as evidence seems straightforward, but "potential evidence" is presumably not a use because any such use would just be a straightforward case of using data as evidence. This suggests potential use is more like an attitude that scientists have toward a set. If it is this attitude that defines data, then we have what I am going to call the problem of circularity: scientists expect data to be evidence; and those data are data because scientists treat them as potential evidence. *But why think they can be used as evidence at all?* The answer to this question requires thinking of data as more than just evidence or potential evidence. And because of the circularity, a relational account must just take it as given that data are potential evidence.

These two issues show that data should not be defined solely in terms of evidence: something further must be said about what data are if we are to understand how they can be used. My point here is not that data *cannot* be evidence, but that in order to address why data can function evidentially, there is a logically prior question about what they are.

But perhaps this is too hasty. Leonelli may have anticipated some of these objections and may have more of a story to tell about what defines data. For she compares data to biological individuals, which also have some kind of identity through continuous change over time, like a succession of states (2016, 82). This comparison preserves the intuition that something about data does persist across time and space, even though what persists continuously changes. If this is right, then data can be connected to their collection and to previous uses, giving scientists motivation for treating them as potential evidence, while at the same time preserving a relational account.

This analogy only works loosely and it only works if we consider the materiality of data important for their identity. I will briefly discuss why it should not satisfy us. First, biological individuals may be too unlike data. We might think, for instance, that reproduction, evolutionary relationships, birth, and death are important determinants of biological individuality. These considerations are not, however, relevant for data. Second, biological individuals, to the extent they change, do so materially; this is what the analogy hangs on. I am sceptical that materiality is important for data identity.

One reason for thinking the material change in data can be overplayed stems from the high level of stability data often have. This is *the problem of instability*: data must have some stability to function as evidence. This is exemplified in cases where changes in data form are symmetric: for example, cases where we can take digital information and write it down on a data sheet, then enter that information on a computer, thus getting the same material object that we started with. Transformation that allows for a return to the original form in this way does not seem very substantive since data can often be moved and transformed without *loss of information*. If the information were lost, then change back to the original would not be possible.

The requirement that data be stable is also exemplified when data are moved and transformed, especially when scientists critique or respond to one another. Consider as an example the historical and philosophical work that Allan Franklin (1981) conducted on the electron's charge, building on Millikan's oil drop experiment. In the early 20th century, Robert Millikan was interested in precisely measuring the charge on an electron and famously measured the falling rate of electrically charged oil drops to do so. Based on how fast the drops fell, he was able to calculate the size of an electron charge. He did not, however, publish all his data, only a selection. Franklin (*Ibid.*) revisited Millikan's notebooks to see, among other things, how Millikan's conclusion might differ if all data were included in the calculations. This is where my point about transformation arises. Millikan recorded his data in a notebook using a pencil. His notebooks were later photographed and the photographs were stored as microfilm in the Millikan Collection at the California Institute of Technology. Franklin obtained digital copies of these microfilms (or at least some versions of his published paper used digitized versions of the microfilms). It seems to me that the data underwent extensive transformations before Franklin could verify Millikan's results. They began as pencil parks in a notebook and ended as bytes on a hard drive several decades later. Despite these extensive transformations, it would be odd to say that Franklin was *not* working with Millikan's data; the entire purpose of Franklin's work was to re-examine the data to determine whether some data points that Millikan omitted affected the results. This suggests that, despite some great material and contextual changes, Millikan's data did not change. Leonelli's account of data has an insight here by noting the significance of the form data must take to use them in certain ways. However, it would seem that the identity of the data (in terms of their informational content) persists through this material change. Specifically, the persisting information, in the Millikan case, is the record of the following:

> The notebooks contain observations on 175 drops along with voltage and chronoscope corrections and measurements of the density of clock oil (Franklin 1981, 187).

The notebooks contained information about the charge (voltage) on each drop as well as time corrections and the density of the oil. They are the records of the measurements and observations that Millikan made and consisted, in this case, of a table of numbers with labelled columns. This information seems to me the same regardless of whether it is in a notebook, Franklin's hard drive, or transcribed from my own computer to my notebook. I take it a relational account is committed to the idea that the data on the hard drive are different from the data in the notebook and this is puzzling.

These material changes may seem too trivial to worry the relational account, but they are actually quite substantial. Once microfilmed, Millikan's notebooks were archived with other material from his life and curated. There is even a published guide to assist the researcher in navigating the microfilm archive (Goodstein, Gunns, and Underleak 1977). Extracting data from these notebooks was no easy task.

A relational view of data might want to give due consideration to elements of data practices, such as those illustrated by this electron charge example. Indeed, any account should do this and the view I defend in section 3 seeks to accommodate this consideration. But an account must also be able to explain how different scientists across time with different interests and using different tools could nonetheless study the same phenomenon and I do not believe relational accounts have an explanation for this as yet. Franklin and Millikan were both interested in the same oil drops and the same charges on those oil drops. Their research interests were slightly different: Millikan wanted to calculate the charge of an electron and Franklin wanted to determine whether Millikan made no important omissions. Both of these different research interests required the same data set. It is difficult to make sense of how they could have their respective research interests and pursue them, unless they were working with the same data. This case suggests that the data, the records that began in a notebook, provided a link between two scientifically-minded researchers and a set of oil drops. This conclusion gets us out of the problem of stability: data are stable enough to support a variety of research interests and this stability stems from the fact that they are records, not just evidence.

Relational accounts do have resources to discuss the relationship between data and the world. One possible strategy may be to use meta-data. Leonelli discusses (2016, 189–90) the importance of curatorial work in packaging data. Such work involves recording what kind of experiment and recording techniques were used in producing the data. Such meta-data are important for communicating how data were collected, what instruments were used, who made the record, and under what conditions, etc. So perhaps meta-data can explain why scientists should expect data to serve as evidence.

Without denying the importance of meta-data, one might still worry about how meta-data make a set of data *that set* and different from another set. If I read Leonelli correctly, meta-data are primarily important for evidential, but not identity, reasons. By allotting data-collection and experiment to meta-data, this relational account suggests data identity is not *primarily* affected by how data are produced. Assigning meta-data this secondary role is reasonable; after all, meta-data must be recorded in addition to recording data, but I contend that a set of data would have a life of its own even if one neglected to record the meta-data (though they may be evidentially not useful).

I have argued that the ontology and use of a data set leave open questions about data identity. I noted that data must have some sort of existence and identity prior to their use of evidence and, therefore, use-as-evidence might not be enough to determine identity.

This leaves room, I believe, for a treatment of what data are *qua* data, independent of evidence. The following section suggests some additional resources from representational accounts could provide a stronger account of data.

2.2 Data Records

The starting point for this chapter's account of data is this: data should be defined in terms of records. The records in question can be of events, objects, behaviours, processes, any number of things that have been detected. Scientists, we might say, use data to record, share, and carry specific details about a part of the world. By being used in this way, data provide a means for scientists to work with a great deal of empirical content that would be impossible to aggregate otherwise. In this section, I will suggest that the function of data to record empirical information is of primary importance because, without this, they lack even the potential to function as evidence.

Data-as-records is illustrated by an example from Bogen and Woodward. I can collect data on the melting point of lead by melting a lead sample and observing my thermometer reading when the lead melts (Bogen and Woodward 1988). This is an observation. If I do this repeatedly, I have a set of observations of at what temperature lead samples melt. If I write down the temperature associated with each observation, then my written records are a set of data—my data are a record of the results of my experiment. It is necessary that each observation be an observation of the same type of event or type of object. Sometimes the records can be written down, but at other times they may consist in, to use Strasser's example (2012), collections of, say, plants, which we might think of as records of a species. What form data records take is variable, but an essential element, and a defining criterion for a data set, is what they are records of. We can think of the content of the records as empirical information.

There are a number of accounts of empirical information that could be compatible with this view of data, perhaps with little modification. What I have in mind in particular is information as the representation of an entity, as defined by van Fraassen (2008, 179–80) and therefore as bearing empirical content. This representation connects the data to the worldly entity; in the case of data it is a recorded measurement or observation result. This is all I mean by data carrying information. I do not mean information as signal and noise, as in Shannon's sense (1948).[13] It cannot be merely signal

[13] The Information Shannon discusses is relevant to data, as Woodward notes (2010), but it is too basic and fundamental a type of information to be evidentially useful and so it cannot be data. It is possible that a more complete account could take this basic form of information and supplement it to create data, but this project is beyond the scope of this chapter.

and noise because this combination provides no specification of what is recorded: signal and noise are too primitive a form of information, too bare, to be evidentially useful without a richer understanding of what a recorded observation is an observation of.

Representation in this case is not friendly to a strongly naturalized account of representation, such as isomorphism (French 2003, 2014), because data depend upon a choice of what to record, what observations data represent cannot be specified except by appeal to scientific practice. I will discuss this dependence that data have later in this section. But the kind of representation needed here is in principle compatible with, say, an inferentialist account (Suarez 2010; Suárez 2004) or even an agent-based account (Giere 2010). Whether these accounts are compatible may depend upon how content is characterized, which is beyond the scope of this chapter. But what this chapter will help clarify is the way in which data, as records that are representational, must still involve judgement. Before explicating this in more detail, I show how this view avoids the problems facing representational accounts *and* the criticisms that Leonelli levels at traditional representational views of data.

Recall that relational accounts faced circularity and stability problems. Treating data as records avoids both. The first problem is avoided because this definition is non-circular. It is clear why a scientist would collect data: because they are records of worldly events against which theories or models can be tested and which stores information. And if this is what data are, then it is no longer mysterious why scientists would find them worth collecting. The issue of stability is avoided because data remain stable to the extent that records are stable. However, this treatment may bring to mind the representational view that Leonelli criticises (2016, 73–74). The representational view is committed to two things: (1) that data are mind-independent representations when reliably produced by the scientific method and (2) that the exclusive role of data is to test theory. Leonelli argues that data cannot be defined in this way. I am not attempting to defend a representational view with these two commitments. Like Leonelli, I also reject data as mind-independent representations of the world that are stable. This may seem odd and counter to the entire point of developing a representational account. However, there is sound reasoning behind this claim.

Thinking data are tied to an epistemic context will hopefully seem less odd when we examine what is involved in using data as records. I contend that this use invariably requires human judgement. And if there is judgement involved, there is reason for thinking that data depend upon the epistemic context in which they feature. This appeal to judgement is necessary because it is otherwise impossible to establish which observations qualify for a data set; in other words, we must specify how to choose what to record and how to group the records.

This specification is not trivial to address. Any given experiment can present to a researcher any number of things to record and only through judgement is it possible to extract data from an experiment.[14] For instance, it is usually not the case that scientists would record whether they wore a watch, the size of the lab bench, what was for lunch, or the colour of their lab coats. It is impossible to record everything and scientists must make deliberate, informed choices about what is and is not important information. Those choices are informed by knowledge and understanding of, for example, theory, experimental techniques, or instruments. To return to a previous illustration, when melting lead, one might record the temperature because atomic theory predicts at what temperature lead melts. It is therefore an interesting study to compare what theory predicts with the world, which is done best, in this case, by comparing the actual temperature at which lead melts with what theory predicted. To do this, I need to know how to identify lead, how to melt it, how to measure its temperature as it melts, and how to read and record the reading on a thermometer.

In being able to judge what to record, scientists must be able to determine when two events are the same. If this were not the case, it would be unclear which data would belong in which set. There are criteria for justifying the treatment of two events as the same. Criteria, in this sense, are related to justification. To return to the lead example, I can justify using two measurements when my samples are of similarly pure lead that I heat using the same burner and measure using an accurate thermometer. Justification is important because scientists must establish that their data sets of records of the same thing. This is particularly salient a problem in the case of unusual readings. Unusual readings suggest something has gone wrong. Say I have an outlier, a number that is much farther from the average temperature reading than my other recordings. If someone challenges me on this number, I need to be able to justify its inclusion and I do so by appealing to criteria that establish the outlier is the same type of event as the other recordings. I need to be able to say things like "I used the same thermometer and the same lead sample under the same conditions." Without this kind of justification, there is reason to doubt that my outlier can be included in my data set. And with this doubt comes uncertainty about what the data are records of because to make a record, one must know what it is one is recording.

[14] In this sense an experiment produced any number of signals and noise. This is why Shannon's information is insufficient: scientists must choose and judge what to record, without this specification, there is simply no way to determine which signal and noise is relevant.

I have suggested the identity of data depends at least partly on their empirical information: the product of what data-collectors judge to record or what data-users judge their data are records. By defining data in terms of records, I am more closely connecting the concept of data to the that of measurement. My view in particular is related to the idea of measurement as information gathering and might be compatible with Tal's (2014) or Parker's (2015) account.

Considering data collection as measurement or information gathering makes more explicit the role of judgement in collection. What we measure or what information we gather is a selective choice; we do not record and measure all possible parameters or information, just a subset. Furthermore and more importantly for my discussion, in judging what to record, those who collect data distinguish the phenomenon of interest from others that are irrelevant for their purposes. Millikan had to be able to distinguish falling, charged oil drops from all other phenomena surrounding his experiment, including oil drops that were not charged. I am considering this selective recording a judgement because Millikan could have chosen to record other things (though that information would have been useless for his study) and because he required extensive knowledge about electrons, oil drops, and his instruments in order to make the kinds of records he did.

As mentioned earlier, the account defended here shares with Woodward (2010, 792) the emphasis on the connection between data and information. However, there are two reasons for thinking there are signification differences between these two accounts.

The first is that I emphasize the dependence of the data upon the judgements scientists make when selecting what to record. This is a very different claim from the one I take Woodward to defend, which is that data depend upon experiment outcomes, but not judgements. A more congenial position on data comes from philosophy of experiment. Hacking's (Hacking in Pickering 1992, chap. 2) description of experimental practice as human-dependent activity, but with some realist commitment, provides a particularly important backdrop to this project, though his writings on data may be notably different.[15]

The second reason my discussion is dissimilar from Woodward's and more similar to Leonelli's is that I assign a more explicit role to the epistemic context. To the extent that judgements form part of this context, data—as products of those judgements—are also part of that context. Woodward assigns little role to the epistemic context. He suggests in his (2010, 796–98) that data do not depend upon theory. And further discussion he offers

[15] More strongly constructivist views about experimental work, such as those that Pickering (1992) or Latour and Woolgar (2013) defend are less closely aligned with the perspectival account I wish to defend. However, their work on practice, activity, and experiment provide important foundations for this work.

of the epistemic context is restricted to either what motivates scientists or to what helps scientists make data-to-phenomena inferences. There is no discussion of the role that the epistemic context plays in shaping and determining the data themselves, apart from motivating scientists to collect them.

It is possible that Woodward's view is more congenial than the preceding paragraphs suggest. But these two points of divergence between the account I defend and Woodward's are significant because the former has a much milder empiricist commitment: data, as the empirical contribution to science, depend much more on the scientists and the context in which they work than Woodward suggests.

2.2.1 Data and the Epistemic Context

So far I have argued that relational accounts leave room for more discussion of data-as-records (section 2). I then argued that data should be defined in terms of records (section 3a). I now would like to suggest that because data are part of an epistemic context, they are unstable in the sense that as the epistemic context changes, the data can change as well.

There are two motivations for endorsing this view of data as unstable. One is that what scientists select and record can change based on changes in epistemic context—and so the data change—and the other is that a set of records can turn out to be records of two different things: in both cases the data change because the content of the data, the empirical information they carry, does not reflect the kinds of judgements that scientists would like to make. I discuss each motivation in turn.

Two records can only be a part of the same set if they both are records of the same type of observation. Think of data sets as sets of individual records of the same general type of observation. If data were not records of the same kind of observation or measurement, the data could not be used as records or evidence. I cannot, for instance, use data to reason about the melting point of lead if one datum is a record of melting lead and another of melting iron. To reason about the melting point of lead, I require a set of data with an internal coherence and this is formed when all members of the set are records of the same type of observation or measurement. Even an individual scientist often works with a great variety of data sets, each a collection of data that are the products of different measurement outcomes.

The reason data are not fully stable is that the information they carry is subject to change in the face of certain epistemic changes. In particular, if scientists come to make new distinctions about what they are recording, then the information data carry is not straightforwardly preserved. What scientists took their data to reflect at one point in time differs, in such cases, from what it does at a later time.

Historical chemistry provides an illustration of this point. Over the course of several experiments, Henry Cavendish (1766) thought he had isolated and weighed phlogiston, a substance once posited to form acids, metals, and combustion. His data were numerical records of the weight of the phlogiston. It was reasonable of him to make this assertion because phlogiston was a widely accepted substance and because many chemists, including Cavendish, distinguished the products of a reaction on the basis of what could be isolated and the gas he called phlogiston was one such substance. Chemists today use other criteria such as weight as the basis for distinguishing the products of a reaction. The changes in data that I have been discussing occur because of the change in classification. We no longer say Cavendish had records of the weight of phlogiston, but that he had collected data on the weight of hydrogen gas. The reason the data here are not stable is that the information we take them to carry changes. The data do not stand as records of the measured weight of phlogiston, but as records of the measured weight of hydrogen gas. When chemists accepted phlogiston chemistry, these data were records of phlogiston weight. But today in our epistemic context, the data are records of hydrogen gas. The claim that data identity changes may seem deeply unintuitive, but the rest of the chapter will show that this need not be so.

As the first step toward motivating the instability of data identity, consider what the alternative is here. It may be more intuitive to think the identity of the data identity does not change here. The marks on paper or something else about what Cavendish collected in the 18th century must in some way be the same as it is today. However, the numbers only become data in conjunction with the information they carry, so although the marks do not change, the identity of the data do. We need to know what our data are records of in order to determine what kind of data we have. Once we attend to the fact that data carry information, it is less intuitive that data cannot change. And the change is only partial. The marks on paper, the description of the experiment, and the published papers are unchanged. But our epistemic context, and thus the identity of our data, has changed.

The second reason for thinking data are not fully stable is related. We might discover that records we took to be of the same thing were actually records of two distinct things. So not only is the event or phenomenon the data were records of different, but also the data are not records of one type of thing. We can say that data sets of this sort are very heterogeneous. This is a case of only partial stability because the description of what data are records of changes in response to things we have learned. The new data cannot be used for the same generalizations, they do not support the same claims, and we do not take them to be records of the same events we did before. This last consideration is a direct consequence of defining data in terms of what they are records of. There are, in other words, strong inferentialist reasons for thinking the data are not the same.[16]

These two features of instability differ because, regarding the first, the data are all subject to the same generalizations, but the generalizations change, whereas—for the second feature—generalizations do not hold across the data set, though they may hold for a subset.[17] What is common to each of these types of change is that the change destabilizes what we take the data to be records of—and hence the data are not stable since data are identified in terms of empirical information. We can see this change because the empirical information carried by data is determined by the judgments that scientists make about what they are selecting to record. More specifically, the change here is not token change. For example, a particular number inscribed in Millikan's or Cavendish's notebook is always going to be that number, regardless of where it is and how it is recorded. However, what that number stands for is not necessarily stable in the face of all types of scientific change.

Data instability of the kind discussed above is in tension with Bogen and Woodward's commitment to establishing reliable data experimentally. As discussed above, their account suggests data are largely independent of theory and that data can therefore serve to arbitrate between different theoretical claims. In other words, they rely on a *reliabilist epistemology* to grant data autonomy from theory. However, what those data are and how they can be used are not independent of scientific practice, including theory, because data depend upon judgements that scientists make and these judgements are not made in isolation of theory. This is independent of how reliable we take the data-producing method to be. In coming to better understand what we observe, we may, and have, come to recognize important distinctions that undermine what we took to be the subject of our investigations and the way we carried out those investigations.

The instability here is not incompatible with a relationist account such as Leonelli's. This is because the kinds of changes in records I have been discussing are compatible with other kinds of changes discussed by Leonelli, such as material changes and changes in how the data are used (see Section 2 above). My purpose in this chapter is

[16] It may be instructive at this point to compare this inferentialist motive with Suarez's (2004) inferentialist account of representation. Briefly, he stipulates, minimally, that representations have a force that points to a target and representations must allow for inferences about the target. The force is established at least partly by convention.
[17] This is distinct from Goodman's Grue problem (1955) because Goodman was concerned with criteria for "lawlikeness" and prediction—and hence induction (p. 310)—whereas I am concerned with changes in identity. The two questions are not completely unrelated.

not to reject the importance of material and use changes, but to highlight another form of change that I believe is sufficiently interesting to address directly and that changes in the data in ways that are more fundamental than those addressed by other accounts.

There are, however, important differences between this account and relational accounts when it comes to stability. I take relational accounts to define the instability of data in terms of evidential use and materiality: as the use of data changes, their physicality changes and the data themselves change. In contrast, I am defining instability in terms of the what data are records of, irrespective of the particular evidential use to which the data will be put. So there is a sense in which for both accounts the epistemic context determines data identity, but for relational accounts it is use and material and for mine it is the subject of the record.

2.2.2 Perspectival Data

We might say that data, because of their dependence upon the epistemic context, are perspectival. Perspectivism, as Giere (2006) articulates it, is about representations, especially models. But the term perspectivism is just as apt for data because data are representational and because perspectivism also emphasizes the contextual nature of scientific practice, which is a feature that data have. Another reason to invoke perspectivism concerns how scientists make judgements. Recent work by Massimi (2012) suggests that we conceive of perspectivism as the epistemic context in which scientists work. Scientific work is always situated within a network of beliefs that should be reliably justified and coherent. She writes "Justified-belief-attribution is always perspectival and contextual: it has to do with the way each belief fits into the agent's epistemic perspective" (2012, 48).

Data are part of the epistemic context because scientists make judgments based upon their beliefs. And because collecting data is associated with a judgment, data collection should be cast as a perspectival activity that partly forms a perspective. If this is right, then these judgments are not merely informed by the epistemic contexts in which scientists work: they in fact partly constitute this epistemic context.

Another reason to invoke perspectivism here is to distinguish between context-dependence and theory-ladenness.[18] Theory-ladenness suggests a high level of dependence on a particular form of knowledge, specifically theoretical knowledge. Data, although they depend upon the epistemic context, do not depend upon theory so heavily. If they did, data could neither arbitrate between theoretical claims nor support or refute such claims.

[18] For some discussion of the issue of theory-ladenness, see for example (Kordig 1971; Brewer and Lambert 2001; Schindler 2011)

The practice of collecting data sets, analyzing them, and using them evidentially would then be very mysterious or even pointless. So data must have some level of independence from theory, which is part of what Bogen and Woodward (1988) took such pains to show. Hacking (Hacking 1983, 185) also argues, but in the context of observation more generally, that there must be enough independence from theory to provide empirical constraints.

The kind of epistemic context upon which data do depend—and of which data are a part—is more fundamental than theory. This context includes a range of knowledge, such as knowledge of instruments, experimental technique, and especially knowledge classification. That is, how to classify what is observed. Scientists make judgments about classification in association with data-collection and use. Such a judgment might be "this is a meteorological object," or "this is a lead sample." These judgments are perspectival, I suggest, because they are intimately connected to the understanding scientists have and this understanding is distinct from theory. One can, for instance, judge an object in the sky to be a comet and they might make this judgment independently of modern astronomical theory. And yet if the epistemic context changes sufficiently, scientists would make, or might make, different judgments. Instead of judging comets as meteorological, we now judge them to be astronomical. Let's examine a brief example to illustrate the perspective-dependence of data.

2.2.3 New Stars and Old Data

Here is an example that illustrates the perspectival view I am defending. In the 2nd century A.D. observers in China recorded a "new star." This record has since sparked several contemporary studies that attempt to make sense of what this "new star" might be in contemporary terms. Clark and Stephenson (1977) made an attempt to use this early observation. Thorsett (1992) and Green and Stephenson (2003) published further discussion of the issue. There are two challenges that these studies faced: one easy, one very challenging. The first is that these ancient observers did not write English and the second is that "new star" might mean several things. Establishing what it was they saw requires addressing these two problems with two tasks. The first is a straightforward translation of their language into English; this was the easy task. If you know the relevant languages, translation is straightforward. But translation alone does not establish what it was they saw in contemporary terms and this second task was the harder. To do this, Thorsett (1992) suggested that the "new star" might be the supernova MSH15-52, which was in contrast to a set of supernovae suggestions from Clark and Stephenson (1977). Thorsett went about arbitrating between these possibilities using a number of methods.

For one thing, a contemporary pulsar may have originated from supernova MSH15-52. Pulsars are small stars that can be detected using their radio emissions. Sometimes they are produced by supernovae. Thorsett estimated how old that pulsar is (which gives an indication of when the supernova occurred). This timing estimate seemed to match the Chinese observation. Thorsett also investigated three further considerations: where supernova MSH15-52 was likely to be in the night sky; how bright it was; and whether these estimates matched historical observation, or at least did not obviously conflict with the historical record. The result of these estimates and comparisons was that Clark and Stephenson's suggested supernovae did not fit well with the record, but MSH15-15 did.

The whole purpose of these studies was to put more specific constraints on contemporary theories. If modern astronomers could determine precisely when the supernova occurred, contemporary astronomers could use that information to test predictions more precisely.

