Pluralism, Truth and Synthesis

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A Thesis Submitted to the School of Graduate Studies in the Partial Fulfillment of the Requirements for the Degree of

PhD

in

Philosophy

McMaster University
Hamilton, Ontario

PhD (2025)
Philosophy
McMaster University
Hamilton, Ontario

TITLE: Pluralism, Truth and Synthesis

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NUMBER OF PAGES: viii, 127

Lay Abstract

How far can we tolerate or encourage differences between approaches to scientific research of the same phenomena? That is, when do such differences become conflicts which researchers need to resolve? Scientific pluralists argue that we can tolerate or encourage many approaches, and that a plurality of approaches is what makes science objective and successful. On this view, the structure of scientific practice is plural across the board. However, pluralists have had trouble arguing this without undermining the objectivity of science. And a study of the evolutionary synthesis (c. 1920-1950) shows that even when there are many different approaches to a problem, they conflict in scientific practice until proven otherwise. A plurality of approaches is not what makes science objective and successful; instead, it is a striving for truth and the friction between different approaches.

Abstract

How far can we tolerate differences between approaches to scientific research of the same phenomena? When do differences become conflicts which researchers need to resolve? Scientific pluralists argue that we should tolerate many approaches, because plurality makes science objective and successful. On this view, the structure of scientific practice is uniformly plural. Pluralists seek a middle way between relativism, where there is no epistemic objectivity or success in science, and monism, where there is only one correct approach to scientific research about some phenomenon.

Scientific pluralism has three core commitments: the philosophy of science should be empirical; philosophers should not assume that scientific research will reach a particular outcome; plurality is constitutive of successful science.

Chapter 1 introduces those commitments and raises a theoretical problem for scientific pluralism. It looks at two epistemologies of science, from Longino and Chang, which draw a distinction between different and conflicting approaches. However, neither epistemology firmly separates itself from relativism.

Chapter 2 raises an empirical problem. It studies the evolutionary synthesis (c. 1920-1950), a period when there was a plurality of approaches. However, a close reading of work by Sewall Wright, R. A. Fisher, and Theodosius Dobzhansky shows that plurality is not met with tolerance in scientific practice, but with friction and conflict.

Chapter 3 argues that we can solve both problems by dropping the third core commitment. To do justice to criticisms of scientific monism, we should also say that there is no uniform structure to successful science. We may call this view methodological monism. The structure of scientific practice is variable, moving through stages of relativity, plurality and unity in a nonlinear fashion. But, drawing on Peirce and Price, scientific practice nonetheless has an overarching goal: striving for truth, aided by the friction between different approaches.

Acknowledgements

I thank my supervisor, Dr. Alex Klein, for his patience and encouragement. I am especially grateful for his relentless comments on my work, which helped me see which directions were most fruitful and interesting.

I also thank my supervisory committee members, Dr. Sandra Lapointe and Dr. Megan Stotts, for their fresh perspectives, helpful questions, and professional mentorship. And the external examiner, Dr. Holly Andersen, for her challenging and constructive comments.

Many colleagues and staff members were also indispensable to me throughout my time at McMaster. I thank the department staff and leadership, including Dr. Mark Johnstone, Rabia Awan, Angeli Busa, Alina Dawood, Kim Squissato, and Kristina Vukelic, for helping me navigate the financial and bureaucratic hurdles of doctoral studies. I thank my colleagues, including Taylor Aitken, Nigel Freno, Scott Metzger, Brent Odland, Siddharth Raman, and Isaias Ruiz, for their discussions, support, and friendship.

Finally, I thank Ryan and my family for their love, faith, and curiosity, which helped me both to focus on my work and to keep perspective.

Declaration of Authorship

I, Stephen Ross, declare that I am the sole author of this dissertation, which was prepared with help and advice from my supervisory committee.

Contents

Introduction

Scientific pluralism developed in the late 1970s. Since the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962, anglophone philosophers of science had been rethinking the relevance of the history and sociology of science for their discipline. This coincided with a questioning of old doctrines, especially those of the logical empiricists.[1] Logical empiricism had promoted the unity of science, though this idea had many interpretations. One interpretation, held for example (though in different ways) by Rudolf Carnap and W. V. O. Quine, was that the sciences should be unified by a common logical language which defines clearly what it means for statements to depend upon experience.[2] In another vein, Ernest Nagel argued that the special sciences should be translated and reduced by bridge laws to be special cases of the basic sciences. Carl Hempel had similarly argued that the unity of science rested on the clarification of theoretical terms; he also worked on a uniform standard of confirmation and explanation across the sciences.[3]

However, this was not the only interpretation. Another, present in Otto Neurath's writings for example, developed a sociopolitical view of the unity of science, as opposed to the logical view. Given the right scientific institutions, it would be possible to use the sciences' free and exact inquiries to promote the general welfare and counter ideologies. This interpretation owes itself in part to the political experiences of the logical empiricists in Europe, but also to their interaction with the American pragmatists, such as John Dewey and Charles Morris.[4] After the rejection of logical empiricism, concerns about the social impact of the sciences and its relationship with both liberalism and socialism continued to grow.

[1] Ludwig and Ruphy 2021 and Richardson 2006, p. 16.

[2] See Frost-Arnold 2013, pp. 117-138.

[3] Richardson 2006, p. 17.

[4] Richardson 2006, pp. 14-16 and Reisch 2005, pp. 27ff. and 83ff.

It will be helpful to note two ideas in Kuhn which had a large influence on pluralist philosophy in the decades to follow, before we examine scientific pluralism in more detail. Kuhn was at once deeply influenced by the preceding logical empiricist views and also one of their most potent critics. Like Carnap, he believed that the sciences are organised into two levels: a higher level (for him, the paradigm) which gives the rules and basic concepts of the science, and a lower level which takes the higher for granted and works out the implications (for him, normal science). Again like the logical empiricists, he believes that the higher level can undergo abrupt and drastic changes where an established theory is displaced by another. Unlike the logical empiricists, he denied that we can objectively measure progress across those changes: progress is internal to a paradigm, and paradigms are incommensurable, so there is no such thing as a shared standard by which their views can be rationally compared. Kuhn also held that, faced with an anomaly, we could modify the theory, or change our equipment and methods, or ignore it as a fluke with an unknown but harmless cause.[5] Later, this view, which was expressed before in different ways by