This example illustrates a perspectival shift and it illustrates a change in data. The modern astronomers did not simply take the historical record and "interpret" the observation in contemporary terms. If it was a matter of interpretation, then the record itself should provide sufficient information for a translation. An interpreter (translating French into English) needs only hear the French phrase to translate it into English: no other research or information is required. This astronomy example is not so simple. The ancient Chinese did not discriminate between comets, supernovae, and some other astronomical phenomena. From their perspective, bright objects in the sky were all "stars," which was a completely reasonable judgement given the epistemic context. However, contemporary scientists make much finer distinctions between bright objects in the sky and "star" is too vague a term to constrain contemporary theories. So rather than just interpret the record in a new way, modern astronomers had to use the historical description of the event in conjunction with contemporary data and knowledge of astronomy to determine what event the Chinese astronomers observed. This process is very much like identification, which requires an understanding of how to determine the identity of an object, which is precisely what Thorsett knew and what he did.

Let us return to the two earlier points about why data change is not interpretation change. First, the data in this example presuppose content. Thorsett and other contemporary astronomers were not just using raw numbers both in working with ancient texts and in working with the data they extracted: they worked with records *of* something that was observed. Initially those records were of a "new star" and after contemporary research and re-identification, they were records of a supernova. In order to place constraints on contemporary theory, Thorsett's interpretation of the data first *required* that the data be records of very specific things, such as a supernova. Otherwise the data would have been useless.

Second, this example illustrates how once re-identified, the new data cannot be used in a straightforward way in the old perspective. Ancient Chinese astronomers did not recognize supernovae and would be unable to use an observation about a supernova without first learning about modern astronomy and the taxonomic distinctions it recognizes, which would be highly anachronistic.

It is important to note that there is a sense in which there is something that persists between the ancient Chinese records and the contemporary data. Otherwise, what would be the connection between the contemporary research and that event thousands of years ago? Surely there is one. The thing that persists is some coarser grained description of the event, such as "small bright light that appears at such-and-such time and location." This toy example is not particularly helpful for the ancient or contemporary astronomer, however, since the former are interested in stars and the latter in supernova: "small bright light" is not equivalent to "new star" nor to "supernova," even setting translating texts aside. It does illustrate, however, one way we could track the origins of Thorsett's data in this particular example and why a historical event is of interest to the contemporary scientist.

This description of re-identification may resemble Leonelli's "re-contextualization" (Leonelli 2016, 189). Despite the similarity, they differ in important ways. Leonelli notes that data are often packed and described in such a way as to allow them to be transported and later used (called de-contextualization). When scientists retrieve de-contextualised data, they then must "re-contextualise" them by situating the data, which involves classifying, describing, and forming them materially so as to provide evidential value for the particular inquiry.

This is substantially different from my view in two ways. First, I believe data, to be used as evidence, must bear partially stable information, *regardless* of the particular inquiry context in which they were or have been used, provided the *perspective is the same*.

The account defended here differs in a second way from a relational account: re-identification occurs only when we change how we classify what we have recorded, but not every time data is put to a new use; consequently, data do not change often. Some circumstances that precipitate re-identification may include substantive theoretical change —such as a shift toward a heliocentric universe—or some other change in our understanding of the world or our instruments. A paradigm shift is an example of the kind of change required for re-identification, though milder shifts in understanding may also be sufficient, provided they result in new taxonomic distinctions. Perspectives allow us to talk about these kinds of data changes without invoking the kind of radical upheaval associated with paradigm shifts.

We might think that I have, in considering data perspectival, just made them fully relative, mind-dependent entities. Didn't perspectivism promise to be a more constrained view than this, one that didn't preclude empirical constraints in the sciences? In response, perspectives only arise in cases where we want to compare, or examine the differences between, different perspectives. That is, perspectives only arise in the context of data when examining history, disagreement, or other cases where there is a kind of disunity of judgement.

2.3 Conclusion

In this chapter, I sought to address what data are and what relationship they have to the epistemic context in which scientists work. The account has the advantage of accommodating the empirical contribution data make, while also giving due consideration to the role of human knowledge and understanding in determining data identity. Because of these two constraints, data do not fit neatly with antirealist analyses of science, but nor do they have a place in the picture painted by realists with a strong empirical commitment. My account of data is, consequently, a kind of middle ground. To strike this balance, I argued that, contra relational accounts, data are generally highly stable across different inquiries and across material forms. Data have this stability because they are at least partly representational. I then argued 1) that data are not stable in the face of certain kinds of scientific change and 2) that we can think of data as perspectival in the sense that they are part of the epistemic context. This contextual dependence comes from the distinctions that scientists make about what they record. One upshot of thinking about data in this way is that it shows how even old data can be repurposed and improved to reflect the contemporary epistemic context and help scientists investigate contemporary research questions. This account also gives credence to the profound changes in science and the surmountable difficulties associated with navigating the ancient sciences and our changing understanding of the world.

I have so far only made cursory remarks about what an epistemic context is and what relevance the distinctions that scientists make have. The following chapter will develop these ideas more fully.

3
Pluralism and Practices

Abstract
What does it mean to be pluralist about epistemic contexts? This chapter will argue that we should understand the epistemic context primarily in terms of epistemic engagement, in contrast to a strong pragmatic approach, although both approaches are committed to associating the epistemic context, and pluralism, with scientific practice. Epistemic engagement is made possible by common commitment to a conceptual taxonomy. When there are rival taxonomies, or alternative taxonomies, we have pluralism, understood in this context as a form of perspectivism. I draw on a Wittgensteinian tradition in the philosophy of language in developing this account of epistemic engagement.

3.0 Introduction
The preceding chapter argued that data are tied to the epistemic context and vary as the context does. There briefly arose the question of what the epistemic context is. And in answer I briefly mentioned that the distinctions scientists made were an important part of this context. The purpose of this chapter is to explore more fully what an epistemic context consists in and how we should understand the distinctions that scientists make.

In section 3.1, I discuss the motivation for pluralism in science generally. Pluralism has recently become a very popular way of avoiding reductive and simplistic views of science. But when one is a pluralist, what is the subject of the pluralism? Pluralist accounts can be roughly grouped into ontological and epistemic versions. The first make claims about what the world is like, the second make claims about the structure of scientific inquiry. I suggest in section 3.2 that we should consider ontological pluralism a secondary inquiry after epistemic pluralism, which is consistent with several ontological approaches: one should have an epistemic view before one develops an ontological view.

But what does it mean to be an epistemic pluralist? What is such an account pluralist about? I argue in section 3.3 that such a view involves commitment to a pluralism about scientific practices. Understanding pluralism therefore requires defining practices and characterizing how they can be distinguished. I show that several forms of epistemic pluralism all have a similar commitment to distinguishing practices by appeal to some notion of an end. Using ends as the basis of pluralism in this way is a difficult project for several reasons that I discuss. We can find an alternative way of defining and characterizing practices in the philosophy of language, which I discuss in 3.4. This approach, which is broadly Wittgensteinian and perspectival, uses epistemic engagement as the basis of distinguishing practices in the context of science. I show what this could look like in 3.5. This approach is more precise and avoids the pitfalls of using ends.

Pluralism, as a general term, suggests the acceptance, or at least tolerance, of a set of alternatives, be they alternative classifications, practices, ontologies, theories, etc. Whatever might compose a set that we are pluralist about, a pluralist view must have some basis for distinguishing between members of that set. I am going to assume that a pluralist view, to be pluralist at all, must give us some criteria for distinguishing between members of the set. Without these criteria, we do not have a pluralist account. Part of the general argument of this chapter is that many pluralists get their criteria from pragmatism and that there is a better alternative, which I provide.

3.1 Pluralism and Practices: Historical Context

The tradition of philosophically examining science using pluralist lenses stretches into what are now historical texts. There are two threads in this tradition that are important for this chapter's argument. The first thread is the shift from analysing theory to analysing practices. The second thread is the shift from expecting unification in science to expecting pluralism.

We can see both of these threads emerge from concerns with logical positivism, a tradition which paradigmatically analysed science in terms of its theory and expected unification in the sciences. Theory should be analysed propositionally and the activities and interests of scientists, though interesting for sociology or history, are concerned with discovery, not with justification, i.e. the logical structure of theory (Reichenbach (1938)). With this distinction between discovery and justification, philosophers could abstract and analyse independently of the activities associated with scientific practice. With this form of abstraction came the expectation that sciences would converge on unified explanations (Hempel and Oppenheim 1948).

Subsequently, philosophers shifted focus from abstract propositions to the things scientists were actually doing, i.e. the practice of science. Kuhn's (1976) analysis of paradigms offered a holistic way of thinking about the tight connection between theory and the activities that scientists performed, although it offered much more than that too.

The semantic turn continued the emphasis on practice. Unlike Kuhn, this analysis emphasized theory, but instead of treating theory propositionally, philosophers appealed to models. This shift promised to abstract less from scientific practice, i.e. what the scientists were actually doing. Another important development away from positivism was the more explicit introduction of plurality in science. Suppes (1978) drew attention to what looked like a divergence, not a convergence, in scientific *practices*, not just theories, which suggested the sciences would diversify, not cohere on a single unified explanation. The evidence for this claim comes from an examination of the things scientists say and do, i.e. from looking at practice.

For Suppes, the complexity and diversity of scientific practice offers justification for a pluralist attitude. It is consequently natural to treat an examination of practice at the same time as an examination of pluralism, since justification for the latter can emerge from an examination of the former.[19] Nevertheless, it is not obvious what sort of pluralism best accounts for the disorder we see in scientific practice. Philosophers have made a number of suggestions since Suppes in 1978.

The varieties of pluralism may be divided crudely into two camps: an epistemic and ontological camp. The ontological camp holds that the world is inherently pluralistic: there is a diversity of ontology that cannot be reduced. Cartwright defends a version of this view in *The Dappled World* (1999). Dupré has a related account in *The Disorder of Things* and Chakravartty more recently defends an ontological pluralism about properties (2011).

The second type of pluralism, epistemic pluralism, does not necessarily make the ontological claim. Instead, this view holds that science is composed of different epistemic systems that cannot be reduced or eliminated. Longino (Longino in Kellert, Longino, and Waters 2006), Kusch (2017), Chang (2012), Wylie (2015), and Danks (2015) are some defenders of epistemic pluralism, in various forms. Pluralists of this breed are often adverse to making ontological claims (Kellert, Longino, and Waters 2006 Introduction). Because perspectivism as discussed in the last chapter is concerned with epistemic side of science, this second form of pluralism is the more relevant and therefore the subject of this chapter. I provide motivation for this choice and why ontological pluralism is of secondary importance below.

[19] See for example the introduction in (Kellert, Longino, and Waters 2006) for a discussion of pluralism and various ways this might apply to the sciences.

3.2 Ontological Pluralism

Ontological pluralism takes various forms. Many are associated with the Stanford school and include Dupré, Cartwright, and Suppes. Cartwright, Dupré, and more recently Chakravartty defend more ontological forms of pluralism. However, there are important differences in the way these philosophers make ontological claims. For Cartwright and Dupré, the ontological claim is secondary to epistemic considerations.

In *the Dappled World*, Cartwright approaches ontological pluralism after raising some epistemic considerations. She first brings our attention to the various things we know in our everyday lives and the number of very precise things the natural sciences have taught us, which are all epistemic considerations. She points this out here:

> Besides this odd assortment of inexact facts, we also have a great deal of very precise and exact knowledge, chiefly supplied by the natural sciences. I am not thinking here of abstract laws, which as an empiricist I take to be of considerable remove from the world they are supposed to apply to, but rather of the precise behavior of specific kinds of concrete systems [....] (1999, 24).

She takes it as a fact that there are a number of things we know. She then asks what ontological conclusion this fact licenses. From this epistemic claim, she argues that the laws of nature are constrained in their application to very specific and controlled—usually experimental—contexts, what she calls nomological machines (1999, chap. 3). The view that best supports this is metaphysical pluralism (1999, 31). Different parts of nature are governed by different laws that may or may not be related to one another, but whatever relation there is between these laws is not systematic.

Notice that in Cartwright's view, the ontological claim about nature emerges from the epistemic claims she makes about human knowledge. She does not appeal to content that is speculative, highly abstracted, or highly theoretical, but to well established knowledge of every day facts and to well established scientific facts. The scientific facts are connected to specific or practical problems, like sending information through fibre optics cables (1999, 30), and involve very little abstraction. We can say generally that her appeal to knowledge is an appeal to knowledge of specific facts and that possession of this knowledge is demonstrable through the solving of specific problems. So even though Cartwright is ultimately concerned with metaphysics, her analysis of science begins with epistemology.

Something similar underlies Dupré's promiscuous realism (Dupré 1981; Dupré 1995, 1996). He writes

> It is, of course, impossible for any such philosophical thesis to contradict an empirical demonstration—a demonstration derived, that is to say, from the investigation of the actual practice of science—that science is at this time in a state of radical disunity. But the deeper question is whether science is disunified simply because it has not yet been unified, or rather because disunity is its inevitable and appropriate condition (Dupré 1996, 102)

A philosophical account must be informed by, and be consistent with, an examination of scientific practice. A philosophical investigation of scientific practice shows that science is very disunified and what this disunity consists in is the purview of philosophy. Two options that Dupré points to here are, first, that the disunity may just be an artefact of the current state of science, a state that may later give way to more unity. Or, second, the disunity may not be accidental, but a fact about scientific inquiry.

In other work (Dupré 1995, sec. 1), Dupré defends the second option. He does so by showing that different biological classifications satisfy very different theoretical needs and provide explanations for very different natural phenomena. These epistemic considerations suggest disunity is not just an artefact of scientific practice, but an essential part of it. The disunity is essential because, according to Dupré (1995), our (humanity's) varied interests give rise to a plurality of natural classifications. A consequence of this initial position is a metaphysical conclusion: nature has a variety of structures, giving us a number of different natural classifications. There are two important elements to Dupré's thought: first, his interest in ontology emerges from epistemology; second, that epistemology concerns primarily an investigation of scientific practice.

Chakravartty (2011), however, takes a different approach. Although he is also interested in ontological pluralism, there are several worries we might have with his approach. I discuss his view and then show the worries, which generally concern how realist his account can be. He, like Dupré, is interested in classification and ontology. He takes it as given that science as a whole is committed to a kind of taxonomic pluralism. The task he sets himself, given this commitment, is to provide a realist ontology. At minimum this realist view is committed to the sociability of properties.

> […]Taking properties to be the focus of realist commitment in the first instance introduces precisely the sort of taxonomic flexibility the realist needs in order to satisfy the requirement […] that there exists more than one structure of natural kinds (2011, 169).

The realist here is committed to the existence of properties and on the basis of these properties, scientists create different taxonomies. Any particular taxonomy is relative, pluralist, and mind dependent, but the properties are not. This gives the realist, one supposes, the flexibility to accommodate the diverse taxonomies in the sciences, but without falling into strong relativism, constructivism, constructive empiricism, or instrumentalism. There is, however, still a nominalist threat, which Chakravartty avoids by the following clarification (2011, 170): properties are not randomly distributed, but grouped together, hence they are "sociable." Sociable properties give us reason for forming some taxonomies over others, i.e. taxonomies are not arbitrary.

There are two puzzling features of this account. The first puzzle concerns whether properties can be grouped in the way Chakravartty suggests. Let's take his example of electrons to illustrate this puzzle.

> [...] Some groupings of properties are more sociable than others. The mass, charge, and spin of an electron [...] are always found together where there are electrons [...] (2011, 171)

Taxonomic pluralism is possible, Chakravartty contends, because the mind-independent properties can be grouped in different ways. The choice of which properties form the basis of the taxonomy is conventional. However, this electron example does not sit very well with this brand of pluralism. These three properties—mass, charge, and spin—are *always* associated with electrons. How then, can they give rise to taxonomic pluralism if they are always grouped together? And why are they always grouped together? The plausible answer is that they are the properties of an object: an electron. This suggests that it is in fact the electrons that are important here; they are what bind these different properties together and make them interesting for scientific investigation. Only by being the spin of an electron is the property of spin a property that scientists can study. This position that electrons occupy in physics is not one of convention, i.e. it's not clear to me that scientists could have chosen other properties for their taxonomy because the kind of constraints the world places on building a taxonomy are stricter than the constraints of a convention. What implications does this have for Chakravartty's taxonomic pluralism? The discussion implies that conventionalism creeps too far into taxonomy. So although Chakravartty is striving for a realist view of taxonomic pluralism, it is not realist enough. And it does not provide an ontology for the epistemology of science.

The second puzzle about Chakravartty's account is that it does little to illuminate why science might give rise to taxonomic pluralism, a fact that it takes as given, but which the other authors discussed here do not because they draw pluralist conclusions about ontology based on epistemic analyses. But why and when is science pluralist? One answer he could give is this: scientific practice has a number of taxonomies because scientists group things in different ways. Why? Because there are a number of properties that can be naturally grouped in those ways. How do we know that? Because scientists make a number of taxonomies. This, however, is circular and I don't take Chakravartty to have addressed why it would be that science is taxonomically pluralist.

As an argument against the content of Chakravartty's project, this argument of circularity is not fair. He is, after all, establishing a realist metaphysics that accounts for the pluralism we see in science. That is, he takes for granted a plurality of taxonomies. But as an argument against the approach, my criticism still stands because it is unclear what status pluralism in the sciences has. Is it necessary? Productive? Why is it there? What drives these different taxonomies? These questions seem like the kind philosophers should entertain and the kind that should be addressed before ontological claims. If this criticism does apply to Chakravartty, we should conclude that one should have a story to tell about scientific practice before one has a story to tell about the underlying metaphysics. Cartwright and Dupré have this kind of story in place and Chakravartty should too. Consequently, the discussion will now turn to scientific practice and what pluralism may look like in that context.

3.3 Epistemic Pluralism

The epistemic attitude toward pluralism takes various forms.[20] These attitudes generally make few claims about ontology; the emphasis is epistemic. One common thread to pluralists is the desire to be empirically informed. Some pluralists include the authors of *Scientific Pluralism* (Kellert, Longino, and Waters 2006). Other examples of pluralism that are motivated and informed by detailed case studies include Mitchell (2002, 2003, 1992), Giere (2006), Chang (2012), and Danks (2015, 2007, 2005). There are important differences between these views, but also important similarities. Two important similarities are an attention to scientific practice and the appeal to ends, epistemic or otherwise, when characterizing practices. Let's examine what these ends are and the role they play.

3.3.1 Ends

[20] A note on terms: I will use "pluralism" in this chapter to refer to views that reject unification, reduction, convergence, and elimination in science. I use "pragmatic pluralism" to refer to pluralist views that take aims or some other end as the basis of their analyses of science. Such analyses are pluralist in that they reject unification as the aim of science.

The main similarity I focus on here is the role of ends, or aims, in pluralism. There are a number of roles that an end can play, but there is a general commitment that many authors have to using them to define practices. Because the structural disunity we see in science is, according to these views, a product of the diverse pursuits of diverse aims, the epistemic pluralism discussed in this section is really a *pragmatic* pluralism, so called because it is not the representational or ontological features of science that are important, but the practical abilities required to further epistemic or other ends. I will argue in this section that epistemic pluralists are consequently not just pluralist about practices, they are primarily pluralists about aims, which gives rise to a pluralism about practices. Let's examine some passages where this view emerges. Chang is particularly explicit about the role of aims. He writes the following about how aims structure practices:

> A system of practice is formed by a coherent set of epistemic activities performed with a view to achieve certain aims (Chang 2012, 293:15).

My main interest in this passage is the appeal to aims, which give coherence to the activities that scientists perform. Chang uses aims to determine when scientists are or are not part of the same practice. I take the following to be a sympathetic reconstruction of how to use Chang's account to determine whether scientists are part of the same practice:

> Scientists are part of the same *Practice* when they share the same (i) aims, (ii) use the same methods to pursue those aims, and (iii) evaluate their work using those shared aims.

Although Chang does not explicitly formulate a system of practice in this way, I think this is in keeping with the spirit of his thought. Crucial for this account is the notion that scientists occupy different epistemic practices, but how are we to determine whether they are members of the same practice? In this paraphrase of Chang's view, it is an analysis of the aims that tells us when scientists are part of different practices. If two scientists have different aims, they are part of different practices because they will require different methods and will evaluate their work differently (by appealing to different aims).

Other accounts of epistemic pluralism also appeal to some kind of end, even if the end goes by another name. Longino claims that different groups of scientists develop different *questions* and different methods to address those questions (Longino in Kellert, Longino, and Waters 2006, 111). She argues a number of approaches can provide an account of the same phenomenon by each providing answers to questions best addressed by that approach (Longino in Kellert, Longino, and Waters 2006, 127). An "approach" here is equivalent to an epistemic practice. Different questions require different methods because the question determines what causal space scientists will be investigating. Questions consequently function as ends in structuring scientific activity.

Another pluralist who appeals to ends as criteria for distinguishing practices is Danks. He (2015) suggests practices are goal-dependent. Different practices have different goals and which theory that practice considers "best" depends upon those goals. Because there is no practice- or goal-independent method for determining the best theory, we should be epistemically pluralist.[21] Like Chang and Longino, scientists have ends and these ends give us the means to distinguish practices.

There are also model-based versions of pragmatic pluralism. Two I will mention here are Giere's perspectival realism (2006) and Mitchell's integrative pluralism (Mitchell 2003, chap. 6), both of which have strong pragmatic threads.[22] Giere's view is that models are partial representations that represent in the following way:

> Agents (1) intend; (2) to use model, M; (3) to represent a part of the world, W; (4) for some purpose, P (2010, 274).

Notice that the representation, and therefore suitability of a model, depends upon purposes, i.e. on the ends the scientists (agents) are pursuing. This suggests that models are associated with practices the members of which have purposes and if the purposes change, then the choice of models will change. Once again, an end plays a role in distinguishing practices.

Mitchell's view is related. She also argues that different models provide "perspectives" on target phenomena. Models do this through idealizing, abstracting, and selecting different features of a phenomenon to represent. No model perfectly represents. The resemblance to Giere's view is strong here, but there is an important difference that sets Mitchell's views apart from many other pluralists. Different models can be integrated to provide better predictions and explanations of phenomena; this is an unusual move for an epistemic pluralist. The pragmatic thread winds through this view when evaluating models. A model's adequacy is determined by the aims of the scientists who use it. Consider the following:

[21] He thinks he also implies an ontological pluralism.
[22] See chapter 1 for a more detailed discussion of these views. Here my main purpose is to show their pragmatic commitments.

> What the investigator wants to do provides the source of criteria for judging representational model adequacy. Different models can each correctly describe the same complex system and yet not be reducible to a single representation from a single perspective. Empirical confirmation warrants correctness, while pragmatic concerns decide adequacy (Mitchell in Massimi and McCoy 2019, 188).

This is one of the most explicit and clear accounts of the role of aims among pragmatic commitments. The aims, in this case, are "what the investigator wants to do." And what the investigator wants to do determines what models are adequate, i.e. which are to be used and which are to be rejected, avoided, and disregarded.

The discussion has shifted here from accounts of pluralism about practice to pluralism about models. This shift is licit because models are typically associated with particular practices, so if there is a shift in models, there is a shift in practices. Mitchell draws attention to this connection between models and practice (Mitchell in Massimi and McCoy 2019, 185).

So far we have examined several different accounts of science that take some notion of an end as criterion for distinguishing practices. The general approach to ends in science, a pragmatic approach, is natural and intuitive in several respects. It promises to give a clear way of identifying when scientists are part of the same or different epistemic contexts, practices, or research programs (if ends differ, so do the contexts in which scientists work). It also provides a tool for evaluating scientific activity (an explanation, prediction, or theory is adequate or acceptable to the degree it achieves the relevant ends). It allows us to set aside metaphysical questions about truth (an explanation can be successful without the need to specify whether it is true). And finally it gives us a framework for thinking about disunity and diversity in the sciences (and it may have other advantages too). Despite these diverse merits, I am going to suggest the pragmatic approach does not provide as complete a picture of science as it may seem, though it is an insightful and powerful approach nonetheless.

The picture is difficult to complete because ends are not easy to elucidate with precision. We can preview this difficulty by comparing the contemporary pragmatic approach with Kuhn's discussion of values or criteria for theory choice (1979). The similarity is that pluralists and Kuhn make appeal to criteria for deciding which theories, claims, or explanations to endorse. However, the more recent pluralist views depart from Kuhn's discussion of values in at least one important way: the ends are internal to the practice and hence have a strong element of subjectivity. Kuhn, in contrast, treats values as objective—in the sense of shared by all scientists, regardless of practice (Kuhn and Epstein 1979). Objectivity in this case is little different from inter-subjectivity, but the point holds because there is very little "inter" attached to the subjectivity of the pragmatic pluralist views discussed here. Many forms of pluralism do not stress the objectivity of questions or aims and I will suggest this prompts several puzzles about the role of aims in science, which I raise in the following section.

However, it is important to recognize before proceeding that some authors take pains to point out the difficulties associated with subjective aims and that some authors are not explicit about the subjectivity of aims. Danks suggests it is important for a perspectival pluralist to avoid inherently individualistic analyses (Danks in Massimi and McCoy 2019, 136) and Longino's and Mitchell's views could be easily construed so as to avoid subjectivity. However, Danks does appeal to aims that seem subjective and individualistic and his analysis of pluralism seems to appeal exclusively to the cognition of individuals. The scientific aims Chang discusses are also very specific to individual scientists. Few others explicitly reject subjective aims, apart from Kuhn, which suggests that appealing to aims without falling into an individualistic analysis of science is a difficult balance that needs more attention.

I think there is a key piece missing from our current thinking about pluralism with such a strong pragmatic foundation. The missing piece is how aims are situated in the complex practices in which scientists work. Appealing to aims without this more complex picture gives us four problems that all concern whether aims or some other end can in fact perform the function pragmatic pluralists expect. My purpose in raising these issues is not to deny that subjective ends have a role in science, but to suggest that they *must* play a more minor role than we might expect. I discuss each issue in turn before offering my own view of pluralism that situates aims within scientific practices.

3.3.3 Can Aims Partition Practices? Four Reasons Why They Cannot

The first issue is that any given scientist may have a number of aims, some of which are shared by other scientists, some not. Determining which aims are relevant for membership of a practice is not straightforward. For instance, two scientists may share a broad vision for their work, but differ in how they expect to achieve that broad end because the narrow ends may not be shared. Both the broad and narrow goals can guide and inform a scientist's work, but if scientists share only some of these, which should we choose when deciding if they are part of the same practice? Lavoisier, a chemist, and Einstein, a physicist, may both have had general aims of providing systematic, unifying, and mathematical accounts of the universe, but their narrow ends certainly differed, as did their day-to-day practice. This suggests the narrow goals may be more important.

However, Bohr and Einstein both shared many narrow goals and differed strongly on broader aims associated with how to evaluate a statistical theory and whether such a theory is satisfactory (Hansen 1976). Are they then members of different practices? That may be too strong since they both worked with the same phenomena and the same mathematical apparatus. The lesson we should draw from this comparison is that it is unclear how to choose which ends should guide us in identifying scientific practices. We would need a principled strategy for choosing and we do not yet have one.

The second issue is that ends can be difficult to discern. Although this problem can be overcome by careful analysis of a scientist's work, there is a worry. Presumably scientists know who they are working with, i.e. who the members of their practice are. The criteria we use to distinguish practices, therefore, should not be obscure. The worry is this: if goals are difficult to discern, it may be hard to also give an account of how scientists could know to which practices they belong. The method of partitioning practices, this suggests, should be one we can easily follow and "we" should include not just philosophers, but scientists as well. It is not clear that consulting aims or ends is sufficiently easy or even necessary when determining practice membership, especially if careful textual analysis is required to make this determination.

The third issue is that it is not clear that scientists must have the same aims to be part of the same practice, nor that having separate aims precludes their membership in the same practice. This point should be particularly salient today when we have incredibly large scientific projects conducted by hundreds of scientists. Do all of those scientists have the same ends? Probably not, though establishing this would be difficult (see the second issue above). Take two hypothetical scientists working on genetic predispositions for cancer. One scientist may be driven by a desire to cure cancer, but the other might be driven solely by curiosity about genetic mechanisms. This is a pretty profound difference in goals, one humanitarian and medical, the other mechanistic and completely removed from humanitarian interests. Despite these profoundly different ends, both scientists can work on the same project and work toward producing the same results. Whether these scientists achieve the same results is independent of their subjective aims. This illustration suggests that the difference in aims plays little or no part in the fact that they are part of the same system of practice.

The fourth and final issue is related to the other three. The general approach associated with distinguishing practices based on the aims of individual scientists is isolating and exclusive in ways that may not accurately capture what science is like. If scientists with different aims are part of different practices, it would seem easy to develop an oppressive and exclusive form of science where those with different aims could be excluded solely on the basis of possessing different interests. But isn't science tolerant of different aims, approaches, and inquiries? This is a reasonable ideal to strive for even if it is imperfectly followed. And I contend that science is much more permissive of multiple aims and projects that can productively interact. I believe Mitchell's integrative pluralism shares this contention and other pluralisms probably do too. In striving to characterize science such that we acknowledge its inclusive and tolerant side, we need to situate aims so that they do not play a divisive role.

I have suggested that using aims or some other end to identify or define scientific practices face four issues. The first concerned how to choose aims, the second whether aims can provide the guidance required in discerning systems of practice, the third that different aims do not preclude membership of the same practice, and the fourth that aims so characterized are in tension with inclusive science. It is not worth, however, rejecting the relevance of ends entirely, if such a project is even feasible. The result would be an artificial characterization of science: we would be claiming that science is not a goal-oriented activity. Although it may not always be clear what those goals are or how many goals there are, scientists certainly go about their daily work with purpose. If there is purpose, surely there are aims as well.

But despite the importance of aims, there remain several questions about them: which aims do or should scientists have, how widely are they shared, and where do they come from? Until they are addressed, these questions and the issues I raised suggest aims are not the kind of clear and stable features of a practice that can provide the primitive basis of pluralism. If we use aims as criteria for distinguishing practices, it is just too unclear when we have a plurality and when we do not. We need a better understanding of the context in which aims feature and the basis of pluralism may lie elsewhere.

How then should we think about epistemic pluralism? What is pluralism in the sciences pluralism about, if not principally about aims? A thread in the philosophy of language that Wittgenstein started has resources for defining practices that avoids an appeal to aims. This thread gives us two things: first, it provides other criteria that I argue better establish how to tell practices apart. Second, it gives us a context for situating aims and understanding their role in a practice. I discuss what this thread is and how it applies to scientific pluralism. The foundation for this view of practices is the conceptual taxonomy scientists use. I discuss what such a taxonomy amounts to and why we might think of it as perspectival.

3.4 Practices and Language-games

Our understanding of pluralism in the sciences could be informed by an examination of practices elsewhere. I will be drawing in particular on a Wittgensteinian tradition in the philosophy of language that, like some philosophies of science, focuses on activity and pluralism in the context of practices. This Wittgensteinian approach suggests the way scientists respond toward one another's work—their epistemic engagement—should be the basis on which we characterize scientific practices. I first show that Wittgenstein's discussion of language-games gives us a basis for thinking about practices in general (section 3.1), then I describe how a discussion of language-games can inform our thinking about practices in the case of science in particular (section 3.2).

4.1 Language-games

I take Wittgenstein's discussion of language-games as a starting point for an analysis of practices. The discussion of games emerges in his later work. I will focus in particular on the *Philosophical Investigations.* There are two points I will be supporting; one is that his analysis is at heart an analysis of activity and the other is that activities have a degree of completeness that provides partial autonomy from one another. One of the first references to language-games is the following:

> Here the term 'language-*game*' is meant to bring into prominence the fact that the *speaking* of language is part of an activity, or of a life-form (Wittgenstein 2008, sec. 23).

Wittgenstein is explicit in claiming that speaking a language—using a language—is part of an activity. As such, we might expect any insightful analysis of a part of language to be equivalent to an analysis of activity. This is consistent with some pluralist analyses of science, for example Chang's treatment of a practice as an activity. There is reason for thinking, therefore, that Wittgenstein's views of activity have some bearing on scientific activity.

There is a second reason, however, that Wittgenstein has for using this term, something he call the completeness of a language-game, which will also prove important for an analysis of science. Completeness, I will suggest, concerns the epistemic requirements that members of a practice must meet. I discuss what this means in Wittgenstein's writings, then suggest it can supply criteria for distinguishing practices.

> If you want to say that this [primitive language-game] shews them to be incomplete, ask yourself whether our language is complete;—whether it was so before the symbolism of chemistry and the

> notation of the infinitesimal calculus were incorporated in it; for these are, so to speak, suburbs of our language (Wittgenstein 2008, sec. 18).

In this passage, Wittgenstein is responding to an objection that language-games, in being isolated, are too incomplete, i.e. cannot be understood in isolation. And he responds to the objection by suggesting that completeness does not apply to language straightforwardly because it is continually changing (at what point would a language become complete)? This is not meant to suggest that it is possible to know only a single language-game. Black (1979) suggests we call such a view "autonomy" and argues it is implausible. I agree and doubt Wittgenstein held such a view. Rather, Wittgenstein's point here concerns what someone must know and do to participate in a particular language-game. A language-game is "complete," so to speak, in that the activity is self-contained; we need not appeal to other activities to understand *this* activity, this language-game. Other games may, however, have some relevance to a given game, but understanding the given game does not require appeal to another game and an explanation of how to play does not involve the description of another game, or knowing how to play another game. Understanding, in this case, includes knowing the point of the game, etc.

Wittgenstein's point about the completeness of language-games is figurative. He introduces it to illustrate certain features of language. Because the purpose is merely to illustrate, there are relationships between different things we do with language that may not be clearly captured by only thinking of language as a language-game or a set of language-games. For example, one need not know how to play American football to play European football, but it is less clear that one can engage in the activity of praising without some awareness of the activity of criticising or condemning. This suggests different parts of language have a level of connectivity that games often do not.

This caveat and constraint on the autonomy of language-games is consistent with Wittgenstein, who does, after all, indicate that he means language-game both to talk about language as a whole as well as simplified elements of language-activity, i.e. the appeal to games is helpful, but not necessarily literally true. The simpler cases make addressing certain questions easier; Wittgenstein writes that investigating simpler, more primitive examples clear the "fog," i.e. makes it easier to understand some features of meaning (2008, sec. 5). Specifically, the appeal to language-games may give Wittgenstein a way to investigate the role of context in understanding rules and the project-ability of concepts (Cavell 1962, 71).

But rules are not the only feature of language or practices that language-games can elucidate. They also show that the functioning of a word in one context (language-game) can differ substantially from its function in another. This is very closely related to the concerns scientific pluralists have, namely, how should we evaluate scientific activity? Both Wittgenstein and scientific pluralists emphasize contextual evaluations. Consider the following:

> If you do not keep the multiplicity of language-games in view you will perhaps be inclined to ask questions like: 'what is a question?' —Is it the statement that I do not know such-and-such, or the statement that I wish the other person would tell me….? Or is it the description of my mental state of uncertainty?—And is the cry 'Help!' such a description?
>
> Think how many different kinds of thing are called 'description': description of a body's position by means of its co-ordinates; description of a facial expression; description of a sensation of touch; of a mood (Wittgenstein 2008, sec. 24).

In asking us to consider the number of language-games, Wittgenstein asks us to consider 1) the sheer number of contexts in which a word can appear and 2) how very different the function of that word can be in those different contexts. The plurality of language-games in conjunction with the completeness of a given language-game suggests the following: we can understand an action (know how to do it and why) in a particular context, but that action does not necessarily have the same function in other contexts (in other language-games). We need not appeal, in other words, to other language-games when trying to understand an action in *this* language-game. The parallels to scientific pluralism are very striking. Both views about practice suggest we should evaluate an activity within a practice and that other practices may have little bearing on this evaluation. So far I take this discussion of Wittgenstein to indicate there are similarities between his views and scientific pluralism. However, I think there are also resources in Wittgenstein's thought that can help clarify what it is we should be pluralist about more specifically.

To make this clarification, I now return to the first purpose I claimed Wittgenstein had in introducing language-games: to emphasize language as activity. We might ask why he places this emphasis and what participating in such an activity entails. I raise these questions because answering them will help us get clear on how to determine whether two people are playing the same language-game, which will in turn clarify three things: when scientists are part of the same scientific practice, what it means for there to be multiple practices, and what we should be pluralist about.

One reason for Wittgenstein's emphasis on activity lies in what a participant in a language-game must know and do. Some such knowledge includes knowing how to affect others, how to respond appropriately to the actions of others, and whether others are responding appropriately to us. We can see this in both Wittgenstein and in expositions of his work. In (2008, sec. 2 and 8), Wittgenstein describes a primitive language-game consisting in builders using terms to give orders and follow them. Playing the game involves not just knowing, say, the definitions of the terms, but also how to use these terms to affect the behaviour in others and also how to act and respond appropriately. Black (1979, 346) discusses this as well. He writes that

> In both [language-games] we have two persons using and co-operatively responding to one-word sentences....

There are two main lessons I want to draw from this discussion of language-games as activities. The first is that, as activity, it involves social action, i.e. acting with others and hence responding. Two people, we might therefore say, are playing the same language-game—and thus participating in the same activity—if they are using the same body of words and responding to one another in the same ways. This will not be true of all language-games necessarily; Wittgenstein uses examples of language-games that one plays by oneself (2008, sec. 23) and, for such games, responding to *others* is not applicable, though we might think it possible to respond to oneself. The general notion of appropriate response, therefore, applies.

The second lesson I want to draw is that the completeness of a given language-game suggests mastery of other games is not necessary for playing a given game. A language-game does not require recourse to other games or the understanding of other specific games. One can investigate a game and how to play it, in other words, without investigating the workings of other games and how to play them. I do not take this to be absolute; language-games need not be fully autonomous as Black (1979) argues at length. And Wittgenstein suggests (2008, sec. 5) it is merely useful to consider primitive languages (language-games), useful for bringing to light similarities and differences within language (2008, sec. 130). Completeness and the relationship between language-games is a much more complex topic than this cursory discussion suggests, but for the purposes of this chapter, it is sufficient to claim that language-games and hence practices can be at least partly independent from one another.

These two features of language-games—activity and completeness—apply to the philosophical treatment of practices more generally, i.e. not just language. We can treat such practices as consisting in activities and we can also identify a plurality of activities that bear some independence from one another. These two features of scientific practice, action and independence, are analogous to the emphasis Wittgenstein places on activity and completeness.

What sort of activities are associated with practices and how does this help us identify what we should be pluralist about? These questions can be addressed by a further examination of the philosophy of language literature. Another philosophical view that addresses practices that makes more explicit the connection between practices and activity is Dummett's. He writes, of someone learning a language, that

> What he learns is a practice; he learns to respond, verbally and non-verbally, to utterances and to make utterances of his own (1993, 47).

We can see here that an essential part of learning a practice, and hence being a member of that practice, consists in responding to what others in that practice are doing. Dummett was writing about language more generally and I will shortly be concerned with scientific practice, but the point will hold because it is a general point about what membership of a practice consists in. Perhaps one modification to Dummett's statement is to replace "utterances" with something like "activity," since scientists are concerned not solely with utterances, but also with experiment, evidence, analysis, and explanation, some of which may or may not take the form of an utterance.

Having discussed how Wittgenstein and some of those he inspired thought about practices, let's return to an earlier question I posed: how do we know when two people are part of the same practice? If we think of practice as analogous to a language-game, which I suggested is justified, then we can answer this question by investigating whether scientists 1) are performing the same actions and 2) responding to one another appropriately. Having explored the origins of this view in the philosophy of language, I now discuss what this view might look like in the scientific context in more detail.

4.2 Scientific Practice as Epistemic Engagement

The last section explored how Wittgensteinian philosophers of language characterize a practice. In this section, I defend a pluralist account of scientific practice using this approach from the philosophy of language. The novelty of this view is that rather than taking ends as criteria for identifying practices, it takes the responsiveness that scientists exhibit toward one another's actions, which I claimed was an important feature of membership of a practice in general. In what follows, I describe (1) how this view is committed to an analysis of activity; (2) the relationship between activity and membership of a practice; (3) the kinds of activity that determine membership of a practice; (4) how this kind of activity gives us criteria for identifying practices.

The starting point for this Wittgensteinian approach to practices is activity. This means that, like Chang's pragmatism, this account takes practices to be composed primarily of activities and that understanding what a practice is requires an account of the actions of which a practice is composed. The difference between practices, consequently, consists in a difference in activity. Different practices, different activities.

If practices differ because of a difference in activity, then we need an account of scientific activity; presumably it is not the case that any and all actions that scientists make are relevant. The activity in question is the activity a scientist must perform in order to be a member of a practice. Being part of a practice is not like being part of a club that requires only membership dues. I could be a member of such a club and attend no club events, interact with no other club members. This is unlike a scientific practice. A scientific practice requires performing specific activities.

But what kind of activities are important for membership of a practice? The discussion of language practices suggests the relevant activities include responding to the various actions of other scientists in that practice, by which I mean, the scientist's work must be influenced by others in the practice and that work must then have an effect on other members. These effects include, but are not limited to, citing one another in scientific publications, providing alternative interpretations of an experiment, challenging someone's methods, someone's analysis, gathering new evidence for old claims, etc. I do not take this to be exhaustive: there are many ways to influence and to be influenced. But these forms of influence concern the inquiries scientists undertake and, as such, are epistemic.

What makes these forms of influence important is that they reflect two things scientists must know: first, each scientist must recognize the significance and form of the activities the other members perform; second, each scientist must know what kinds of responses are appropriate to the activities they recognize. For example, if one scientist criticises another using data as evidence, then the criticised scientist must be able to recognize that it is a criticism and how it is that the data provide an evidential basis for that criticism. That criticized scientist must then also know what kinds of responses to that criticism are available. Each and every member of a practice must know these two things.

Having discussed what scientists must know and do to be part of a practice, we now have the resources to give a foundation to epistemic pluralism. Practices are defined in terms of the activities of which they are at least partly composed. Scientists must be able to perform those activities in order to be members of a given practice. An examination of what activities scientists are performing tells us whether they are members of the same practice. The specific activities we must examine are those associated with responding to others.

4.3 Aims Situated

This view I defend shares with the other pluralist accounts an appreciation of aims, but with very different emphasis. The day-to-day activities scientists undertake may involve aims and fully understanding what they do will likely involve an appeal to these aims. But unlike the pluralist accounts discussed earlier, this analysis starts with the way scientists interact and based on that interaction, we draw conclusions about what kinds of aims are in play. By addressing activity first, we can determine when and to what extent goals are shared.

One way to situate the aims scientists have is to examine scientific activity. For instance, one important motive a scientist might have for responding to someone else is to criticise, support, indicate approval, or imitate an approach toward achieving an aim. In other words, common aims might motivate scientists to respond to one another. Common aims that do provide this kind of motivation will be apparent in the actions scientists take toward one another and so an examination of activity should lead to an understanding of aims.

Treating aims subsequently to activity addresses the problem I suggested other forms of pluralism face: the problem of specifying which are important and which incidental. Those which are important and those which are shared will feature in the activities scientists perform. The incidental aims will drop out. This approach to aims through activity is particularly useful in cases where aims may not be explicit, obvious, or clearly established; we do not need a clear and explicit articulation of aims to determine whether scientists are part of the same practice. We just need to look at what they are doing.

There is another serious upshot to thinking about aims in this way. It is not always clear in what direction an inquiry should go, nor is there always consensus about that direction. Thinking about practices in terms of activity, rather than aims, gives scientists a context for informing and working with one another without recourse to splitting them into different practices. In other words, there are cases when scientists might have different aims, but can still provide insight or work that is relevant for one another. If practices were defined in terms of aims, then this relevance would be somewhat mysterious because the scientists would be in different practices. But by defining practices by activity, there is room for scientists with different aims within the same practice. The case of Einstein and Bohr discussed earlier illustrates this. These scientists had very different explanatory aims, but were nevertheless able to critique each other's position and develop their own in response to criticism. Their work was enhanced by having different aims, but in such a way that they could still have productive exchanges. An analysis of practice based on activity, I think, allows for a more inclusive and productive kind of science.

I have suggested that rather than using goals to distinguish practices, we use this relevance and responsiveness as the basis of distinguishing units of scientific activity. I see two advantages in what I have proposed. The first is that it emphasizes what participation in a practice consists in, namely the degree to which scientists can engage meaningfully with one another, even in the face of disagreement. Secondly, my view gives us a way of approaching the role of goals and aims that situates them within a practice.

4.4 Objections

This suggestion that we focus on the responsiveness scientists show toward one another faces a number of objections. I will try to address three. The first objection is that I have not said whether disagreements can or should be resolved. The pluralist and relativist views discussed earlier have answers to these questions. Although I do not take the responsiveness approach to provide a recipe for resolving disagreement, I do take one advantage to be that it allows for rationally resolvable disagreements. Other accounts do too, such as Kusch's relativism, so it is not unique in that regard.[23] What is perhaps different, however, is that the responsiveness approach suggests disagreeing scientists actually do have a lot in common, such as common goals, interests, and membership of a practice.

One might object—and this is the second objection I will consider—that disagreeing scientists often have little in common, suggesting that interaction of the kind I described does not indicate membership of the same practice. Disagreements between scientists and religious figures may provide an example of deeper disagreements between people who share very little. There are two responses to this. The first is that I do not intend to have given a universal account of disagreement and I would certainly expect some kinds of disagreement to take forms other than those discussed here. The second response I can offer has two aspects and relates to the responsiveness criterion. The first aspect is that sometimes scientists and religious figures do have a lot in common and are

[23] I will discuss relativism and how it differs from my view in chapter 5

responsive to one another. Kusch (2018, 45–49, 60–62) has an aptly chosen case study to illustrate this. He points out that the disagreement between Galileo and Bellarmine over geocentrism and heliocentrism was underpinned by a number of common commitments, even though Galileo is sometimes seen as the quintessential scientist and Bellarmine the unscientific theologian. The second aspect is that to be a part of the same practice as I suggested requires responsiveness to take a certain form and unqualified disagreeing does not necessarily satisfy this criterion. The people in question must produce effects in one another's work, a demand that not all disagreements can satisfy. Modern debates about, say creationism, might be an example of disagreement between people who are not a part of the same practice because the level of disagreement does not concern specific parts of the practice, such as evidence and experiment. I would need to say much more to make these responses robust and they face their own objections, but they suggest that there is extensive room to account for disagreement, responsiveness, and membership of a practice.

The final objection is the following: sometimes scientists who do not engage with one another seem to have the same goals and to be part of the same practice, in some sense. A salient example is the co-discovery of evolution by natural selection made by Darwin and by Wallace. Darwin and Wallace did not communicate until their theories were almost fully developed, suggesting what is important here is the problem they were trying to solve, not how they engaged with one another. In response I would not want to reject the analysis that Darwin and Wallace were driven by the same problems, but I would suggest out that our ability to make this claim and to lump them into the same practice hinges upon their communication and upon the reception their theories received. To repeat an earlier claim, we might begin the order of explanation with the responsiveness, even while accepting that the teleology of the practice is important. Now one might respond that the common goals are what makes the responsiveness possible and so I have the order of explanation backwards. To this I would again ask to which goals should we appeal? This question is more easily answered by first looking at responsiveness. And it might be reasonable to accept that Darwin and Wallace were not members of the same practice until they began communicating, or perhaps until their peers responded to their work as work that addressed the same problem. After all, how do we know whether they were working on the same problem? A straightforward way would be to bring them into contact with one another and then see what kind of exchange ensues. Or to examine the activities of scientists who were influenced by both of their works. The very fact that there are such scientists is significant.

3.5 Practices and Perspectives

Having discussed how we might think of practices, I would like to draw a connection to perspectivism and by doing so, show how the judgements discussed in chapter 2 are connected to the epistemic context. We will therefore also have a clearer picture of what that context consists in. We might wonder whether there is any advantage to introducing perspectives. However, we have just relied on the claim that scientists do engage with one another in a practice, and that they must do so to be a part of a practice, but without considering how or why this is possible. Also unaddressed is the relationship between practices: are all scientific claims internal to practices? If so, then the view defended above may be indistinguishable from relativism. I will argue that the notion of a perspective addresses both of these issues. First, perspectives offer an explanation of how responding is possible and, second, appealing to perspectives allows for fruitful exchanges between practices in a way that does not sit well with strong internalism. I discuss more in chapter 5 how perspectivism and relativism differ.

Recall that I argued that an essential feature of membership of a practice consists in knowing how to influence other's work and knowing how to respond to the work of others (in the practice). How is such behaviour possible? While this may look like a sociological question, it is actually epistemic. A more specific way to pose the question that brings out the epistemology is the following:

> What must scientists know when they are part of a practice?

The interest here is not what a practice is, but what preconditions must be in place for membership and the answer is epistemic, not sociological. My suggested answer is that scientists must know how to make the right distinctions. The distinctions that scientists make give rise to a conceptual taxonomy. So knowingly belonging to a practice presupposes using the same taxonomy. Before discussing the taxonomy and why scientists must know it, it is important to say more about these distinctions.

What kinds of distinctions are scientists making? A ready response might be the distinctions that allow scientists to identify natural kinds. These kinds certainly have scientific importance; for example by being the basis of inductive inferences (Goodman 1955), a part of the causal structure of the world, and the basis of phenomena that scientists aim to understand. Another reason for thinking taxonomies should primarily concern natural kinds comes from Massimi (2015, 2014). Her account takes a Kantian version of natural kinds as the basis for thinking about scientific perspectives and how those perspectives change over time. Kuhn, she argues, may have taken a similar approach. Indeed, we can see Kuhn making explicit claims about the importance of natural kinds in the context of scientific change (1990). There are a number of important metaphysical questions about this general approach, questions I hope to set aside here because I believe we can posit the ontological status of kinds subsequently to establishing that scientists do categorise objects by kind. Scientists, to respond to each other, must recognize the same kinds, regardless of their naturalness.

To apply this thinking about kinds to an example, consider that the molecular biologist—to be a member of the practice of molecular biology—who must be able to identify which biological molecules are DNA and which are RNA (among many more). For this scientist, understanding the metaphysics of natural kinds is essential neither for their work nor for their membership of the practice. But he or she would need to know the structure of each molecule since that is the basis on which RNA and DNA are distinguished.

I noted that natural kinds *might* offer a ready answer to the question: what must scientists be able to distinguish? We may have some reticence about full commitment to this suggestion. Scientists, after all, must know all sorts of things, not just what natural kinds their colleagues study. Surely this is a requirement, but a very low bar indeed. It is more reflective of science and less ontologically controversial to keep the discussion more general. By more general, I mean at the level of a conceptual taxonomy, not just the conceptual taxonomy of natural kinds.

The level of interest is more general than natural kinds because scientists must also be able to distinguish features of the human part of science, features such as experiments, equipment, models, data, and statistical tests. These may not be natural kinds, but they are some of the things practicing scientists must master. I have in mind the difference between, for the molecular biologist, a scanning electron microscope and an agarose gel. Or the difference between a test that gives a p-value of .06 and a test that gives .04. For scientists to be members of a practice, they must know not only something about the natural world, they must also know something about how to investigate it and how others are investigating it. I take it that objects within these two domains form a taxonomy and that knowing such a taxonomy is necessary for responding appropriately to the work of others.

These two epistemic considerations—knowing how to distinguish parts of the world and parts of the investigation of it—are necessary for practice-membership. More specifically, we can presume that scientists must know these things when they are influencing, and responding to, one another. This is a legitimate presumption because the form of participation within a practice involves reference to elements of a common conceptual taxonomy. What might this mean? And how is it perspectival?

The first question, about why participation requires a taxonomy, may be answered briefly: participation in a practice—participation that consists in the appropriate response to others—will invoke at least some elements of the taxonomy in some way. Einstein and the Copenhagen group, for example, disagreed over the suitability of a statistical interpretation of quantum theory (Hansen 1976). Despite the different interpretations, the disagreement presupposed a common identification of electrons and light waves, among other things. I have assumed a synchronic analysis of perspectives or practices here, but the analysis applies diachronically as well.

Why is it fruitful to think of this as perspectival? The reason is that different practices have different shared taxonomies. Familiarly and hopefully intuitively, organismal biologists study organisms and use certain methods to do so while particle physicists study fundamental particles and use their own methods to do so. They are different practices and therefore committed to different perspectives. This becomes a particularly fruitful point when thinking about disagreements. In trying to understand why there is a disagreement, we are in part seeking what is and is not shared between those who disagree. In other words, what does the disagreement consist in? Perspectivism of this variety gives us a way of investigating what is and is not shared in disagreement. For instance, if scientists can respond appropriately to one another and recognize their responses for what they are, we have reason to think they are part of the same practice, in which case the disagreement is shallow and resolution is possible. If this kind of responsiveness is not present in a disagreement, then we have reason to think there are deep differences and that resolution is beyond reach.

There are several other upshots to thinking of science as perspectival. First, it provides a framework for justifying the thought that different scientific disciplines are quite different and are not reducible to one another. It also tells us when we have different disciplines (or practices). Perspectives also make sense of the spectrum of differences we can see between disciplines. By this I mean that some practices are more closely related than others. Perspectivism tells us why: because there is more or less overlap between different taxonomic systems.

There are important similarities and differences between perspectivism as I have described it and other accounts of scientific practice, which I have already discussed above. But paradigms or lexicons offer a second point of comparison, and perspectivism as described in (Massimi 2015) a third.

Perspectives could be similar to Kuhnian lexicons in that there is an emphasis on language, especially conceptual taxonomy (Kuhn 1990). But lexicons take a very different attitude toward language than perspectivism. As Kuhn articulated it, lexicons are associated with communities of scientists and a lexicon consists in—at minimum—a taxonomy of terms. So far this sounds similar to perspectivism. However, there are for Kuhn very specific requirements for that taxonomy (Kuhn 1990, 3–4); three requirements for that taxonomy are particularly relevant here. First, terms must be non-overlapping in their referents, except hierarchical terms. Second, the meaning of terms differ to the extent that their referents differ. And third, learning another taxonomy is like learning a language, except that translation between taxonomies is impossible. It is impossible because, for translation to be possible, it is necessary that the terms to be translated have the same exact referents as the taxonomy into which they will be translated. In other words, translation is impossible because there is no one-to-one mapping between terms of different lexicons, where terms are defined by their referents.

Like Kuhnian lexicons, perspectivism emphasizes taxonomy. Both also emphasize the importance of evaluating scientific claims within their historical context. However, this similarity is superficial given the many differences in how these views characterize that taxonomy. The first and most important difference is the characterization of meaning. Kuhnian lexicons consider terms defined by their extensions. It is consequently passive since the knowledge of a term is exhausted, I presume, by the grasp of a fixed extension. Perspectivism, in contrast, follows a more Wittgensteinian approach to meaning-as-use. The meaning of a term is to be analysed in terms of a practical ability, as discussed above. In other words, meaning is not exhausted by extension, but consists in part in what a term enables one to do.

Another difference between these views is incommensurability. Kuhn emphasizes incompatibility and incommensurability: communication is broken or never formed between paradigms or lexicons. This is partly because scientific terms are defined completely internally to the practice and any difference in definition precludes communication (so the Kuhnian story goes). Perspectives as I have described them are not so rigid and isolated. There could in principle be more or less overlap between perspectives.

Also importantly, there is room in the perspectival reading of science for extensive shared knowledge that transcends particular perspectives. That is, differences between perspectives are differences in degree, not kind. Some features of science are shared universally, or perhaps should be if something is to be considered scientific at all. Such features may include *sensitivity to evidence*, a preference for *simplicity*, a preference for *methods* that give *repeatable* results. This is not exhaustive, but failure to endorse features like these gives us grounds for thinking the practice in question is not scientific.

It would be difficult to recognize science as a practice without cross-perspectival standards such as these. There are two reasons for this difficulty: one posed by scientists who interact and one by the history of science. The first difficulty is, as Massimi (2018, 351–52) argues, that each perspective would merely license the truth of its own claims if standards did not transcend perspectives. Such an internalist view of standards is antithetical to scientific practice because any scientific claim could be just as true as any other (provided there is a perspective that accepts it as true). If this in turn were the case, we would struggle to understand why disagreements would take place and what would motivate change. Why would phlogiston chemists see oxygen chemistry as a rival theory and not just an irrelevant view? Why would astronomers abandon Aristotelian explanations of heavenly motions? These kinds of cases would be unmotivated and mysterious if science were not beholden to standards that transcend perspectives. Scientists who explicitly disagree or in some other way endorse different theories see relevance and significance in the work of others and this is not easily explained by an analysis of science that treats standards as internal.

But there is another problem too with thinking standards are internal to perspectives, a historical problem. How could the history of science, if standards were internal, have any relevance to the science of the present? Take as an example that contemporary chemistry recognizes oxygen as an element that binds with metals, something that Lavoisier also accepted in the 18th century. We can find similar examples where modern claims were also accepted in the past. However, if past scientists were part of different perspectives with their own internal standards that differ from ours, in what sense are the claims the same? The idea I am pushing here is this: the cognitive significance of claims like "oxygen binds with metals" is broadly the same for Lavoisier as it is for us. But if his standards and our standards differ, then it is unclear how we could share this cognitive significance. Internal standards raise the problem of incommensurability and this is incompatible with the expectation we have that science builds upon its history and that there is much that we share with our scientific ancestors. We should conclude, therefore, that there is not in fact such a deep divide between the perspective of modern chemistry and of past perspectives.

These standards as I have discussed them are, ironically, similar to Kuhn's discussion of values (1979, chap. 13). Kuhn takes as criteria for theory-choice universal values that different scientists apply differently when assessing scientific claims. His values include things like simplicity, fruitfulness, among many others. Standards are also similar to what Massimi (2018, 354) calls standards of performance adequacy, which are standards that scientific claims must meet in order to be accepted by other perspectives. The particular form the standards take depends upon the perspective.

I would like to make a modification, or change in emphasis, to the existing discussion of standards because of a possible worry that Kusch has raised about appealing to these kinds of standards. Kusch suggests that such standards (or principles as he calls them) are too abstract to inform us about disagreements, which are the product of very specific epistemic contexts (2018). Once we de-idealize the disagreement, it is not so clear that standards are so shared or play much of a role in resolving disagreements. This concern suggests we should think about standards a little differently, or at least to account for why things that are as abstract as a standard might still carry relevance for particular cases.

To avoid this problem, the clarification we need is this: to be cross-perspectival, a standard must not only be shared across perspectives, as Kuhn and Massimi have argued, but instantiations of that standard must also be *recognizable* by the relevant scientists as instantiations of the standard. How does this solve the problem Kusch raises? It does so by specifying in what way standards have relevance for de-idealised cases of disagreement. Standards have relevance because scientists recognize that they do so. There is still room for extensive disagreement over, for instance, the variety of forms standards can take, which standards to prioritize, and whether a single standard has been applied correctly.

This modification I offered is not the suggestion that all scientists know explicitly a body of standards and that each scientist can immediately recognize instantiations of those standards. What this does require, however, is that when a scientist presents evidence, an argument, or makes a claim, other scientists can in principle recognize the merits or deficiencies of that evidence, argument, etc. And a description or explanation of those merits would likely involve an appeal to standards such as simplicity or consistency with empirical evidence. Placing this requirement upon scientists, that they recognize the merits in the work of others, seems to me a realistic and not overly-idealised requirement. It goes some way toward ameliorating Kusch's concern that standards are too abstract *and* it suggests there is a kind of cognitive continuity present through disagreement and change.

3.6 Conclusion

This chapter has motivated a pluralist account of practices that is inspired by both Wittgenstein and the literature on perspectivism. I argued that criteria for distinguishing practices would give insight into what practices are and what pluralism should consist in. I argued against an existing attitude toward distinguishing practices that appeals to some notion of an end, a so-called pragmatic approach. I argued epistemic engagement provides a better criterion not only because it shows us where the limits of a practice lie, but also establishes what epistemic preconditions must be in place for practice membership. Part of those preconditions involve epistemic standards. Such standards are present through disagreements and change. By tying standards to epistemic engagement, I have provided a characterization of standards that is not too idealised, and therefore have addressed a criticism Kusch has levelled against them. To further make the case that perspectivism is not an overly-idealised position, the following chapter provides a perspectival interpretation of several experiments and exchanges during the Chemical Revolution.

4
Acids and Rust: A Case Study

Abstract
This chapter defends a perspectival account of scientific disagreement and applies it to a case from the Chemical Revolution. The following interpretation of this period uses several features of my account developed in preceding chapters; data do not provide a absolute arbitration and a central feature of disagreements between chemists during this period concerned taxonomy. I argue that my interpretation has several advantages over several recent analyses of this period. The perspectival view is distinctive in that it avoids discontinuity, specifies the type of disagreement, allows for the rational resolution of disagreement, and is sensitive to the historical epistemic context.

4.0 Introduction

This chapter illustrates the perspectival view of science discussed in the preceding chapter in a case study. I will contrast and defend my interpretation in more detail in the following chapter (chapter 5). The case study is the Chemical Revolution and is of particular interest because it demonstrates a classic case of pernicious scientific disagreement.

I will suggest that a central problem that chemists sought to address during the Chemical Revolution was the identity of several substances and how best to determine those identities. I intend this analysis to be in the spirit of Siegfried and Dobbs (1968), who suggest that the emphasis we place on the Chemical Revolution should be greatest on (1) changes in nomenclature, followed by (2) changes in the use of weight and, finally, (3) the replacement of phlogiston by oxygen. I also would like to de-emphasize the extent to which the Revolution consisted in a head-to-head confrontation between rival theories (oxygen versus phlogiston) and to emphasize that using weight changes how chemists determine which substances are simple. This new use of weight was, however, a natural extension of already existing practices and so does not constitute a radical shift. Such an analysis should be compatible with the point made by Klein (2015) that changes during the Chemical Revolution were less drastic than sometimes emphasized and were embedded in a continuous historical tradition. I will pursue this line of argument to illustrate the point I made in Chapter 3 about how to think of scientific practice in terms of scientists' responsiveness to conceptual/taxonomic changes rather than in terms of aims.

4.1 Historiography of the Chemical Revolution

Broadly, there are two approaches to thinking about the Chemical Revolution. The first suggests it was revolutionary in that fundamental elements of chemical practice changed, elements such as methods, ontology, explanation. The other approach places less emphasis on the change. I will describe features of these two approaches. I will then indicate how my view relates to this debate.

4.1.1 The Revolution was Revolutionary

One broad interpretive approach to the Chemical Revolution characterizes this period as revolutionary. Kuhn advocated this view (1976), as have in more recent times Siegfried (2002, 1968) and Chang (2012). Chang's view has sparked recent debate over how to interpret this period and I focus on his view with an eye toward advancing the debate.

Chang calls his reading of the Chemical Revolution normative pluralism (Chang 2015, 2012). This reading claims that scientists work in different systems of practice, each of which has its own set of methods, goals, and explanations. These systems are incommensurable and cannot be evaluated in terms of one another, which may bring to mind Kuhnian paradigms (Kuhn 1976). However, unlike a paradigm a system of practice can co-exist with another system. A system of practice also offers an analysis based on activities first and propositions secondarily, if at all. Another key difference is that Chang makes a normative claim about the number of systems being practiced. We should, Chang argues, promote a variety of systems and resist the imperialist tendency to follow just a single system.

Kusch (2015) rejects Chang's reading while not abandoning the anti-realist stance. Chang (2015) has acknowledged some of the concerns, so I will only mention two that remain contentious: the normativity and role of social factors (and I shall return to a more philosophical examination of these views in chapter 5).

One crux of the debate is the normative element: why should the revolution taken the actual course that it did? Or, was it justified? Kusch argues that there were good reasons, social reasons, for adopting the oxygen system of chemistry. To do this, Kusch appeals to experimental work in Germany that won over chemists to the oxygen system. He uses Hufbauer (page 77) to argue that social factors provided sound motivation for adopting oxygen chemistry.[24] Chang has responded (Chang 2015) that he is making a

[24] Hufbauer actually claims he is assuming a social analysis and does not claim to be arguing for it. It seems unlikely that Kusch can actually use Hufbauer's research as evidence for a social reading given this admission.

normative claim about the Chemical Revolution: scientists should have been pluralist and should not have been monist. The social motivation does not provide sufficient reason to abandon a system of practice. Despite this exchange, both Chang and Kusch think revolutionary change marked this period, i.e. there was extensive experimental and conceptual revision. They are both consequently committed to a pluralist reading of the Chemical Revolution.

4.1.2 The Revolution was not very Revolutionary

Both Kusch and Chang consider the Chemical Revolution a period of drastic change. Klein (2015) criticizes this interpretation, arguing instead that this period in chemistry saw more gradual change. She is not alone. Holmes (1995), Multhauf (1962), and Chalmers (2013, 2012) have argued in different ways for thinking change during the Chemical Revolution was more gradual.

There are two arguments for this reading. One is that chemists throughout the 18th century were committed to the same ontology of substances; Klein and Chalmers in particular defend this view. The other argument is that the methods chemists used during this period do not change in a way that resembles a revolution. We might attribute this view to Multhauf. If one accepts this broad view that there was no revolution, we must consider the changes we see during this period as relatively minor and part of a more continuous practice of chemistry.

Chang responded (2015) to Klein's criticism by acknowledging that there was indeed no drastic ontological change—and hence he agrees there was no revolution in this regard—but he argues the changes in methods, standards of judgement, and semantics were quite drastic and if we defined a revolution in these terms, this period was revolutionary.

From this debate on how to characterize the chemical revolution, there are several main points of contention I wish to distil out.

1) Was there a Revolution at all?
2) What was the disagreement about?
3) Was theoretical change during the Chemical Revolution well motivated?

A perspectival analysis will provide some answers to these questions. I will begin by focusing on a few particular experiments and exchanges between three natural philosophers: Cavendish, Lavoisier, and Kirwan.

4.2 Eighteenth Century Chemistry: A Brief Study

The study of acids was intimately linked to questions about the behaviour and nature of airs. Chemists were particularly interested in how acids were related to different airs and to metals both in terms of how these different substances reacted, but also in terms of how each was composed. It is partly in this context that the question of phlogiston arose and it was the study of acids that led Lavoisier to propose a new principle, the principle of acidity, which he called oxygen. I will discuss the general problem of acids and then discuss a series of experiments conducted by Cavendish, Lavoisier, and Kirwan. Cavendish began this work in the 1760s and this predates the experimental work of Lavoisier and Kirwan that I will discuss by at least a decade.

There are several advantages to focusing on this early acid work. The first is that the experimental context became incredibly complex once phlogiston became more hotly debated later in the 1780s. Part of the complexity stems from interconnected issues of acidity, calcination, combustion, and the composition of water and metals. The early acid experiments were less muddled by this complexity. The second advantage is that this subject is relatively self-contained and shows clearly the differences in experimental practice between several very influential chemists who had very different beliefs and offered very different explanations. The final advantage is that all chemists involved had extensive common commitments. Questions about qualitative versus quantitative approaches and principlism versus compositionalism do not need to arise here because all were quantitative and all were compositionalist. This common ground narrows and emphasizes what precisely the differences were between these approaches to the study of acids. I will now discuss what the questions were associated with acids, then analyse the different approaches that Cavendish, Lavoisier, and Kirwan took. So from the start I am providing an analysis that suggests the Chemical Revolution was not very revolutionary. My analysis will suggest identity played a central role in change and disagreement.

4.2.1 Acids

In the 18th century, the acids were contrasted with the alkalis or bases. A substance was an acid if it effervesced when combined with an alkali and when combined, these two substances formed a neutral salt (Siegfried 2002, chap. 4). The effervescence produced an air and the acids therefore, along with combustion, provided a way to study the connection between solids and airs or elastic fluids. Acids could dissolve metals and this property in particular gave them a special role in experiment. Metals also underwent some transformation, becoming a calx or rust. Cavendish did some early work in this area, but acids came to play a central role in experiments during the late 18th century.

Crudely put, there were two general accounts of the relationship between acids and metals, one offered by phlogiston chemists, the other by antiphlogiston chemists. The former claimed that metals are compounds formed by a calx and phlogiston. When the phlogiston is released, the calx remains. The antiphlogiston account (also called oxygen or French chemistry) claimed that metals were elements and acids compound, formed of oxygen and hydrogen. When combined with metal, the acid decomposed and the oxygen binds with the metal to form a calx and the hydrogen is released as a gas, which was called inflammable air for a time. The crux of the difference between these two accounts was what was simple and what was compound. This difference began to take shape with the early work of Henry Cavendish.

4.2.2 Henry Cavendish

Henry Cavendish (1731-1810) was a wealthy aristocrat who took a serious interest in natural philosophy. He seemed to have few interests apart from his scientific work, which was thorough, precise, quantitative and published with care and caution. He worked on a number of topics in addition to chemistry, such as electricity and meteorology. Cavendish's work in physics in particular is well known and perhaps more famous than his contributions to chemistry. He found a scientific community in the Royal Society of London, though his experiments were informed by and responsive to scientists from farther afield as well, including scientists from Scotland, Germany, and France. Cavendish did his chemical work in his laboratory in Westminster, a laboratory that his father likely started. Some of Cavendish's earliest work in chemistry was with arsenic. His work quickly expanded to include pneumatic chemistry and the study of acids.

Cavendish's initial interest in acids stemmed from an interest in arsenic because, although considered a metal, arsenic did not behave as other metals did (Cavendish 1921, 298). This was not the only motive, however, for his interest in acids. The following experiments I discuss concern the production of a type of gas Cavendish called "factitious air" and his interest in the properties of this air was strong. Factitious air was any gas that could be bound in a solid state. Joseph Black (1728-1799) worked on this subject and was an important inspiration for this project.

4.2.3 Cavendish's Early Study of Acids

To study the behaviour of acids and of metals, Cavendish used laboratory equipment that allowed him to combine substances with great control and to measure the products of the experiment (1766, reprinted in 1921, 77). I will describe his apparatus in some detail because it will illustrate the great similarities in experimental practice that he shared with Lavoisier.

For the work I will discuss, Cavendish used a bottle to hold the reacting substances. To the top of the bottle he affixed an S-shaped glass tube. This tube curved down through a large vessel full of water and then up into another bottle, this one inverted. This second bottle he filled with water and inverted so that the bottle opening was submerged in the vessel of water. In essence, Cavendish connected two bottles with a glass tube and ensured there would be no leakage by submersing key parts of the system underwater. As gases (or airs as he called them) were produced in the first bottle, they would travel through the glass tube into the second bottle, where they would displace water. Based on how much water was displaced, Cavendish could measure how much gas was produced.

With this apparatus, Cavendish investigated the reactions of three metals with three acids. The metals were zinc, iron, and tin. The acids were vitriolic acid (sulphuric acid), spirit of salt (hydrochloric acid), and nitrous acid (nitric acid). He dissolved the metals in the acids, one acid and one metal at a time, in the first bottle and captured the gas produced in the second bottle. Generally, he found inflammable air (hydrogen gas) was the product. Or, when not inflammable air, some kind of acidic fumes (Cavendish 1921, 78–79). Although he does not here discuss how he identifies the gas as inflammable air, we might infer from later passages (1921, 80) that Cavendish did so by observing whether the gas ignites when lit with a flame. Cavendish measured precisely by weight how much metal he used and how much gas was produced.

We can see from this paper that Cavendish was highly quantitative, something he was well known for (Mccormmach and Jungnickel 2016, 171). He starts his experiments with careful measurements and diligently weighs the results. This clearly shows he is familiar and has great facility with quantitative chemistry, a skill often attributed to oxygen chemists, but less often to phlogiston chemists.

Cavendish offers the following interpretation (1921, 79) of his experiments on these acids and metals:

> It seems likely from hence, that, when either of the above-mentioned metallic substances are dissolved in spirit of salt, or the diluted vitriolic acid, their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are dissolved in the

> nitrous acid, or united by heat to the vitriolic acid, their phlogiston unites to part of the acid used for their solution, and flies off with it in fumes, the phlogiston losing its inflammable property by the union. The volatile sulphureous fumes, produced by uniting these metallic substances by heat to the undiluted vitriolic acid, shew plainly, that in this case their phlogiston unites to the acid; for it is well known, that the vitriolic sulphureous acid consists of the plain vitriolic acid united to phlogiston

Cavendish interprets this experiment as showing that two types of acid—spirit of salt and vitriolic acid, dissolve both zinc and iron and when they do, an inflammable air is released and this air is phlogiston.[25] Crucially, the metal, and not the acid, supplies the phlogiston that is released. In its gaseous state, the phlogiston forms inflammable air, which can be ignited by flame. This would be nicely consistent with the idea that phlogiston is involved in all forms of combustion. However, if the acid is *nitrous* acid, the phlogiston combines with some part of the acid once the metal is dissolved and forms the acidic fumes that Cavendish isolated. As it joins the acids, the phlogiston loses its inflammable property and therefore cannot be ignited by flame. At the end of this passage, Cavendish appeals to what he takes to a well-known fact: that vitriolic sulphurous acid is formed by the combining phlogiston with vitriolic acid. We can assume that he takes this for granted and is not directly testing it as an experimental hypothesis.

The classificatory choices Cavendish made, i.e. the way he individuated substances, was based upon what he was able to isolate, in this case inflammable air. Because the reaction of metals and acids seem to produce a product that can be isolated, it is natural to think that metals are complex substances that give off the inflammable air. Cavendish classified metal as complex on the basis of what the experiment produced. His choice of what to measure led him to this conclusion. As we will see shortly, this is a markedly different strategy for classifying metals when compared with Lavoisier's work.

It is tempting at this point to follow Blumenthal and Ladyman (2017) in ascribing to Cavendish two "mistakes."[26] One was neglecting to weigh everything he should and the other was to appeal to a hypothetical entity, phlogiston, in analysing his results. It is a temping analysis because, to the modern eye, it is natural to weigh everything and phlogiston is not hypothetical, it is non-existent. However, we should resist this temptation because it is subtly anachronistic. Although Cavendish did not weigh all he could have, there is scant evidence that his contemporaries identified this as a mistake, suggesting his actions fit contemporary scientific norms. It is also difficult to see how

[25] This is not the only interpretation of the relationship between phlogiston and air during the 1770s. Priestley (1775), for instance, does not associate it with inflammable air.
[26] I am grateful to an anonymous referee for bringing this to my attention.

appealing to phlogiston could be a mistake. Phlogiston was accepted as a legitimate explanation since Stahl (1730) and there was as yet no salient reason to *avoid* appealing to phlogiston. There is also a more general problem with an analysis that calls these "mistakes." It suggests that scientists can conduct their work while making no assumptions, taking no background knowledge for granted, and letting no hypothesis go untested at any time. Such an ideal just does not sit well with the practicalities of science. Every scientist must take at least some things for granted.

Nevertheless, Cavendish's explanations for his results stand in marked contrast to Lavoisier's work, which was in other ways strikingly similar, even though it began a decade later.

3.1.2 Lavoisier's Lime, Chalk, and Acid

Lavoisier's early work on acids was motivated by an interest in how elastic fluids became fixed and separated from inelastic substances or, in modern terms, the nature of gases binding and forming solids (Lavoisier 1776, 221).[27] Cavendish, recall, called these same substances factitious. The similarity between these interests should be striking: both were investigating the relation between gases and solids. To do this, Lavoisier conducted a series of experiments that resemble very closely Cavendish's work, with a subtle difference that led to very different conclusions.

For the first experiments, Lavoisier dissolved iron power in nitrous acid to its saturation point (1776, 293). He carefully weighed both the acid and the iron before mixing and diluting the solution with distilled water. He separated out two portions of this solution and added chalk to one, lime to the other (1776, 294–95). Upon adding the chalk or lime, iron rust precipitated out of the solution. Had he not added the chalk or lime to the iron solution, the iron would not have precipitated out and would have remained dissolved. He uses lime and chalk as a kind of control; the results cannot be just a peculiar feature of lime, for example, because the results are similar when using chalk.

The point in discussing such detail is to show that thus far Lavoisier's work is very similar to Cavendish's, apart from precipitating the rust. Lavoisier has dissolved a metal in an acid, just as Cavendish did. He also weighs his starting ingredients and his results. However, Lavoisier is at this point more interested in the metal than the vapour and makes an important departure from Cavendish. Lavoisier washes the iron rust in distilled water and weighs it (1776, 295). He finds that the metal rust, the calx, is heavier than the

[27] Lavoisier had previously worked on other topics in chemistry (see Guerlac (1966) or Holmes (1988) for example), but the acid work is particularly relevant here because of its connection to Cavendish and because Lavoisier's evolving views on oxygen emerge here.

metal was before being dissolved in the acid. This is true for both the lime and chalk solutions, though the chalk solution produced heavier iron rust than the lime solution. Cavendish does not isolate and weigh the calx he produced. Lavoisier concludes the following:

> The results of these experiments are, 1st, that iron and mercury dissolved in the nitrous acid, acquire in general a remarkable increase of weight, whether they be precipitated by chalk or by lime. 2ndly, that this increase is greater in respect to iron than to mercury, 3rdly, that one reason for thinking that the elastic fluid contributes to this augmentation is, that it is constantly greater when an earth is employed saturated with elastic fluid, such as chalk, than when an earth is used which has been deprived of it, as lime (Lavoisier 1776, 295).

Calxes, such as iron rust, are formed from metal and an elastic fluid and are therefore compounds. Because the calx is heavier than the metal, it stands to reason that there is more substance to the calx than the metal when the quantity of metal is sufficiently controlled. Because the amount of metal was controlled in this experiment, the extra weight must have come from somewhere. Lavoisier attributes the gain in weight to the elastic fluid in part because chalk contains more of it than lime. Recall that the iron rust produced by using chalk was heavier than the rust produced by lime. Lavoisier demonstrated this difference between lime and chalk in a series of earlier experiments that I will not discuss in detail here (Lavoisier 1776, chap. 1).

At this point Lavoisier has concluded that metals are simple and calxes compound, which is opposite to the conclusion that Cavendish reached. Lavoisier made no appeal to phlogiston, though he also as yet makes no reference to oxygen, except as the "elastic fluid" that binds with metal to form rust.[28] But the crucial difference is that Lavoisier classified metals as simple substances whereas Cavendish classified them as complex. This is the crux of the difference between these interpretations, though other important differences exist. The problem is therefore a classification and identity problem and it emerges from a heavily shared experimental and classificatory practice.

So far I have shown that classificatory differences emerged in chemistry over the problem of acids and metals, which fits neatly with the perspectival emphasis on taxonomy that I defended in section 1. I have not as yet shown any disagreement. I now turn to this topic to show that not only was there extensive shared background prior to

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[28] Lavoisier choice of terms changes. Eventually he uses the term "oxygen" (Lavoisier in Best 2015) but in earlier work we can find him referring to an "elastic fluid" (1776) or "pure air" (1777).

the disagreement, there was also a shared interest in resolution and in establishing how best to approach explanation in chemistry.

4.2.6 Oxygen Confronts Phlogiston

Cavendish and Lavoisier, although engaged in very similar projects, were unaware of one another's experiments and alternative conclusions about the nature of metals. This difference becomes apparent later and is particularly striking in the work of Richard Kirwan (1733-1812) who, like Cavendish, was a phlogiston chemist, but communicated regularly with oxygen chemists.

By the time Kirwan (1787) addressed questions associated with metals and acids, the differences between Lavoisier and phlogiston chemists became far more complex and connected to a number of related problems. One striking criticism that Kirwan levels against Lavoisier concerned the basis for classifying metals and calxes. By this time oxygen chemistry became much more explicit and Lavoisier no longer uses "elastic fluid" to discuss a constituent of calxes, but uses "oxygen" instead. Acids, both phlogiston and oxygen chemists claimed, were compounds. The oxygen chemists claimed they were formed from oxygen combined with a base, whereas phlogiston chemists claimed that phlogiston was a component of acids.[29] This is a departure from Cavendish's earlier work, which did not consider phlogiston a component of the acids.

Kirwan's main criticism comes from his 1787 *Essay on Phlogiston.* This text provided the most recent and powerful defence of phlogiston theory. Lavoisier's wife, Marie-Anne, translated Kirwan's text into French. Added to Kirwan's passages were responses from leading antiphlogiston chemists, including Lavoisier. The French version with those responses was translated back into English two years later by William Nicholson of London. A crux argument in the text concerned affinity tables and how Lavoisier used them. It is worth discussing these tables in detail because they were an important part of chemical explanations, but only for a limited time. I discuss their history and why Kirwan thought they were a problem for Lavoisier.

4.2.7 Affinity Tables and Their Use

Affinity tables were a widely used explanatory tool in 18th and early 19th century chemistry (Holmes 1962).[30] Klein has argued (1994) that affinity tables formed the basis of the

[29] Kirwan offers a brief discussion of these two views of the composition of acids (Kirwan 1789, 38–39).

[30] As Holmes points out, the concept of affinity had different uses. One was to explain the combining of different substances. Another use was to provide a systematic ordering of substances, as in the affinity tables.

concept of composition during this period. The first affinity table was constructed by Geoffrey (1718). The table was made more systematic by Bergman in 1778.[31] The use of these affinity tables declined after concerted attacks by Berthollet that began shortly before 1800.[32] Before its decline, the basic principle of the affinity table was that substances are attracted to one another and combine because of their affinities and different substances have different, but constant, affinities. For example, if three substances are mixed, then the two with the strongest affinity for each other will combine, to the exclusion of the third. These tables were initially interesting for explaining the behaviour of acids, metals, and bases and for providing order to the wide range of substances that chemists studied.

Lavoisier was interested in using affinity tables, not to accurately predict reactions, but to provide order and explanation. In this case, affinity tables could explain how oxygen, the elastic fluid of his early experiments, behaves. Oxygen, as the principle of acidity, has an affinity for other substances and therefore it was quite natural to explain its behaviour with the already widely used affinity tables. Substances combust, so this explanation goes, because of their affinity to oxygen and they stop when the source of the oxygen is at equilibrium with the combusting substance (Holmes 1962). Between Bergman and Berthollet's criticism of affinity tables, a number of chemists—including Kirwan, Morveau, and Fourcroy—worked on filling in the tables with more detail and more substances and attempting to quantitatively measure affinity (Kirwan 1789, 173).

The problem was that Lavoisier's tables had many exceptions that could be easily demonstrated through experiment. This suggested Lavoisier's table was inadequate. Without affinity tables, oxygen chemistry becomes much less compelling because it is unclear why a substance would combust and why it would stop combusting. The phlogiston chemists could neatly provide this explanation by appeal to phlogiston: a substance burns when phlogiston is released and stops when the phlogiston is depleted. But why should oxygen bind with iron when affinity tables suggest it does not? And why should combustion stop at any point once it has started? Kirwan writes:

> Besides, though iron and zinc are the only metals which by Mr. Lavoisier's table have a greater affinity to the oxigenous principle, than inflammable air has to that principle; yet inflammable air is also set loose during the solution of other metals, which by that table have a weaker affinity to the oxigenous principle than inflammable air has to it (1789, 176).

Kirwan points out that that oxygen chemistry is committed to the idea that when metals are dissolved in a water and acid solution, inflammable air is produced. Their explanation

[31] This was translated into English a few years later (Bergman 1785).
[32] For a discussion of the fate of affinity tables, see (Holmes 1962).

for this is that because water and acids are at least partly composed of inflammable air and oxygen.[33] The metal binds with the oxygen to form a calx and the inflammable air is released as a vapour. However, there is a tension in the oxygen account, so Kirwan's criticism goes, because Lavoisier's affinity table suggests that only iron and zinc have a strong enough affinity for oxygen to separate it from the inflammable air, and yet when other metals are mixed in acid, inflammable air is released.

Kirwan's alternative explanation (Kirwan 1789, 45) is that when water and metal are combined, the water drives inflammable air out of the metal. Calxes are what remains after the inflammable air is driven off. The water explains the fact that calxes are heavier than metals. This explanation may be reasonable because water is heavier than inflammable air. This explanation does not encounter the contradictions in affinity tables that Lavoisier's explanation must face. We can see here that Kirwan's explanation is more consistent with Cavendish's early work in that both Cavendish and Kirwan argue metals are compound substances and that part of their composition includes inflammable air *and* that inflammable air is phlogiston.

There are two things to note about Kirwan's explanation of the composition of metals. First, the notion of which substance is simplest is somewhat obscured because both calxes and metals are not simple and isolated substances (metals are partly composed of phlogiston and calxes have great quantities of water in them). This suggests Kirwan was not as interested in composition as he was in other topics like affinity tables. Second, Kirwan did not take issue with Lavoisier's weight data. He accepted that calxes were heavier than metals and that this was puzzling. But he did disagree over what type of inference this data permitted. Any conclusion, Kirwan seemed to have thought, required a consistency with affinity tables. Affinity tables, in other words, constrained the type of inferences one could make from the data, a constraint that Lavoisier did not recognize. The main point of difference between these chemists that I would like to emphasize is this: Lavoisier distinguished the elements from the compounds by weight and Kirwan did not. I will return to this point below.

Lavoisier responded to this worry about inconsistencies by claiming that the affinity tables were not meant to account for experiment directly, but to explain why different substances combust.[34] And not only are the tables not intended to predict, they *cannot* make predictions about most experiments directly because they have not been sufficiently perfected due to a practical limitation: affinities vary with temperature and

[33] A related problem is the question of the composition of water. Oxygen chemists believed water was compound while many phlogiston chemists, including Kirwan, believed water was simple.

[34] Lavoisier in (Kirwan 1789, 45)

temperature is not incorporated into affinity tables. To fully explain the results of experiment requires a specification of temperature, which would lead to a new affinity table for each incremental change in temperature.[35] Such a project is impracticable. So Lavoisier is not committed to affinity tables that provide consistent, experimentally verified predictions.

This discussion so far has shown two things. First, all chemists I have so far discussed were interested in the reactions of acids and metals and the gases produced. Second, there were important differences in the conclusions each chemist reached despite this common interest. The difference was in how these chemists thought substances should be individuated: a problem of classification.

4.3 Evidential Reasoning

Having discussed the details of this early work on acids, let's return to the three questions we began with, which were (1) was there a revolution; (2) what was the disagreement about; and (3) was change well-motivated. The easiest to answer is (2), which is that the debate was about classification, specifically the classification of elements.

The answer to (1) is more complex to address, but the answer will be no. A revolution suggests the extensive replacement of the conceptual or experimental practices with a new set of practices, but I have shown that there was extensive experimental and conceptual continuity between phlogiston and oxygen chemists. It is worth distinguishing here two senses of "revolution," one technical, one informal. A revolution in the technical sense concerns radical conceptual or experimental upheaval. This is the kind of revolution Kuhn (1976) was interested in. An informal use of "revolution" involves great change, but without specifying, or requiring, conceptual or experimental revision. Chemists during and since the 18th century referred to the great changes in chemistry as a "revolution," but this is informal and possibly even rhetorical. One can consider this period revolutionary, i.e. a period of great change, without requiring systematic conceptual and experimental revision. I have argued that there was not revolution in the technical sense, but there likely was a revolution in the informal sense.

Before discussing (3), I would like to argue that continuity during the Chemical Revolution cannot be explained away by differences in the experimental interests of oxygen and phlogiston chemists. It is tempting to analyse the differences between these chemists in terms of their interests and goals. Chang (2012) provides such an analysis. Although interests were undoubtedly important, as I will show in this section, the full

[35] Lavoisier also studied heat in other contexts and methods to quantitatively measure it.

story cannot stop with this explanation because of how goals changed and how those changes informed and were informed by other considerations such as experimental results and classification.

Cavendish, as a chemist working in the pneumatic tradition, was very concerned to isolate the gases produced by experiment. Some of his experimental choices can therefore be traced to the tradition in which he worked. The situation was more complex, however, because Cavendish had several evolving goals. The classification of "airs," or gases, became troublesome for natural philosophers when it came to the factitious airs, which possess some kind of important connection to more solid substances. This problem was probably a motivation for Cavendish, but it was not the only motivation. The unusual properties of arsenic were also a puzzle that motivated him. Cavendish's goals for his work in chemistry, I am suggesting by appeal to these cases, varied. Any one goal was not necessarily the only goal at play and no one goal necessarily guided Cavendish through his entire experimental practice. However, it is fair to say that his experimental pedigree (pneumatic chemistry) led him to ask certain questions and to use the relevant methods. Lavoisier also worked in the pneumatic tradition and so was also interested in isolating gases and determining their relationship to solid substances. But Lavoisier did choose to weigh aspects of his experiments that Cavendish did not, specifically the calx.

This emphasis on weight may be unsurprising given his interest in the balance, not simply as an instrument of measurement, but as a principle of natural and economic regulation. Bensaude-Vincent (1992) suggests the balance symbolizes for Lavoisier a kind of natural order. The weight that was conserved through reactions was just one example of a kind of natural regulation that provided a kind of balance and equality (*Ibid.,* p. 227). Given this background, a willingness to consider weight paramount is unsurprising. However, this inclination to measure all that could be measured did not exhaust Lavoisier's interests. Later, Lavoisier was also interested in distinguishing the compounds from the elements, those substances which could be decomposed from those which could not. This project eventually culminated in a collaborative revision of the nomenclature of chemical substances on the basis of elements (1796, 1787). So we might say Lavoisier was interested in a unifying principle for identifying elements and providing a classification on this basis, but was not as interested in problems like anomalous metals, even though he also took an interest in the connection between gases and solids.

Kirwan (1782) also had experience measuring weight and was skilled at it. However, Kirwan also worked extensively on affinity tables and took quite seriously the explanatory power an affinity table offered (Kirwan [1782]).[36] Given this interest, it is unsurprising that Kirwan was particularly concerned with Lavoisier's table and less concerned about the other features of oxygen chemistry. And this is the case even though Kirwan has a lot in common with Lavoisier. Both, for instance, find the weight gain of calxes important.

Despite these differences, the broadly shared experimental practice and taxonomy would have made it hard to avoid adopting oxygen chemistry in some form. I say "some form" because oxygen chemistry was not the only logical possibility, but in the second half of the 18th century, long-term adherence to the available alternatives may have been difficult. This brings us to question (3): was the disagreement well-motivated.

4.3.1 The Appeal of Oxygen

Chang (2012, sec. 1.2) claims that there was little reason during the Chemical Revolution to suppose weight could be used reliably in chemistry and that, as a consequence, Lavoisier's work was not as compelling as it may seem to the modern reader who is now accustomed to the conservation of mass. There is indeed a puzzle here: given that producing isolated products and weight were both well-established features of chemical practice, why prefer one over the other? Data, a typical form of empirical evidence, certainly fail in this case to provide the kind of straightforward confirmation one might expect. This is because different chemists chose to weigh different substances.

Despite these caveats, I will suggest that there was good reason for an 18th century chemist to use weight as a criterion for individuating reactions and substances because it was quite natural, even though it was also new and even though there was no rational compulsion to adopting weight. There are three motivations for this claim, which come not only from this historical case, but also from research by Multhauf (2015, 84).

First, weight is quantitative, precise, and repeatable; measurements can be retaken with the same result, substances need not be isolated to be weighed, and it is objective (different chemists weighing the same substance can get the same result). These are some reasons for thinking that weight offers a perfectly good kind of data to be used as evidence and that it could be a reliable method for determining what substances one had. Indeed, all chemists I discussed weighed substances quite diligently. It was not weight full stop that distinguished Cavendish, Lavoisier, and Kirwan. What differed was the persistent application of weight as a criterion for distinguishing the element from the compound that made Lavoisier's work stand apart.

[36] Affinity tables were not his only project; he was principally concerned with defending phlogiston against oxygen chemistry.

Second, weight was already a well-established part of chemistry methods. We can see that Cavendish, in addition to Lavoisier, was skilled at measuring the weight of different substances. He was not alone. Kirwan and many of the other leading chemists used weight with high precision.[37] It would have been, this suggests, quite natural for Lavoisier to use weight to track oxygen and determine the composition of different substances. It was a new part of the chemistry practice, but not very unlike what others were doing. Indeed, despite initial scepticism, many chemists, including Kirwan, came to accept Lavoisier's work quickly.[38] It would have been difficult for chemists to maintain scepticism of weight data. Weight was, after all, already a trusted tool.

Thirdly, not only was weight quantitative and well established, it also fit neatly with intuitive conceptions of simplicity and complexity. It is quite natural, even trivial, to think that simple things weigh less than more complex things, all else being equal. If you take any two things and put them together, be it coin, fruit, or horses, the sum weighs more than a single component. This point is quite general. The only exceptional thing that Lavoisier does with this intuitive idea is to consider it paramount and the basis of determining what is elemental. Weight takes priority, say, over affinity tables. The consequence is that someone like Lavoisier is perfectly happy to tolerate exceptions to affinity tables, something Kirwan was unwilling to do because he considered affinity tables much more important. Recall that Kirwan's explanation of the weight gain of calxes entailed that the notion of simple substance played little or no role, a consequence he accepted, but which Lavoisier would have found intolerable. It is interesting to note that Kirwan eventually came to endorse oxygen chemistry.[39]

I have so far argued in this section that it was reasonable to follow Lavoisier's use of weight and perhaps difficult to endorse an alternative. But this was not conclusive: dissent was not irrational, just difficult to maintain. There are two lingering questions. First, have I set aside important complexities and underemphasized important differences in experimental practice? Second, why not be pluralist; couldn't both oxygen and phlogiston chemistry carry on independently?

Setting aside as many complexities as I did was justified because although it was a complex period, complexity is not what made this disagreement distinctive. Any scientific disagreement has the potential to become complex. Within this complexity there were certainly important differences and this paper did not explore all of them. However, my

[37] See (Multhauf 1962) for a discussion of how widely used the balance was during and preceding the Chemical Revolution.
[38] For a discussion of Kirwan's work and his eventual conversion, along with Berthollet, to oxygen chemistry, see (Mauskop 2002).
[39] For a discussion of Kirwan's later views, see (Mauskop 2002).

purpose was to show the neglected similarity between these chemists. Of course there were differences, but in light of the similarities, they were not as fundamental as some pluralist readings suggest.

For the second question, why not be pluralist, there are some reservations we may have. The first reservation with pluralism is that both phlogiston and oxygen chemists recognized the same problems and were interested, generally, in solving those problems. The composition of metals was not just of interest to Lavoisier, Kirwan thought it worth exploring and arguing about. The projects were too closely related for either to ignore the other's work. Oxygen chemistry influenced and stimulated phlogiston chemistry and vice versa. As a consequence, each chemist was able to respond to the other because of the significance each experiment had to the overall project of trying to classify and understand a set of substances. If the debate is indeed structured as I am suggesting, then it is hard to imagine how, say, an oxygen chemist could feel satisfied ignoring a phlogiston chemist.

The second problem with pluralism is that, as I argued, there was extensive shared background to oxygen and phlogiston chemists. All worked in the tradition of pneumatic chemistry, all used broadly similar methods that involved isolating substances, weighing them, and exploring their properties. They were also all exploring substances that were not far removed from everyday life. Metals, water, and acids are not obscure substances and there was no disagreement about what were instances of these substances. The issue was how to classify them, as I argued earlier. The classificatory problems were not particularly obscure either, even though solving them was complex and difficult. Given the complexity of the task facing these chemists, it was certainly reasonable for there to be what we might call a period of uncertainty. But this is not a permanent state. Eventually, after more debate, experiments, analysis, and the revisiting of old work, the debate became resolved.

4.4 The Historiography Revisited

I claimed there were two broad approaches to the Chemical Revolution. The first approach takes the idea of revolution quite seriously and suggests there were drastic changes in the transition from phlogiston to oxygen chemistry. The other view is opposite: the changes were not drastic and there was a great deal of continuity between phlogiston and oxygen chemistry. Emerging from the tension between these accounts were three main questions that could be made more specific. The questions were: 1) Was change well motivated? 2) Were chemists part of the same or different practices? 3) What was the disagreement about?

The perspectival analysis gives these answers: 1) the change was well-motivated; 2) there is an important sense in which these chemists were part of the same practice; 3) the disagreement concerned taxonomic problems that emerged from experimental results. Let's look at this answers in more detail and compare with the other analyses.

My analysis is much more closely aligned with Klein in that I have suggested a great deal of continuity during this period. I suggest, then, that the answer to 2) is that chemists were part of the same practice. The changes were not, therefore, very revolutionary. I also argued that non-social factors were important drivers of change, and in answer to 1), that the change was well motivated. But I agree with Chang that the social factors did not provide a well-motivated force for change. I disagree with Chang in that I argued that phlogiston and oxygen could not long persist simultaneously. In answer to 3), I suggested the disagreement concerned how to distinguish elements from compounds and that Lavoisier suggested this be done by weight to the exclusion of other considerations.

4.5 Conclusion

This chapter illustrated a perspectival interpretation of scientific disagreement that advances existing interpretations of this period. In 4.1 I indicated several particular areas where the contention in the literature is acute. These concerned how deep the disagreement was (was it revolutionary?), what the disagreement was about, and how we should evaluate the outcome. Section 4.2 examined several experiments during this period and 4.3 evaluated the evidence different chemists offered for the interpretation of their experiments and provided some analysis. In 4.4 I revisited the contemporary, philosophical disagreement. Perspectivism has provided a basis for thinking about disagreements that are serious, but not as insurmountable as other views suggest. My view acknowledges the historical context in which the disagreeing parties worked, but also accommodated evidential reasoning more naturally than other views.

Through this discussion of the Chemical Revolution, I rejected pragmatic and relativist interpretations of this period. But where exactly do these views diverge and what motivates the divergence? The next chapter evaluates pragmatism, relativism, and perspectivism more abstractly and in relation to one another. All three views are motivated by, among other things, an attempt to better understand disagreement. I will argue against the first two and in favour of the third.

5
Three Views of Scientific Practice

Abstract
In this chapter I argue in favour of perspectivism against scientific Changian pragmatism and relativism. I first motivate, and then criticize, Changian pragmatism and relativism before showing how perspectivism avoids criticisms faced by the other views. All three views are brands of pluralism that purport to offer insight into disagreements. The two views I reject, however, fail to account adequately for the objectivity, continuity, and broad appeal scientific explanations have. These problems emerge because each view takes a subjective attitude toward the evaluation of scientific products. However, these problems are avoided, or at least better understood, by framing some kinds of disagreements as taxonomic, and therefore identity, problems. Perspectivism as I defend it attempts this framing.

5.0 Introduction

How should we think about scientific disagreements? Should they be resolved and if resolution is possible, is it rational? This chapter presents three different accounts of scientific practice that purport to help us understand disagreements and controversies by answering these kinds of questions. The first two accounts I will suggest face similar and critical problems. These deficiencies are avoided by the final view, perspectivism, which I defend. We saw some of these differences emerge in the previous chapter about the Chemical Revolution, but here I will examine more abstractly the different commitments these views have and why we might be tempted to accept, or reject, those commitments.

In section 5.1, I discuss a brand of Changian pragmatism that uses action as the analytic tool for understanding disagreement. Section 5.2 addresses relativism with respect to situated judgements that are justified within an epistemic system. And in section 5.3, I defend a version of perspectivism that focuses on scientific taxonomies and their applicability and relevance to scientific practice.

5.1 Changian Pragmatism

In this section, I present an argument for pluralism about scientific disagreement. Chang chiefly defends this view and calls it pragmatic. It is inspired by the American pragmatist tradition and a particular feature of the view that makes the pragmatic label appropriate is the emphasis on the practical abilities that scientists have. Chang evaluates such activities in terms of human interests. Science is therefore interest- or aim-driven.[40]

This view of pluralism provides an important foil because, like the perspectival view I shortly defend, it is motivated by disagreement and it emphasizes activity, but there is a critical difference. Changian pragmatism takes a strong subjective and internal attitude toward evaluating scientific claims. This attitude shows itself in several premises and is deeply problematic. My examination of the pragmatist argument will show the role of evaluation in the account and why it is a problem. I take it that a possible argument for Changian pragmatism (broadly understood along the lines indicated by Chang) goes as follows:[41]

1. There are different epistemic systems of practice.
2. Judgements and beliefs are evaluated within specific systems of practice according to internal standards.
3. Systems of practice and their associated aims cannot always be ranked or evaluated in terms of one another.
C: We should be epistemic pluralists

In what follows, I look at the individual premises of these argument, highlights points where I think the pragmatist has a point, and flag some worries one might have.

5.1.1. Premise 1: There are different epistemic systems of practice

This premise does much of the work in making this view pluralist and distinctly pragmatic. It is the claim that scientists perform a number of activities, all oriented systematically toward achieving a set of aims. The pragmatist claims each scientific community has a set of aims and must engage in various activities in order to achieve those aims. These activities form a coherent structure because they all relate systematically. This structure is an epistemic system and, consequently, can also include concepts, beliefs, models, explanations, and other epistemic features of a scientist's context. The view is

[40] Interests and aims can take different forms, but they are interchangeable because both concern the suitability of a method or explanation *relative* to a problem, task, or phenomenon that scientists endeavour to investigate. I take Chang (2015, 2012) to be broadly committed to this characterization of pragmatism.
[41] Because this is the main version of pragmatism under discussion here, I will hereafter call it "Changian pragmatism." I indicate where there are exceptions.

pluralist because there are multiple independent and *autonomous* systems (Chang 2012, chap. 5). He describes his approach here:

> I propose to frame my analyses in terms of "systems of (scientific) practice" that are made up of "epistemic activities"…. An epistemic activity is a more-or-less coherent set of mental or physical operations…. and the coherence of an activity is defined by how well the activity succeeds in achieving its aim (2013, 15–16).

As an example, a scientist within such a structure must perform a number of activities in order to measure the mass of an electron. Thomson, one of the first to accurately measure this mass, performed a number of activities associated with his aim (Arabatzis 2006). He had to arrange his laboratory equipment using a cathode ray tube. He had to select and arrange the relevant metals to emit electrons and he had to connect an electrical source to the cathode ray tube. Once he managed to run the experiment, he had to calculate—based on how much cathode rays were deflected—what the mass-to-charge ratio was. This is just a brief characterization of the activities involved in a scientific practice. Each activity Thomson performed systematically relates to one another and these relationships are established by the overall aim of the experiment, to measure the mass of an electron.

One appeal of this pragmatic account is its apparent sensitivity to scientific practice and its aversion to abstraction. The pragmatist argues that by emphasizing the practice, we get a clearer picture of what scientists must know and do in order to answer questions, make calculations, predictions, and offer explanations. Knowledge of theory alone, for instance, is insufficient for understanding what scientists are doing and what role things like theories play in science. It follows Hacking (1983) in pushing the analysis away from representation and toward experiment.

Not only does this view avoid abstract reconstructions, it also gives credence to the diversity we see in scientific practice. Scientists just do seem to be engaged in different activities and these differences structure inquiry in important ways. Chemists, physicists, and biologists are all engaged in activities that systematically relate to one another, but scientists of one type perform very different activities from those of another. They also use very different sets of concepts, seek very different types of explanations, develop very different theories, and make evaluations under very different standards of success. These differences in practice are possible at least in part because there are different activities associated with each practice. Thomson's laboratory work was an activity scientists from other practices had no need to understand or perform, just as Thomson had no need to perform activities associated with cultivating bacteria in petri dishes, an essential skill for some of his contemporary scientists.

Although this account seeks to capture the complexity of scientific practice, we might struggle to endorse it. The main problem is the emphasis on autonomy. Disagreement is meant to be a strong motive for endorsing autonomy, but many of the historical cases that provide that motivation, such as the Chemical Revolution, are not best characterized as separate systems. This is because those who disagree actually share a number of the same methods, activities, and explanatory aims, even though there are important points of disagreement. We saw this in the preceding chapter.

There are other ways of incorporating aims and pluralism into an account of science that does not fall afoul of these deep divisions. Mitchell's integrative pluralism (2003, 2002), for instance, shares pluralist roots and a pragmatic interest in aims, but with the possibility of integration. The key difference as I see it is that Mitchell's pragmatism situates aims and models into a broader scientific inquiry. This focus on methods avoids the kind of strong autonomy separate systems have under Chang's treatment of pragmatism. This suggests one can take an interest in the fact that science has a plethora of aims and activities, but without endorsing this commitment to autonomous systems of practice, further suggesting science can be insightfully examined with an eye toward aims and activity, but without commitment to autonomy. So while this premise does offer some insight into how we can approach an analysis of science, it goes too far in committing to autonomy.

5.1.2. Premise 2: Judgements and beliefs are evaluated within specific systems of practice according to internal standards

This premise is about internal standards for evaluating claims. The pragmatist claims here that whether scientists have succeeded in achieving their aims is internal to the practice. Chang calls this the coherence of the activity (2017, 6–7): an activity is coherent if it allows the scientist to achieve the relevant aims. This requires a rather narrow construal of aims such that they are not shared across practices. Relativism shares a similar premise, but perspectivism does not. The difficulty here is that the pragmatist (and the relativist too) takes a subjective stance toward evaluation and subjectivity of this kind does not fit well with social, collaborative, and integrative features of science. Let's first examine why we might be motivated to accept this premise before looking at the grounds for rejection.

Part of the motivation for this premise comes from the pragmatist attitude toward success. Success is internal to a practice, so the pragmatist argues, because each practice has its own set of aims and so any potential success must be evaluated by that specific set. Consider Chang's Chemical Revolution case (2012, chap. 1), which I have also discussed at length in the preceding chapter. Briefly, the two explanations, one appealing to phlogiston, the other to oxygen, were incompatible. However, phlogiston chemists were interested in qualitatively explaining a broad range of phenomena whereas oxygen chemists were interested in explaining quantitatively a narrow range of phenomena. Because they had different explanatory aims, each explanation could equally and independently be successful. This equality of success was possible because no explanation perfectly satisfied all aims (2012, 293:17–18).

The key claim here is that success cannot be determined from another practice. Once someone in a practice has chosen what he or she wants to explain, a process that is *subjective*, the evaluation of his or her explanation depends entirely on that explanatory aim and on no other aim. Although this analysis gives puzzling results when applied to cases of disagreement, it emerges from a very intuitive idea. That idea is that in everyday life, when we perform an activity, we evaluate that activity on the basis of what we set out to do. To use a very mundane example, I may have the aim of cleaning the kitchen. My actions of mopping floors and washing dishes are successful if they result in a clean kitchen and they otherwise are unsuccessful. These actions are also unsuccessful if my aim was not to clean the kitchen, but to repair a desk. The pragmatist strategy here, we might say, is to take a very intuitive idea of action in the everyday context and more explicitly apply it to scientific explanation generally.

Chang seeks to link success with truth by appeal to coherence (2017, 11). A statement is true if belief in it is necessary for performing coherent activities that achieve specified aims. Success provides the anchor or constraint that attaches systems of compatible beliefs to the world. Chang's main motivation for this account of truth stems from his emphasis on action and rejection of propositional analyses of science. We might be motivated to accept Chang's definition of truth because it is epistemically accessible, i.e. scientists are in a position to make judgements about what is true, based on what activities are successful. (Chang 2017, 6).[42]

So far I have attempted to reconstruct the motivation Chang has for evaluating science by internal standards of success. I now turn to some tensions with this premise.

[42] Although I agree that truth should be epistemically available, the point is contentious. Psillos (1999 Introduction) explicitly rejects the idea that truth is epistemic at all. Within such accounts, truth might play a regulative, indirectly accessible role in science that licenses inferences to the best explanation.

There are two I discuss here: the first concerns how widely shared the aims are; the second is about truth. Both of them put pressure on the idea that evaluating science can be done internally, i.e. that it is as subjective as Changian pragmatism suggests.

The first worry is that aims may not be best thought of as internal. If they are not internal, then aims could be widely shared or recognized as legitimate aims worthy of pursuit. One way to make this worry more direct is to suggest that there are general aims all scientists have. One example is truth—a metaphysically more robust form of truth than coherence. But even if we found truth too committing, there are less metaphysically committing examples of widely shared aims. We might say, and I think it is reasonable to endorse the claim, that all scientists are interested in providing explanations that have as broad a scope and as much precision as possible. Sometimes these different aims are in conflict, but that does not preclude scientists attempting to achieve the best balance. If scientists do aim generally for the best balance between conflicting aims, then there is far less scope for thinking explanations are evaluated internally.[43]

There is still room for disagreement over how to strike the best balance, but if all scientists are engaged in finding that balance, then precision and generality are candidates for cross-practice aims or standards, i.e. standards that are not fully internal. This sits more naturally with how we might think scientific explanations feature in science: an explanation must have general appeal. If explanations did not have this generality, the relevance and power of scientific investigations would be mysterious and much smaller than we would expect. This will feature prominently in the perspectival view I defend later.

The second worry is that coherence sets the bar too low for truth, even with success as a constraint, and therefore it makes truth too easy to achieve. Say I set myself certain aims and develop an explanation that achieves those aims to my satisfaction. My aims might even include some form of compatibility with empirical evidence, so it need not be completely divorced from worldly constraints. Provided this explanation does not conflict with my other beliefs, I am entitled to believe that this explanation is true. Truth, I gather, consists in no more than this and it has the advantage of being epistemically available and metaphysically efficient.

However, there is something missing from this line of thought. Suppose I am interested in explaining moving lights I see in the night sky. I can solve this explanatory aim by positing spacecraft from other planets. I do not believe the only intelligent life is from Earth, so this explanation conflicts with no other beliefs and it also achieves my

[43] See Massimi (2017) for a recent discussion of balancing between conflicting explanatory virtues.

explanatory aim. This explanation could therefore be considered true by the pragmatist analysis and it even accords with some empirical evidence: there are moving lights in the sky that call for explanation. However, an explanation invoking alien spacecraft is implausible because there is no other evidence for aliens (note that one could not prove the non-existence of them) and there is extensive evidence for airplanes or satellites, which also have lights. Yet because positing aliens satisfies an explanatory aim with coherence, the explanation is true according to the pragmatist. We find ourselves in this puzzling position because to rule out implausible views, one might need to consider a broad range of evidence, but if aims are set internally, and subjectively, then one can be as selective as one pleases. There is no obligation to consider the fact that there is poor evidence for aliens and extensive evidence that airplanes fly at night with lights on.

Although simplistic, sufficient detail could be added to this toy example to show that success and coherence are weak constraints on scientific theorizing. We need a constraint that is not so easily specified subjectively and internally to a practice, otherwise extensive odd and unscientific claims could be true according to this coherence view.

To recap, the pragmatist of Chang's persuasion is interested in analysing science in terms of activities that are coherent. An activity is coherent if it is performed so as to achieve certain aims. One could use this to analyse activities— the things scientists do such as explain, predict, and measure—in terms of their success. Whether an activity is successful is relative to the aims a scientist has and these aims vary by practice. I suggested there were two serious worries we might have about evaluating activities in this way. The first was that aims are subjective and the other is that coherence is a weak constraint on truth when aims are subjective. Perspectivism will avoid these issues by offering a more robust and inter-subjective evaluative tool.

5.1.3. Premise 3: Systems of practice and their associated goals cannot always be ranked or evaluated in terms of one another

Premise 3 follows from premise 2 with one additional assumption. If activities are evaluated *only* by internal standards, then there are no extra-practice standards with which we can compare activities. The pragmatists here claim that systems of practice cannot be ranked because judgements are evaluated within these particular systems. There are no extra-practice criteria for giving greater weight to some aims over others. As such, it is possible for conflicting judgements to arise when two or more judgements are associated with different systems. In such cases, the conflict is really just apparent, because each judgement achieves equal success through the evaluation by each separate epistemic system. After providing some motivation the pragmatist may have, I will argue this premise is not warranted because historical evidence does not support it and because the basis on which we evaluate claims is more widely shared than the pragmatist requires, so there are historical and philosophical grounds for rejecting this claim. As for premise 2, the philosophical problem concerns internalism about evaluation.

One motivation for this premise is, strangely, that the alternative is harder to establish. The alternative is the claim that practices can be ranked and providing such a ranking demands some independent strategy that is not deeply sensitive to a particular practice. Changian pragmatism negates the need for such a strategy.

Another strength of this premise is that it provides an explanation for why past practices were abandoned, but without a "Whiggish" interpretation of the past, i.e. an interpretation that assumes the past is directed toward the future. The explanation is that past practices were abandoned because they failed by their own internal standards. For example, scientists have chosen not to pursue geocentric astronomy, but they have done so not because geocentrism fails by heliocentric standards or an external standard, but because geocentrism, *by its own internal standards*, has not allowed scientists to achieve their aims. The practice stopped yielding successes. This response gives us a way to reject systems without appealing to criteria external to the practice.

Despite these motivations there are two serious worries that I think the pragmatist does not have the resources to address. One worry is that historical cases do not actually provide the support Changian pragmatism suggests. Disagreements and controversies tend to be resolved, suggesting there is a way to evaluate conflicting claims in a compelling way. Emerging from the chemical revolution was a commitment to studying oxygen. It is difficult to imagine how we could characterize the debates during the Chemical Revolution as anything other than resolved. This calls for explanation, a call that cannot be answered with satisfaction by the pragmatist, who argues that there is no need for resolution because phlogiston and oxygen were equally successful. But if each was equally successful by their own standards, why was there disagreement and then resolution?

In addition to the historical worry, there is a philosophical worry, motivated in part by the historical worry. It strikes me that although there may be a number of values and aims in science, these values and aims are not practice-specific and are consequently more widely shared than Changian pragmatism requires. I also discussed this in the previous section and chapter 3. The worry is that it is hard to imagine, regardless of their practice, scientists rejecting the values of scope and precision. It is also hard to imagine how they could reject the need to explain the phenomena that fall under their purview. There is a parallel in this argument to Davidson's criticism of conceptual schemes (1984).

Davidson argues that merely considering another supposed scheme would require bringing that scheme within our own scheme, undermining the supposition that they could be different. Common to Davidson and the argument here is a scepticism about our ability to entertain candidates for other modes of thought, reasoning, or cognition. My point here is that without an ability to understand the appeal of a scientific explanation, we could not accept that it as scientific. And if that is correct, it is not so plausible that there could be rival systems with alternative modes of reasoning, i.e. that use very different standards for evaluating explanations.

There are still resources for motivating disagreement, even if we reject the pragmatist analysis. Chang claims that phlogiston chemists wanted to explain the properties of metals and the oxygen chemists wanted to explain weight data with more precision. However, it is not the case that oxygen chemists thought the properties of metals were not worth explaining, they just gave greater emphasis to other problems. We should not conclude, as the pragmatist does, that disagreement is to be understood in terms of different aims, rather we should understand it as different emphasis on aims, which is a much more superficial form of disagreement, one which does not preclude more widely shared aims and values.

There is some motivation for this premise that rival explanations cannot be ranked when they satisfy different aims. However, there are two serious worries, historical and philosophical, about characterizing disagreement in this way. I argued we are not warranted in accepting this premise.

5.1.4. Conclusions about Changian Pragmatism

I have provided motivation for several premises that support a form of pragmatic pluralism about scientific practice as defended by Chang. The conclusion of these premises is that we should be pluralist about science, specifically about the epistemic systems to which scientists belong, which Chang describes in terms of activities oriented toward achieving aims. This conclusion follows because if scientists are performing different epistemic activities (1) and if those activities are evaluated internally by different standards (2) that cannot be evaluated in terms of other standards (3), then we should be pluralist about those practices (conclusion). This argument gives an interesting, but ultimately unsatisfactory account of disagreement.

I suggested there were several serious worries about this account, mostly associated with internalism about success and evaluating science. They apply to premises 2 and 3. If these worries are as serious as I claim, then we may need to rethink pragmatic pluralism along different lines (perhaps lines more akin to those defended by Sandra Mitchell's integrative pluralism) and in so doing, we may find other perfectly good and valid reasons for endorsing pluralism. Until we do that, however, we should be very reluctant to accept pragmatic pluralism. Let us now consider another well-known argument for pluralism that takes the path of relativism. Ultimately this view shares some of the same difficulties we saw with Changian pragmatism.

5.2 Relativism

This section lays out and argues for another pluralist analysis of science: relativism. Unlike Changian pragmatism, relativism as discussed here takes a propositional approach, which may give it an appeal that Changian pragmatism lacks. The view is also motivated by cases of disagreement and controversy and shares some similar general commitments with Changian pragmatism. I show the argument structure and then discuss why the relativist is motivated to accept the premises.[44] However, the relativist, like the pragmatist, is too subjective and internal. The relativist argument that I shall analyse can be summarised as follows:[45]

1. There are different epistemic systems defined by different epistemic standards.
2. Judgements are justified *relative* to the internal standards of each epistemic system.
3. Epistemic systems cannot always be ranked or evaluated independently.
C. We should endorse epistemic pluralism.

5.2.1. Premise 1 There are different epistemic systems defined by different epistemic standards

Premise one claims that any given scientist has a set of beliefs and that a subset of those beliefs are not necessarily shared by any two scientists. This may seem trivially true and a premise that realists and monists about epistemic systems could accept. But the relativist is interested in a particular set of beliefs that are less straightforwardly divided into separate systems: those associated with the principles one holds true and exemplars—an approach or method that scientists accept as reliable, exemplary, the standard by which other methods or approaches are to be judged. These elements of an epistemic system—

44 I take my discussion of relativism to be compatible with Kusch's defense of relativism (Kusch and Kinzel 2018; Kusch 2015, 2011), though they may apply to Bloor as well (David Bloor 2004, 1999, 2005)

45 See for example Boghossian 2006 for a reconstruction of the relativist argument along these lines. Kusch and Kinzel make this argument as well in (2018, 66).

principles and exemplars—are elements that members do not by definition challenge. We can think of principles as general propositions that, together with evidence, entail beliefs we should endorse (Wright 2008, 384). Accepting a principle and accepting some evidence consequently determines what relevant beliefs one should have.

There are some important similarities and differences between the relativist and the Changian pragmatism discussed earlier. Both positions take as their first premise the existence of separate epistemic systems. However, the relativist places less emphasis on the role of goals and the structure of activity and much more emphasis on beliefs. Both consider elements of the epistemic system beyond challenge. But for the pragmatist it is the aims and successes of particular scientists that are beyond question and for the relativist it is the justificatory principles associated with that system.

This reliance on principles is problematic and for reasons akin to those that Chang faces in his first premise. I discuss two problems for thinking that principles can give relativists justification for positing different epistemic systems. I first argue principles are widely shared and then that principles do not have the kind of special status the relativist requires. Without the special status, differences in beliefs do not amount to differences in epistemic systems. Both arguments show that the relativist overplays epistemic differences. To get the relativist thesis motivated, it must be the case that different epistemic systems with different principles do or could exist and there is just insufficient reason for thinking this could be true.

5.2.1.1 First problem for relativism

Epistemic justificatory principles, the first criticism goes, are actually more widely shared than the relativist suggests. A number of philosophers have pushed this line. Boghossian develops this criticism in his (2007, 85–86). Massimi (2018) also argues that principles of this kind must be cross-perspectival, i.e. apply across epistemic contexts. And Wright (2008, 388–89) argues that the acceptance of epistemic standards cannot be understood using only relativist conceptions of truth. Kusch has extensively responded to Boghossian so I will focus on this exchange in what follows.

Boghossian (2007, 85) has argued that the differences in principles we see are just derived instances of more general principles that are either universal or practically so. For example, the belief that a telescope gives reliable evidence about the moon is an instance of a more general principle about the reliability of observation. This is a problem for the relativist because if principles do not vary across epistemic contexts, then there may be only one epistemic system. Claiming justification is relative to that one system is then no longer such a special claim for the relativist because all epistemic agents are part of the same system.

Kusch rebuffs this position in several ways (2017, 4699–4701). One response is particularly strong: when two people are part of different epistemic systems, it is difficult to find common principles that they would both endorse. This difficulty stems from the fact that any candidate for a shared principle is likely to be heavily abstracted from the particular case and, if it were heavily abstracted, the principle would not provide any bridge between the epistemic systems. General principles, in other words, do not preclude divisions between epistemic systems because they are too abstract to have a role in the disagreement. A necessary function of standards—as Boghossian envisions them—is to determine what beliefs to endorse.

Kusch (*Ibid.)* illustrates this point by appealing to a disagreement between Cardinal Bellarmine and Galileo. The former believed the bible was a source of evidence that could trump observation whereas Galileo believed the opposite, that observation trumped the bible. We might say that they disagreed over the principle (B1) "the bible is the best source of evidence." Bellarmine endorsed this principle while Galileo endorsed the principle (G1) "observation is the best form of evidence."[46] Kusch suggests there is no more general principle that encompasses these different propositions that Bellarmine and Galileo would endorse, therefore Boghossian is wrong to suggest particular principles are derived from general principles.

We might find Kusch's response a bit quick because of a problem related that facing Changian pragmatism: how should we distinguish between different systems? I will argue that in the case of relativism, it is not clear that principles can provide a way to make this distinction. They cannot fill this role because they cannot be unambiguously formulated in the right way. If they cannot be unambiguously articulated, how can they give us a definitive way to distinguish epistemic systems? I think they cannot.

I contend that how we formulate the principles that disagreeing people endorse could dramatically affect whether we conclude there are multiple epistemic systems. Say, for example, that we reformulated Bellarmine's (B1) principle as

> (B2) The bible is *always* the best source of evidence and when the bible provides no evidence, observation is acceptable.

[46] These formulations of historical principles, and the others that follow, are entirely hypothetical. Part of the purpose in characterizing beliefs in this way to show how essentially speculative an analysis that appeals principles is (and hence unreliable).

Next let's reformulate Galileo's (G1) principle as:

> (G2) The bible is *almost* always the best source of evidence and observation is sometimes better.

I have reformulated Galileo's hypothetical principle in this way because it is just as compatible with empiricist science as the previous principle (G1). However, the crucial difference is that (G2) does not outright reject the value of the bible as a source of evidence. If Galileo did endorse (G2), then the divide between Galileo and Bellarmine is not very profound because both accepted the value of the bible as evidence. The main difference would then be that Galileo put constraints on biblical evidence whereas Bellarmine did not. If this is correct, their disagreement is not the result of different principles, but concerns the scope of a single principle, namely *when* the bible is the best source of evidence. They would, therefore, be part of the same epistemic system, which contradicts relativism.

In suggesting Galileo and Bellarmine endorsed the same principles, I am not necessarily agreeing with Boghossian's criticism. I take Boghossian to endorse principles that determine in a strong sense what beliefs a rational agent *must* have. I reject this strong determining role: there is still scope for disagreement even when principles are shared because the scope and application of them is not given. If there were not this scope, it would be difficult to imagine how a rational disagreement could take place. This lack of clarity stems from the fact that, according to Boghossian's view, the beliefs one should hold are determined by principles and evidence. This is an uncharitable view of disagreement and fails to tell us why disagreements take place and how they are resolved.

However, there is another important difference between Boghossian's and my criticism of relativism. I am not suggesting Bellarmine and Galileo *definitively* shared the same principles, which Boghossian does.[47] Although I think it is likely they shared a number of principles, I am merely trying to show that sharing principles is just as plausible as not sharing principles. This is because the principles can be drastically reformulated so as to show more or less difference and it is plausible that those who disagree would accept the reformulations.

There are two conclusions I draw from this discussion of reformulating principles. First, it may be easy to re-examine cases where disagreement seems profound and to find that the disagreement is actually quite shallow, which undermines the strength of the

[47] General principles are shared propositions that state under what conditions a belief is justified (Boghossian 2007, 85). In being general, such principles are distinct from the particular beliefs particular people hold.

relativist conclusion. Whether this is possible will depend heavily on the case and how plausible it is that we can characterize it as a shallow disagreement. I am not suggesting this is always possible, but I do think it is *plausible*.

Second, the very fact that minor changes in our principles have profound consequences for our philosophical view should strike us as odd. We may then have this concern: can principles be so precisely articulated that they form the basis for a relativist account? I think the answer is no. For example, which of the following would Galileo endorse?

(i) The bible is almost always the best source of evidence; when it is not the best source of evidence, observation is best.
(ii) The bible is always inferior to observation as a source of evidence

In order to justify some claims, the difference between these two principles is moot. However, many claims would be justified very differently if we endorsed (i) and not (ii) or vice versa. The problem becomes quite serious if there were insufficient evidence to accurately reconstruct the principles different people have. For example, we may just have too little evidence to know whether Galileo endorsed (i) or (ii)—perhaps he never clearly and explicitly stated his principles. He might not even have known which he endorses; the question may never have arisen for him. If it is correct that principles are difficult to discern, then they may not be the kind of thing relativists can rely on to justify the existence of independent epistemic systems.

There is further reason for doubting the existence of fully independent epistemic systems. It is not clear that someone could be guided by principles that are different from ours because any deviation from our principles would just be a case of irrationality. Boghossian discusses this challenge (2007, 108–10) and Kusch has not adequately responded to it.[48] This is a serious worry that puts pressure on the claim that there are or could be multiple epistemic systems.

5.2.1.2 The second problem for relativism

I have argued for one problem that Kusch faces. I now turn to the other, which is that epistemic principles are not sufficiently different from any other beliefs. The main feature

[48] This problem is also related to Davidson's (1984) argument against the possibility of alternative conceptual schemes. By merely considering a candidate conceptual scheme, we bring that supposed scheme under our own. See my discussion of Davidson in 5.1.2.

of an epistemic principle, for the relativist, is that it provides a norm for evaluating claims, but is not itself in need of justification and so is not open to question or challenge. However, I contend that even if there are some beliefs that are not challenged by those who hold them, one could still challenge them if given reason to do so. If this is right, then there is no profound distinction to be made between principles and other beliefs. Consider once again Galileo's principle that (G1) observation is always the best source of evidence. What makes this belief special, i.e. a principle? Certainly it is the kind of belief that in most cases we would not expect to challenge. After all, observation typically gives us no difficulties. If observation leads me to believe there is a cup on the table, I can use that belief to inform future action; I can pick the cup up, avoid knocking it over, pour water into it, etc. Challenging G1, in cases like this, would be a very puzzling challenge. What would doubt about G1 amount to, if I can pick up the cup, knock it over, etc.?

However, just because I am normally not in a position to doubt G1 does not mean I am never in a position to doubt it. Distant objects or unfavourable conditions can lead me to rely on other sources of evidence. Doubting G1 in these contexts would be quite reasonable. Such contexts may be rare, suggesting for most purposes G1 is adequate and that *generally* G1 is not subject to challenge. But all this shows is the robustness of G1, not that G1 occupies a special status as a principle on every occasion. One cannot doubt every belief all at once and one needs reason to doubt something, but presenting with the right context, but my argument suggests that principled beliefs do not differ in kind from other beliefs.

What does this criticism amount to? It suggests that disagreements over principles, which are fodder for relativists, need not be very deep disagreements because they are just disagreements over beliefs, which all disagreements are, in a sense. If we accept this, then disagreements that seem to involve principles could, in principle, be resolved and they might not be in principle sustainable.

Is this a fair criticism of Kusch's relativism? It may not seem so because Kusch does acknowledge that relativism is *not* committed to the idea that disagreements cannot be resolved. However, he does admit relativism is committed to two serious claims: (i) disagreements are in principle sustainable and (ii) the outcome of the disagreement is contingent. However, if principles are possible to criticise, even if in practice they sometimes are not, then (i) and (ii) are problematic. (i) is problematic because if principles can be challenged just as any other beliefs, then there is no reason to think disagreements stemming from different principles are any more sustainable than any other disagreement because, in effect, these disagreements over principle are not different in kind from any other disagreement. (ii) is problematic because if principles can be challenged, then any disagreement over them is probably going to be temporary, unsustainable, and not very deep.

I have given the relativist argument that scientists are part of epistemic systems that are constituted by beliefs and goals and that different scientists are sometimes members of different such systems. I gave some motivation for thinking we should accept this premise. I pointed out some differences and similarities between relativism and Changian pragmatism. Both are committed to some contextual unit of scientific practice, but the relativist is more interested in judgements and justification and, therefore, beliefs. I ended by arguing that principles, which I believe Kusch relies on to justify the existence of independent epistemic systems, may not be able to provide this role for the relativist. If my worries are well founded, then there is reason to doubt the power of a relativist analysis of science.

5.2.2. Premise 2 Judgements are justified relative to the internal standards of each epistemic system

The commitment here is that the justification for beliefs is always situated within an epistemic system. Kusch argues for this position in his recent defence of relativism (2017, 4697). I discuss what this premise might mean, how it differs from what pragmatists are committed to, and why it might be a problem.

On its own, this premise can be given a mild reading: scientists in one epistemic context can only justify their judgements by appeal to what is epistemically available to them in that context. The only claims of interest here are those within rational space, those that scientists can support with reasons.[49] It bears close resemblance to the pragmatist premise 2, that evaluations are always within an epistemic system. An important difference is that the relativist ties justification to beliefs whereas the pragmatist ties them to aims without emphasis on beliefs. The pragmatists, in other words, need not appeal to what a scientist believes when evaluating a claim. Consequently, the pragmatist is more interested in *success* and the relativist in *justification*. The former asks, "does this explanation achieve the aims I set out" whereas the epistemic relativists asks, "given my current state of knowledge, is this belief justified"? Relativists can also be fully propositional in their

[49] Relativism can, in principle, cover a broader domain than this. Matters of taste, for instance, may not qualify under this brand of relativism if we consider matters of taste mere sensation reports. This is because these reports would not be subject to rational support. If we considered such judgements subject to rational support, however, then taste may fall within the domain of relativism. Restricted to the scientific domain, it is easy to find examples of justification that are tied to particular epistemic systems. This is the form of relativism I consider here.

characterization of scientific disagreement, whereas pragmatists can include experiments, models, instruments, and other features of scientific practice.

As an example to illustrate the relativist claim, consider again the history of physical sciences. Scientists in the early 19th century epistemic context could not provide any justification for claims about electrons because electrons were not part of the epistemic context until much later in the century; they could, however, make claims about quantities of electricity[50], just not in the sense of an electron as a wave or excitation in a field. Natural philosophers during this earlier time consequently could have had no beliefs or explanatory aims associated with electrons. The general lesson to draw from this is that any particular justification, because it depends on having certain beliefs, concepts, and sources of evidence, is only possible in certain contexts.

Relative justification, given this mild reading, need not be controversial. However, if given a stronger internal reading, this premise is problematic, for reasons similar to those facing Changian pragmatism. The issue with Changian pragmatism premise 2 was that evaluation was too subjective. Similarly, if we give internal justification a highly subjective reading—such as all scientists can specify their own standards for evaluating judgements—then we may have a problem. Relativists like Kusch want to do this when analysing disagreement. Disagreeing scientists have enough in common to find one another's views unsatisfactory, but differ enough to find resolution out of reach. The difference is that their justificatory practices are based on different principles, which are beyond question, so disagreement can continue indefinitely.

The stronger reading of this premise depends upon accepting premise 1, that different scientists occupy different epistemic systems when their principles differ. I already argued premise 1 was difficult to accept, so I will not discuss principles more here. Without that other premise, relative justification need not be inoffensive because we can give it a mild reading.

However, there is reason we might want to give this premise a strong reading. It comes from a discussion of what Fogelin, inspired by Wittgenstein and Putnam, called deep disagreement (1985). There has since been extensive literature on disagreements of this kind—see for example (Lugg 1986; Turner and Wright 2005; Godden and Brenner 2010). The general idea is that some types of disagreement cannot be resolved rationally because such resolution requires extensive shared background, such as shared beliefs about evidence, that is necessary for resolving the disagreement. When there in insufficient shared background, resolution is impossible. This background is what the relativist calls the epistemic context.

[50] See Stoney (1883) for an early example of attempts to quantify units of electricity.

Relativists apply deep disagreements to science by attempting to identify beliefs that are not shared between disagreeing scientists: such beliefs, they postulate, preclude resolving the scientific disagreement. Kusch does this with case studies from the history of science (2015; 2018) These beliefs, principles or standards, provide guidance on how best to go about acquiring knowledge of the facts. How plausible this analysis is will depend upon how seriously one takes the possibility of fundamentally different beliefs. If the difference is too fundamental, then Davidson's criticism of conceptual schemes would apply. If the difference is not that fundamental, it may not be deep enough to motivate the relativist. These are, however, only applicable to the strong reading.

There is one objection that may apply to even a mild reading: relativism is committed to internalism about justification and thus sets the bar too high for what can be known. Lewens (2005, 575) raises this very problem against relativism. The issue is that internalism supposedly requires that potential knowers, to have knowledge, must prove the reliability of their justification. Such a proof, Lewens argues, cannot be developed in a non-circular way. This suggests that one need not know the reliability of one's justification in order to have a true belief, which in turn suggests that the truth or falsity of one's beliefs is independent of the epistemic context in which they feature. Reliability provides a kind of universal tool for justifying beliefs and principles and the epistemic context are irrelevant.

There are two things a relativist could say in response to the externalist criticism. The first is that it's not clear why an internalist about justification would require proof. The externalist has assumed that internalism sets the bar very high for justification, but in fact relativists are interested in the appropriateness of belief, which does require some epistemic relationship between a potential knower and justification, but this relationship can fall far short of proof.

The second response the relativist might give to an externalist concerns how reliability is established and whether there is a genuine external alternative. Externalists tend to be reliabilist about justification: a belief is justified if it is produced by reliable mechanisms. And reliability we usually understand in terms of success (say empirical success) (Lewens 2005, 570). But the reliabilist has smuggled in a crucial concept: success. And they have done so without specifying what counts as successful. For relativists, what counts as successful is going to vary according to the epistemic context. If that is correct, then what counts as reliable and what counts as justification will also vary. Whether a belief is justified is therefore tied to the epistemic context. This leaves us with the original claim of interest to the relativist: whether beliefs are appropriate or not in the relevant epistemic context.

This discussion suggests that the externalist criticism of relativism needs further development to be serious. As it now stands, externalists leave un-analysed key features of justification that are only grist to the relativist mill.

I have given the argument for the second premise in the relativist's argument, which is that justification is relative to an epistemic system. The relativists are interested in whether a justification is appropriate or not and this, they contend, depends upon the epistemic context of the scientist offering the justification. This becomes interesting when two conflicting judgements arise, which are equally justified relative to different epistemic systems. I argued that externalist worries about justification are not serious for the relativist. There are strong similarities between this premise and Chang's premise 2. Both are committing to an evaluation of scientific claims within the epistemic context in which they are made. If we give this premise a mild reading, then endorsing it is not difficult. However, stronger readings give us the same problems Changian pragmatism faces.

5.2.3. Premise 3: Epistemic systems cannot always be ranked or evaluated independently

This premise, along with premise 1, does much of the work establishing the relativist thesis. It suggests that different epistemic systems cannot be ranked or evaluated neutrally. It might be that some systems can be clearly ranked, but this is impossible for certain forms of disagreement. Consequently, there are no system-independent reasons or neutral facts that would compel someone to switch from one system to another. In other words, one cannot be compelled on rational grounds to adopt one system over another or to switch from one system to another. I suggest that there are at least three reasons for accepting this premise. Even though these reasons can give some strong motivation for this premise, I end by arguing that it cannot do the kind of work the relativist needs it to.

The first motivation for this premise stems from the difficulty of providing a neutral ranking. Ranking is not neutral because any ranking just emerges from the confidence we have in a particular system—our own epistemic system—which does not offer neutrality. Any ranking we make, in other words, we always make from within a particular system and that system is going to taint our ranking, making it non-neutral.

The second motivation for this premise is the fact that those who disagree do not accept the opposing view. Epistemic humility, if there is such a thing, suggests restraint when ranking. I might believe in a set of principles and think they are better than the alternatives, but those who disagree obviously do not share my beliefs. There may, at times, be no rational way to arbitrate. If there is indeed no way to arbitrate, then it seems no neutral ranking is possible. I take it that a neutral ranking is an evaluative tool that all parties can, or should, endorse. This is the whole point in having a *neutral* ranking. If not all parties do accept the ranking, we have reason to doubt the evaluative tool is neutral. If all the ranking does is convince us and vindicate our beliefs, then it only affirms our existing beliefs and does little else for those who disagree. In other words, the very fact that there is disagreement suggests ranking epistemic systems is problematic. This criticism trades on two rival views of epistemic disagreement: the steadfast and conciliatory views. The steadfast view (Rosen 2001; van Inwagen 1999) believes it is reasonable to stick to one's beliefs even in the face of disagreement. The conciliatory view (Christensen 2007; Feldman 2007) argues the opposite: that the very fact someone disagrees with us should prompt us to revise our beliefs. If one endorses the conciliatory view, then the very fact that someone disagrees with us should prompt us to revisit the beliefs we hold. This is because presumably those who disagree with us do so in part because they rank their own system higher than ours. But we in turn rank our own system higher. If we then thought our beliefs were not necessarily better than those held by others (including our beliefs about which ranking is better), then the possibility of a neutral ranking is tenuous. However, if one were a steadfast epistemologist, this argument would not be compelling. But steadfast epistemologists are unlikely to be sympathetic to relativism because such an epistemologist does not feel the beliefs of others need prompt change or evaluation of their own beliefs.

I offered three motivations for the premise that epistemic systems cannot always be ranked neutrally. I first argued that the confidence we might have in a particular system does not allow us to rank that system relative to others, at least on occasion. I then suggested there is no neutral, context-free means for evaluating epistemic systems when there is disagreement. Finally, I argued that conflict arises at the level of particular judgements, but not at the level of epistemic systems on the whole, which prevents our ability to rank them in a straightforward way.

Now I will discuss some reasons why this premise might fail, despite these motivations. The first, which I raised during the discussion of premise 1, is that there is insufficient basis for thinking there are different epistemic systems of the kind the relativist needs to get their thesis off the ground. If there are no such systems, then ranking, neutrally or otherwise, is not possible because there is nothing to rank. This should be a serious worry for the relativist. But there are two further worries. The first worry is that the relativist has confused the difficulty of evaluating some beliefs with the thesis that those beliefs cannot be evaluated at all. The second worry is that the impossibility of a neutral ranking does not support the relativist position.

First let us consider the worry about evaluating beliefs. Much of scientific evidence is not proof. This means that there is logical room for alternative explanations or interpretations of the same body of evidence. Boghossian discusses this very briefly in the context of underdetermination (2007, 127). I would like to develop it in a more specific direction.

The relativists take the existence of rival scientific beliefs as support for rival epistemic systems. The relativist assumes, and has some evidence that, those who disagree are peers in good epistemic positions. Bellarmine had very good reasons for his beliefs and Galileo had very good reasons for his beliefs. In such cases, it seems ranking these beliefs neutrally is impossible because each has reasons for endorsing them. The relativist here is taking a view similar to the one that Ruth (2013) defends about disagreement. The view is that one can rationally remain steadfast in their view when disagreeing with a peer: two disagreeing scientists are peers in epistemically good positions and it is therefore reasonable for them to continue holding their beliefs, even though they realise the other disagrees.

However, we might wonder whether we should be committed to disagreeing peers having good epistemic positions. This may be the case sometimes, but is it often enough to get the relativist thesis going? Rather than think the disagreement emerges because there is good evidence for rival views, peers may sometimes disagree because there is poor evidence for both views. In other words, both parties to the disagreement could be in poor epistemic conditions. In such cases, it seems more reasonable to me that when there are rival beliefs each with justification, no one has very good justification and so must make do with weaker forms of evidence.

In such cases, the existence of a disagreement that is not easily resolved is best explained by appealing to limited information, evidence, and justification, not by appealing to relativism. So even if the relativist is correct in claiming a neutral ranking is impossible, it is impossible because there is limited justification available and thus uncertainty, but not because there are well-supported rival positions that could be in principle sustained. If we had more information, more evidence, better justification, then we might overcome the difficulties we had in evaluating rival beliefs.

This is well supported historically. Consider that it was once very reasonable to believe that the planets move in circular orbits because there was limited information and circular orbits fit well with what evidence and knowledge there was. Planets move cyclically and circular orbits would be compatible with this rough observation. However, this fact about the evidence available to past scientists does not provide support for the claim that past astronomers were in a good epistemic position. They were in a relatively poor epistemic position compared to where we are now. Consequently, it may once have been reasonable to believe planets move in circular orbits, but that is no longer the case. So the relativist view of disagreements does not seem right. The relativist assumes those who disagree are in equal and good epistemic positions, but if we thought that disagreements took place because of insufficient evidence and a poor epistemic position, then it is not so obvious disagreements can be sustainable in principle.[51]

Even if we set aside the status of the epistemic position, there is another worry, which concerns whether epistemic systems can in fact be neutrally ranked. If beliefs can be rationally evaluated, then this suggests that epistemic systems can at least be evaluated. The question remains whether the evaluation, and the ranking, can be neutral. I will suggest it does not matter whether we can. If neutral ranking is possible, relativism is unsuccessful. However, the impossibility of a neutral ranking does not provide support for relativism.

Why think a neutral ranking is desirable? It may not actually be something to strive for, regardless of whether it is in principle something we could achieve. For instance, we know a lot more about the natural world now than natural historians did 200 years ago. It is also certainly the case that contemporary scientists will evaluate claims differently from their intellectual predecessors. Both of these claims suggest that contemporary scientists are in a better epistemic position and that in making this judgement we are providing a ranking. We then might ask: is our contemporary ranking neutral? It is not neutral in the sense that it is informed by our current knowledge and those who are in a different epistemic position would make a different ranking. But this concession is not very substantial because it is likely that the contemporary evaluations we make are better than the ones we would have made 200 years ago because we now know more and so are in the better epistemic position. The question about ranking epistemic systems then becomes not one of neutrality, but one of how to be in the best position to offer one. And wouldn't the one who knows more be in the better position to provide a ranking?

There is one further point about this premise worth considering. The relativist claims that the outcome of a disagreement is contingent. We should be sceptical of this. I think the relativist has mistaken contingency for the fact that it may not be obvious during a disagreement which view is correct. In such cases, it may appear that any particular outcome is contingent. This appearance of contingency, however, is perfectly compatible with one party to a disagreement being correct and another being incorrect. It is also compatible with both being wrong or both being partly correct. And, finally, it is

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51 Being in a poor epistemic position will not explain all disagreements, but it suggests there may be fewer instances of relativized disagreements than we might initially expect.

compatible with one party becoming accepted as correct, even though that party is in fact wrong. The point here is that what appears contingent at one time may not be a good guide to what we later accept as true. If we endorse this, then the relativist's point about the outcome of disagreements being contingent is not a very profound point. In cases where it seems like an outcome is contingent, it may be a temporary conclusion (perhaps because both outcomes seem equally reasonable or unreasonable). It might also be that such apparent contingency is not a good guide to what we later accept as true. Regardless of how we should choose to interpret an outcome that seems contingent, there is plenty of room to do so in such a way that avoids relativism.

5.2.4. Conclusions about Relativism

I presented three premises for the relativist view and argued against endorsing them, especially 1 and 3. Against premise 1, it is not clear that different epistemic systems exist in the way the relativist needs them to. The problem is that the relativist relies on a conception of epistemic principles and such principles are difficult to characterize substantively. The second premise—that justification is relative to the epistemic context—may only be controversial for externalists about justification; at least if we reject premise 1. Rejecting premise 1 means justification need not be tied so closely to subjective and idiosyncratic beliefs. For the final premise, that neutral ranking is impossible, I argued neutrality offers no support for relativism because such a ranking is not something we should strive for; so its possibility is tangential. And finally, I suggested that the outcomes of disagreements are not so highly contingent. Although some disagreements may become protracted, it is a difficult to make sense of the further claim that they are always contingent.

These remarks suggest we need an account of scientific disagreement that does not take as strong a stance toward the depth of difference between those who disagree, but still gives due consideration to the context in which those disagreements take place and why both parties are motivated to disagree. The next section considers, and defends, a view that I contend comes closer to these ideals.

5.3 Perspectivism

This section defends the last view this chapter considers. Perspectivism avoids the strong internal evaluations that pragmatists and relativists defend (particularly in their second premises). I will follow Kuhn in centring the account on taxonomy, but with key differences that result in a novel approach to thinking about disagreement and change. The structure of the view is as follows.

1. To participate in a practice, scientists must broadly be using the same conceptual taxonomy.
2. Disagreements can emerge from rival uses of a taxonomic system, where different uses offer different perspectives.
3. Those disagreements can be resolved within an epistemic context structured by shared epistemic standards.
C. We should be perspectivists about disagreements and conceptual change.

5.3.1. Premise 1: To participate in a practice, scientists must broadly be using the same perspectival taxonomy.

Changian pragmatism and relativism take as their first premise the commitment to *plurality*, which I criticised. The problems concerned how to define what the plurality was about. I suggested the grounds for positing the existence of rival epistemic systems were insufficient grounds. This suggests that we need a characterization of disagreements that does not put those who disagree in different epistemic contexts. Consequently, the perspectival premise defended here starts with what scientists have in common by taking as its starting point the *unity* of scientific inquiry.

That particular form of unity is the unity associated with scientific disciplines and when acquiring membership of that discipline, scientists learn the use of the associated terms. Working within a discipline requires knowing a number of things that interrelate and this knowledge forms an epistemic context. A large part of this epistemic context consists in the terms scientists use. It obviously includes other things and activities—hence we could call it a practice—but I claim the terms must be learned first and are of most interest here. These terms form a taxonomy and when scientists learn a taxonomy, they learn how the different things they study are related to and distinguished from one another.

There are three tasks in the remainder of this section. I will first examine the points of common ground and points of differences between perspectivism and the pragmatist and relativist commitments. Then I distinguish my project from Kuhn's. Lastly, I will provide motivation for endorsing this premise.

This premise shares an important commitment with the pragmatist. Both take activity as a starting point for analysing science. The epistemic context with respect to science therefore does not consist only in sets of beliefs, but in abilities. There are, however, important differences between not only perspectivism and Changian pragmatism, but perspectivism and relativism. However, we are concerned here specifically with the linguistic abilities associated with the use of taxonomic terms. As such, this epistemic context is more closely related to the Wittgensteinian view of language (or language-games) as a practical ability.

Perspectivism I defend here specifies preconditions for practice membership. Those preconditions are the use of a taxonomic system. This claim about preconditions is more committing than a trivial reading: scientists know a bunch of technical terms. Such a general claim does not carry the force of the premise I am defending here, which is that scientists *must* know the same set of terms, not merely that they do so. And if they do not do so, then they *must* be occupying different perspectives. This force is what clarifies what membership of a practice or discipline consists in. Chang's pragmatism and Kusch's relativism do not directly address preconditions for membership. Instead, they conclude on the basis of controversies and disagreements that scientists must be members of different epistemic systems or practices, but without addressing directly why we should reach this conclusion solely because disagreements exist.

Despite the important differences I noted between this premise and Changian pragmatism, there is an important point of similarity and that is the emphasis on activity. But whereas Chang focuses on activities that achieve aims, the activity associated with perspectivism is the *use* of the taxonomic system. Because the elements of the system should best be understood by the use to which they are put, we can think of this account, loosely, as a Wittgensteinian approach to scientific taxonomy. This inspiration comes from Wittgenstein's emphasis in his later work on the use of language as a practical ability and on his characterization of the meaning of a word as its use.

Another inspiration for this view is Kuhn, in particular his (1990), but the similarities with his view are importantly limited. Most of these differences I discuss in chapter 3, but there is one that I have not yet discussed. I mean something more specific about membership than Kuhn does. Chemists, for example, learn how to do three things when learning the taxonomy of their epistemic context. They learn the concept (i), they learn how to apply the concept (ii) and thus how to distinguish various objects of study, and they learn how the things that fall under the concept are significant for the discipline (iii). The first of these things, the concept, might be, say, what an element is (i.e. that it has a nucleus and forms molecules). In learning how to apply the concept, they learn how to tell elements apart from other things like compounds or particles; this is the second thing learned. The significance might involve how it is studied and how it relates to the other study subjects. For instance, a chemist recognizes the way elements are related to molecules. This general understanding, of the objects of study, how they are studied, and how they relate to one another, forms the epistemic context of the discipline. To be a scientist, one is always a member of such a context. And in being a member of such a context, scientists have mastery of a number of practical abilities.

This is a very different picture of taxonomy than Kuhn offers. Learning a Kuhnian lexicon[52] is akin to learning a language, but a language than cannot be translated (1990, 5). Those who know two lexicons, therefore, are bilingual, but without the ability to translate one language to another. This picture is very different from the one perspectivism offers. A necessary part of learning a new term or language is the acquiring the ability to paraphrase, either using other words from the same language, or words from another language, a point philosophers since Davidson (1984) have made. Kuhnian lexicons do not sit well with this feature of learning and translating terms or languages. Perspectivism makes no such claims, not only because it does not commit to such a strict holism, but also because it takes a different stance toward the meaning of that taxonomy, i.e. the meaning is not to be primarily understood in terms of reference, but in terms of use.

There are two motivations for this commitment to taxonomy as practical ability. The first is that understanding an epistemic context more generally first requires understanding the taxonomic elements of that practice; this is one place where perspectivism departs from Changian pragmatism and relativism. The second motivation is that examining how scientists use a taxonomy tells us much more about their practice than looking at theory. Let's examine each in more detail.

Firstly, understanding the epistemic context requires understanding the taxonomic system of any given discipline, which has changed over time, losing and acquiring key parts of that system. At the same time and in lock step with these changes, the problems scientists face and the way they address those problems has also changed. For example, phlogiston has utterly dropped out of use from chemistry. Chemists no longer use a eudiometer to study phlogiston and no longer pose questions about phlogiston. Oxygen, when it entered the chemist's vocabulary, gave rise to new questions—say about acidity—and the development of new techniques to address those questions, such as weight measuring techniques. The main point here is that understanding the dynamics of

[52] Kuhn defines a lexicon in this way: "Terms [which form a lexicon] have two essential properties. First, as already indicated, they are marked or labelled as kind terms by virtue of lexical characteristics like taking the indefinite article. Being a kind term is thus part of what the word means, part of what one must have in the head to use the word properly. Second - a limitation I some- times refer to as the no-overlap principle - no two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus" (1990, 4).

an epistemic context requires understanding the nuances of the conceptual system. The order of explanation starts with an examination of taxonomy before proceeding to other elements of an epistemic context, such as methods, evidence, and explanation. Therefore, I suggest that the pragmatist and relativist equally fail to begin their analysis of science at the correct point because they start with the claim that there is a plurality without explaining what commitments one must have for membership. Perspectivism first explores what it is to be a scientist first, or at least what minimal commitments a scientist must have.

Secondly, an examination of how scientists are using a taxonomic system tells us more about metaphysical commitments scientists face than an examination of theory or beliefs. I take it part of what we want to glean from an analysis of science is the metaphysical commitments that science entertains. Examining taxonomic distinctions is an easier way to do this than examining theory.

Consider the following question: how are compounds to be distinguished from elements? Asking this question tells us not only which things are elements, but also what activities a scientist must perform in order to find out. Such an answer is much more informative than just analysing a theory, which on its own tells us little about the activities associated with a discipline or epistemic context. And on its own it tells us very little about what attitude scientists take towards it and what kind of interpretation we can or should give the theory. We can, after all, give theories various interpretations that each yields quite different metaphysical conclusions.

This question of taxonomic distinctions also tells us more than an examination of beliefs. One might be tempted to examine beliefs as a means to investigate metaphysical commitments or disagreements, which the relativist does. This, however, is not straightforward because it's not clear what beliefs every scientist in a practice has. And if it's unclear how widespread a belief is, how can that belief be an indicator of commitment? It's also unclear what method could reveal in a satisfactory way the beliefs of a discipline. Interviews are one option, but not for many studies of the history of science. An examination of texts might, but such texts in many cases may be unable to help us discern committing beliefs from confidence in a hypothesis. Furthermore, as van Fraassen has argued (1980), there is no rational mandate to take a given attitude toward theory, i.e. one need not think theory is true simply because it is successful. Such an argument suggests theory is not a good guide to understanding ontology, or at least even if it were necessary, it is not sufficient.

5.3.2. Premise 2: Disagreements and controversies can emerge from rival uses of a taxonomic system, where different uses offer different perspectives

This premise is partly what makes this account distinctively perspectival and related to pluralist views. It stipulates that scientists in the same discipline or epistemic context *generally* use in the same way the taxonomic system, but not always and not completely. I discuss the significance of what happens when this consensus breaks down or fails to form and then motivate this premise.

Uncertainty or disagreement can emerge in the use of a taxonomic system. When a discrepancy emerges, we can call the different rival uses of the taxonomy different perspectives. It departs from previous views by making no internalist claims about the relativity of explanation or justification, nor about how situated they are within epistemic systems. Instead, this premise merely states that differences can emerge within a perspective that had previously had unity, yielding rival perspectives. However, perspectivism is similar to the other views in that it takes interest in disagreements and controversies and part of what motivates this premise is a need to better characterize disagreements and controversies.

Why might it be valuable to describe these disagreements and controversies as perspectival? There are three reasons. The first reason stem from thinking about historical science. Historical cases provide support in a natural way for thinking that taxonomic problems are often, though not always, central features of dispute. The second reason for describing disagreement as perspectival is that emphasis on taxonomy allows us to reallocate some realist concerns by characterising realist questions as identity questions. Third, this recognizes the extent to which disagreements are situated in a wider, and shared, epistemic context. I discuss each reason in turn.

Historical cases provide support for this premise. As the preceding chapter argued, a reading of the Chemical Revolution suggests chemists during the 18th century did not form a consensus on how to discern the simple substances from the complex and it was consequently unclear which things were simple and which were complex.

This analysis differs substantively from other interpretations because it stipulates that scientists disputed only *part* of the system, much of which was unaffected. No one questioned that there were simple and complex substances, metals, acids, and airs. These were all widely accepted categories. And chemists were particularly interested in studying the properties of substances such as water, metals, and rusts, but not just properties. Some of their interest lay in identity questions, questions concerning what kinds of things these substances were. Are metals simple? Is water simple? What happens when a metal becomes a calx? What is a calx? Much of the disagreement emerged from different answers to these questions. But regardless of the specific answers one might give to these questions, the very fact that disagreeing chemists could ask the *same questions* is an indication that chemists held most of the taxonomic system in common. Differences only emerged when it became unclear which substances were elements. There were certainly other problems too, and not every disagreement takes this form, but taxonomic issues were, and often are, some of the most pressing.

There is an important departure here from relativism and Changian pragmatism, which both use their second premise to establish a kind of internal justification or criterion for success. This second premise of the perspectival argument, however, seeks to establish that disagreements are inherently limited, or constrained, which leaves extensive parts of an epistemic system or perspective unaffected. In other words, I am rejecting the kind of holism that seems to underpin Kuhn's lexicons.

I now discuss how this might avoid some of the problems that realist approaches face when examining cases from the history of science. One way of reading the realist question is to question what attitude we should take toward the entities posited by scientific theories. Although intuitive, this approach faces a number of problems, one of which I will discuss. It is difficult, by framing historical sciences in terms of the realist question about existence, to form an insightful interpretation. This is because there is a sense in which none of the entities postulated by 18th century chemistry exist because we now full or mostly reject theories from that period. But if all those theories are false, what insight could we gain from examining them? There might be kernels of truth that are preserved in later theories, but such an analysis is not sensitive to the historical context and requires extensive reconstruction.

These problems are avoided by shifting the question away from existence considered in the abstract and toward classification primarily. Existence claims are of course not without interest, but the existence or non-existence of entities cannot be investigated without in the process investigating the classificatory practices of the associated discipline that studies them *in its own historical context*. This strategy sits more naturally with what scientists are actually doing and gives us resources for appreciating and understanding what scientists in the distant past were doing and why. Rather than wondering whether phlogiston exists as certain realists might, the perspectivist suggests we instead ask how scientists used phlogiston to identify certain substances and study them. This permits a more natural investigation of scientific practice because scientists often consider what something is rather than whether it exists.

Chemists were curious about what combustion consisted in, a question that does not involve *existence* but rather the *nature* of a natural phenomenon and how it was related to other phenomena such as calcination. Once this question is posed, scientists can then investigate whether combustion is a process of composition or decomposition, which is what was really at stake. Whether phlogiston exists subsequently falls out of the answer. If combustion is a decomposition process, then phlogiston is what is released (and it must therefore exist). If combustion is a composition reaction, then phlogiston as a substance does not play an explanatory role (and so we might say it does not exist).

There are several advantages to this view over the two others. First, it makes no appeal to epistemic principles, which I suggested the relativist has trouble characterizing in such a way as to motivate relativism. Perspectivism also does not in principle need a characterization of truth as coherence, nor does it require the tricky process of disentangling very specific epistemic aims that scientists have. These two merits give perspectivism an advantage over Changian pragmatism, whose issues I discussed above. More generally, I think the perspectival treatment of disagreement is intuitive in at least one important respect: it characterizes disagreements and controversies in such a way that there is reason for scientists to have disputes. The reason is that disagreeing parties are both invested in the disagreement because they are part of the same practice, discipline, or epistemic context. Everyone who is a member of such a context will be affected by the result of the disagreement. In contrast, it is very mysterious to me why scientists would have cause to disagree if they are members of different epistemic contexts.

I suggested this premise concerns differences in how scientists differently use a taxonomic system. I argued this was a reasonable premise to hold because historical cases seem to suggest taxonomic questions were at least sometimes important parts of disputes and committing to this premise allows a change in emphasis away from a primary interest in existence and toward taxonomy problems primarily. These kinds of problems sit naturally with important parts of scientific practice.

5.3.3. Premise 3: Those disagreements can be resolved within an epistemic context structured by shared epistemic standards

Changian pragmatism and relativism take as their third premise a general commitment to neutrality and an inability to rank systems. A worry about that general approach is that it suggests there is no common ground we can appeal to when trying to resolve disagreements and controversies. I argued this was a serious worry, which suggests we should instead look for a way to characterize disagreements such that they can be resolved in principle. This is the motivation for positing this premise.

This premise suggests that, even though discrepancies or disputes over parts of the taxonomic system emerge, they can be rationally addressed by appeal to standards that transcend the boundaries of particular perspectives. So even if it becomes unclear how to tell apart the elements from the compounds, it is in principle possible to achieve resolution. I first describe what these principles are and then illustrate their application. I defend this characterization of principles against objections.

A distinction may be helpful in understanding the role principles have in the context of disagreement. Wright (2008, 381) distinguishes between what I will call *normative principles* that we consult when forming judgements and *descriptive principles* that merely reveal patterns in our past judgements. Normative principles are the subject of Boghossian's and Kusch's views (and probably the views of other epistemic relativists). I think this distinction is not quite accurate.

Let's consider in more detail what this picture of normative principles is and why it is inaccurate. Wright (2008, 384) characterizes these principles as general propositions that state under what conditions a judgement is justified or what beliefs one should adopt (say when one has relevant evidence). Boghossian, Wright, and Kusch, take this to determine in a strong sense what judgements one must adopt—or beliefs one must adopt—to be rational (when presented with such-and-such evidence, I must adopt such-and-such belief). This is certainly an unpromising approach to understanding disagreement. If there were such epistemic principles, it is hard to imagine, as Kusch has argued, how they could have any significance in actual disagreements. Once a principle is accepted, one has only to determine whether the right conditions obtain in order to ascertain what judgements one should make or what beliefs one should adopt. But if this is correct, then there are a vast number of people who are deeply irrational because it is often very unclear what judgements one should make or beliefs one should have.

A better characterization of principles is to equate them with criteria. As such, principles determine what *considerations* obtain when it becomes unclear what judgement is appropriate or when scientists disagree over which judgement is appropriate. By this view, principles tell us something about the context in which disagreements occur and the form those disagreements must take, but they do not stipulate what beliefs one must adopt in order to be rational, nor which judgements one must make. But these principles do have some force in that they allow for correction. They tell us what those who disagree must consider and, if they fail to entertain those considerations, then there is a problem with their reasoning. I contend that principles of this kind play a role in resolving taxonomic uncertainty or disagreement and that they are consequently, in general form, perspective independent. Let's examine what this looks like in context.

As an example of the support principles might play in justifying a particular taxonomic use over an alternative, consider Lavoisier's work (Lavoisier in Best 2016). He used weight as a new means for distinguishing elements (discussed in preceding chapter). It was an open question at one point whether this was a reasonable choice, which gave rise to disputes about which things are elements. Lavoisier argued metals were elements on the basis of weight measurements, phlogiston chemists argued the opposite (see (Kirwan 1789; Priestley 1800). Part of this dispute involved resolving which things are elements, but the resolution required addressing whether weight can be used as a criterion of identity. Both sides of the debate gave reasons supporting their position, but the use of weight ultimately proved the better option, at least for a time. As discussed in the preceding chapter, there were several distinct advantages to Lavoisier's view and even though there was no "proof" he was right, Lavoisier's oxygen system was more *precise* and was more *consistent with empirical evidence*, even though it lacked *scope*. These are some examples of cross-perspectival standards.

The structure of the exchange during this historical episode illustrates the point that taxonomic problems can be addressed rationally. It would furthermore be very odd if these general standards—such as scope, precision, and empirical consistency—were not standards to which one could appeal regardless of their perspective. Would any chemist, regardless of their preferred theory, believe that consistency with empirical data is undesirable or irrelevant? Or that simple theories, all else being equal, are preferable to more complex ones? Although these different standards may come into conflict and so need judgement to balance, it seems implausible that any scientist would reject their appeal, regardless of the perspective they occupy. But the difficulties associated with these standards are difficulties that must be overcome in the usual scientific way: by careful experiment, analysis, and theorizing.

The relativist at this point may claim that the historical scientists I discuss were part of a different epistemic system from me and of course I have my own preference for oxygen over phlogiston, but that claim is not neutral because it is made from my epistemic context. Consequently, the oxygen explanation is only better to me, not better absolutely.

I would make two responses to this charge. The first is that I don't see how one could reject the broad standards discussed above, regardless of when or where one lived and what theories one endorsed. If someone did reject them, there are grounds for questioning their competence as a scientist or rational agent. The standards are of course general and subject to different interpretations and applications, but I nevertheless do not see how generality gives license for rejection. The second response I would offer is to appeal to the purpose of historical inquiry. I think an examination of historical cases is a worthy enterprise because it helps *us* understand how and why historical scientists acted as they did and it helps *us* understand the general project of science and how the sciences of today emerged. An important feature of these motivations is that it begins with interests that we have today, in the present.

Consequently, any analysis of past science, to be a satisfactory analysis, must provide us with an understanding of past events, which includes why they occurred. If chemists were in fact persuaded that oxygen provided better explanations and if we accept that scientists were sensitive to evidence and reasoning, then an analysis of the Chemical Revolution should account for this. If we give an analysis that treats historical scientists as irrational or not subject to some of the general standards discussed earlier, it becomes difficult to see how we could call those historical figures scientists at all. Such an analysis is not very satisfactory.

But here is a clarification about the role of standards, which I do not take to be equivalent to the epistemic principles of, say, Boghossian's view: standards are not epistemic principles that *determine* in a strong sense which beliefs we must have. Rather, I take these standards to be necessary features of the context in which disagreements take place. Returning to Wright's characterization, standards are not fully normative (in that they do not dictate beliefs), nor are they fully descriptive (by being abstractions from past judgements), but they carry partial features of both. They must be recognizable in cases of disagreement, and so have some normativity, and they do describe in general terms some past and present scientists have in common. Disagreements, to be such, require that those who disagree recognize the significance and relevance of the arguments and evidence of others, even if those arguments and evidence are rejected. Standards give us a way to think about why this appreciation is possible and what distinguishes disagreement from failed attempts to communicate. Two scientists who did not share standards could not recognize that they were making an argument.

We can motivate this premise more generally without appeal to historical cases. Questions of identity are general questions and the general form of resolution is to examine the existing means by which those identity questions are resolved and, when inadequate, to develop new methods that extend as naturally as possible from what came before. The general form of this problem concerns uncertainty about what something is, but not, as discussed under the previous premise, what exists. When faced with such uncertainty about identity, scientists and members of epistemic contexts generally can and do appeal to methods that have worked in the past for resolving that uncertainty.

It is important to qualify the character of rational support here. Any arguments for a particular way of applying a taxonomy are situated, i.e. relative to the epistemic context at the time and, therefore, temporal. This qualification should be uncontroversial. The arguments chemists in the 18th century gave are not necessarily the same kinds of arguments a contemporary chemist would give and arguments that were compelling historically may cease to be so. The notion of element is a prime example. In the 18th century, an element was simply that which could not be decomposed. Today every element on the Periodic Table of the Elements can be broken down into electrons and protons at the very least. And yet we still call it a table of *elements*.

Not only are these taxonomic problems temporal, they may also not have an obvious solution and it may not be obvious that one could, or how one would, reach a solution. And so an important criticism could be levelled at this point. The relativists like Kusch do not deny the possibility of rational resolution, just that a particular outcome is necessary. If particular outcomes are not necessary, then disagreements can be contingent as well as rational. Disagreements, therefore, can continue indefinitely in principle. Kusch likens this to a religious conversion, which involves a fundamental shift in beliefs and that such a shift is not compelled.

My response is twofold. First, scientists and anyone else can disagree, but the fact that two people have a protracted disagreement is not an argument for being *in principle* unable to find resolution. Another explanation we might offer of such disagreements could appeal to either insufficient information or misunderstandings. In other words, science is difficult and it is not always obvious or easy what kind of explanation we should offer. This does not require thinking that disagreements can in principle continue indefinitely.

Second, I'm not sure religious conversion provides an adequate standard for thinking about scientific change. For one thing, conversion suggests the complete adoption of a system of [religious] beliefs in exchange for an old system of beliefs. I do not see evidence, in this episode from the Chemical Revolution, for changes that take this form. Much of the change involved select beliefs. Science, although it may be hard to formally specify how, builds upon its history and it does so because changes, even large changes, do not negate all previous belief. Scientific change is much more incremental than religious conversion. I consequently do not find the conversion metaphor persuasive.

5.3.4. Conclusions about Perspectivism

The conclusion these premises support is that perspectivism offers a way for thinking about disagreements and conceptual change that, on the one hand, does justice to the historical settings in which these disagreements take place, and that on the other does not give way to relativism or other strong anti-realist views about science. It provides a characterization of certain kinds of disagreement and offers a mechanism in the form of standards whereby disagreements and controversies are resolved.

5.4 General Conclusion

This chapter has presented three views of scientific practice and the arguments for those views: pragmatic pluralism, relativism, and perspectivism. All three are committed to a premise suggesting that elements of scientific activity are restricted to a context. Although superficially similar and motivated to address disagreement in science, each view characterizes the epistemic context very differently, giving rise to different conclusions. The pragmatist prioritizes aims and activities and is most interested in success. The relativist is interested in situated and justified judgements and how those judgements are tied to beliefs. The perspectivist focuses on the application of a taxonomy.

The pragmatist and relativist are committed to the idea that differences cannot be ranked or evaluated independently. This premise does much of the work in motivating the non-realist aspect of these views. Perspectivism, in contrast, is committed to the idea that differences must be resolved and can be resolved. In forming a resolution, one side of a debate is better defended than another. Scientists are able to evaluate, in other words, the positions and systems of others, regardless of whether we would assign the same relative evaluations to the different views in our present epistemic context. This is possible because we, and past scientists and students of science, can and do appeal to standards that are not restricted absolutely to their historical context.

Changian pragmatism and relativism share much more in common than the perspectivist view defended here. The former two both assign disagreeing scientists to different epistemic contexts and both reject the ability to rank conflicting views. Perspectivism has neither commitment and assigns disagreeing scientists to the same general context and permits contextual ranking. The first two views consider the ranking of deep-seated features of the epistemic context, whereas the perspectivist considers only restricted aspects of the epistemic context that are not so deeply seated.

6
Conclusion

This thesis is an attempt to use perspectivism as a lens for furthering our understanding of great conceptual change and disagreements in science. Because disagreements and change are topics that stimulate realism debates, perspectivism is a promising place to investigate. I showed in chapter 1 some of the tensions between realists and anti-realists. Perspectivism, as Giere articulates it, promises to mediate between these extremes, but because he over-generalizes the importance of models and because he leaves data unaccounted for, his view falls too close to anti-realism. There is, however, scope for thinking perspectivism could be developed more productively. Mitchell has done so in the context of models and integration in the life sciences and I proposed to do so in the context of conceptual change and disagreement more generally.

My main argument has been that disagreements and conceptual change are often best understood as identity problems—and hence taxonomic—not problems about existence. Consequently understanding these topics requires an understanding of the distinctions that scientists make—thought of loosely as a conceptual taxonomy—and it requires an investigation into how taxonomic issues are addressed. The advantages of this approach are several. It allows for some measure of continuity across conceptual change. It avoids whiggish interpretations of the past by showing sensitivity to change and disagreement in the historical context and without importing contemporary science. Finally, it provides some clarification on why some disagreements are difficult to resolve and result in great change.

The first step in my analysis was to address scientific evidence. Within science, one typically expects empirical evidence, especially in the form of data, to resolve disagreements, test theory, or justify theoretical change. This is the intuitive path to understanding conceptual change and disagreement. However, chapter 2 argued that there is a distinction we should make between data and evidence. I make this distinction by arguing against relational accounts that define data in terms of evidence. Rather, data are representational, but are still not mind-independent evidence because they depend upon the conceptual taxonomy used by the scientists. If the taxonomy changes, or features of the taxonomy change, then there is the possibility the data will change as well.

Chapter 3 provides clarification on what the conceptual taxonomy is and its connection to the distinctions that scientists make. I situate my view in the literature on epistemic pluralism. Many existing accounts approach pluralism from a pragmatic position. The diversity we see in scientific practice is a reflection of the complexity of the world and the diversity of aims, interests, and questions that drive scientists to investigate. I suggest there are some limits on this line of thought and argue we should understand pluralism primarily as conceptual and therefore taxonomic. I draw on resources from the philosophy of language, particularly Wittgenstein, in making this claim. Two features of this account are that it takes language to be a kind of practical ability and sharing a conceptual taxonomy is a precondition for belonging to the same practice. If scientists do not share a taxonomy, this amounts to an inability to epistemically engage with one another (because the use of a taxonomy is to be understood as a practical ability).

In chapter 4 I illustrate the perspectival view by examining the Chemical Revolution. Recent historiography has given us three questions about this period that perspectivism can address. The questions are: (1) was there a revolution; (2) what was disagreement about; and (3) was change well motivated? I revisited experimental work on acids that Cavendish, Lavoisier, and Kirwan conducted as the first step toward addressing these questions.

I showed how differences between these chemists are best understood as differences in how they individuated substances. Lavoisier made individuations using weight, whereas Cavendish and Kirwan did not, even though they were all adept at the use of the balance and weighed substances frequently. The answer to (2) is that the disagreement, because it concerned individuating substances, was a taxonomic problem. These differences did not extend across their entire experimental work, i.e. they had extensive shared methods, concepts, and explanatory interests. The new use to which Lavoisier put weight was not a revolutionary development, though it was important and resulted in substantive changes in chemistry. My answer to (1) is that the Chemical Revolution was not very revolutionary. Although there were big changes during this period, there was great continuity in concepts, methods, and explanatory aims. This is sufficient ground for denying there was a revolution.

In answer to (3), I suggested there were reasonable grounds for adopting oxygen chemistry. The adoption was not a straightforward choice because data could not be used unambiguously to refute phlogiston theory. Instead, chemists had to navigate the disagreement using what we can describe as epistemic standards. In order for an explanation or method to be comprehensible and persuasive, it must be subject to an epistemic standard. Such standards during the Chemical Revolution included precision, consistency, and continuity with past methods. The use of weight fit these standards, or criteria, better than alternatives. Importantly, these standards do not determine the outcome of a disagreement, but provide structure and context. So there are resources for resolving disagreements and these resources can have wide appeal for scientists, but data are not always the resources available, for reasons discussed in chapter 2.

In defending a perspectival interpretation of the Chemical Revolution, I argued against relativism and Changian pragmatism. In chapter 5 I contrasted those two views with perspectivism more abstractly by critically examining their commitments. Changian ragmatism and pluralism both struggle with accounting for the objectivity, or at least inter-subjectivity, of science. They place too much importance on subjective, even idiosyncratic, choices that scientists make over how to evaluate explanations, theories, models, and other products of science. As a consequence, it is mysterious how or why scientists would come to disagree. Another issue they face is that they suppose it is possible to make sweeping claims about the variety of epistemic positions, while denying that there is such a position one can occupy. Giere faced this same difficulty (discussed in chapter 1).

Perspectivism avoids both of these issues. It suggests disagreements arise between those with shared background. The evaluation of scientific products is consequently restricted to what the individual chooses or desires, but must have broader appeal, even though such evaluations are always contextual. The relevant shared background that makes these evaluations possible is the conceptual taxonomy. I avoid the “view from nowhere”—which has been a classic foil for Changian pragmatism and relativism—because I do not take disagreements to arise between systems, but in very specific contexts, i.e. perspectivism does not posit that there are scientists who make radically different evaluations while at the same time denying that we can make judgements about those evaluations (for example by ranking them).

Perspectivism, over these 5 chapters, provides a way of thinking about disagreements and change in science. It shows why these issues are so difficult by addressing the role of data and evidence in science and their dependence upon conceptual taxonomies. It provides clarification on what disagreements are about and how they can be resolved, but also why resolution is often difficult.

Does it, however, strike any kind of balance between realism and anti-realism, as promised in chapter 1? Perspectivism is compatible with two senses of realism. It satisfies the minimal realist position van Fraassen describes, i.e. a commitment to truth or approximate truth of scientific theories. That is, commitment to an explanation is not just a commitment to its success: a good scientific theory is a theory about the world, not just one that provides successful predictions. This was not part of my argument, but perspectivism as I defend it is not in tension with it.

More importantly, Perspectivism is also compatible with Hacking's definition of realism. He suggests that there comes a point when the doubting existence of something in particular becomes pointless of even meaningless, at which point we are, in a sense, realists. However, if there is no cause for doubting existence, then asserting existence does not carve a useful distinction (1983, 53), i.e. it does not add any certainty or metaphysical robustness if there is already no cause for doubt. Existence, this suggests, is not the kind of general or deep worry we might expect.

My exposition of perspectivism as a taxonomic view has been in the spirit of this view of realism. By reframing existence issues as identity and taxonomic issues, perspectivism suggests there is much less scope for doubt. Where there is no doubt, there is no ground for scepticism about existence. Hence there is a mild realism to which we can be committed, even in the face of disagreement, controversy, and substantive conceptual change.

Perspectivism is not, however, compatible with all forms of realism. My characterization of taxonomies as practical abilities may be in conflict with the realist semantic commitment, which requires terms to have extension. The metaphysical commitment may also sit uncomfortably with perspectivism because the realist wants to claim that entities exist full stop, whereas the perspectivist suggests the realist question often cannot be posed. This is because it is not existence that is at stake, but identity (also understood as how scientists classify the parts of the world they study). Perspectivism is not, however, in conflict with the epistemic commitment (that our theories are in some sense true or approximately true and thus involve stronger commitment than some anti-realists suggest).

A number of important questions remain; and there are many further areas for development, both about realism generally and about perspectivism. Some are particularly pressing. Doubt plays an important role not only in motivating realism, but also in motivating conceptual change. Further developing this concept would improve our understanding of when realist questions do sensibly arise. I have, after all, been suggesting that identity (or taxonomic) questions arise first, but not that realist questions do not arise at all. Another topic that needs further clarification is the depth of the distinction between more abstract theory and scientific practice. I have devoted little space to abstract theory in preference to examining practice, but the two are obviously related in important ways and the distinction is often, but not always, robust. Finally, it is likely there are a great diversity of types of disagreement that are not easily captured by my view, or that are at least interesting and in need of different philosophical analysis. And many other disagreements, contemporary and historical, could probably be fruitfully analysed using the lens of a perspective.

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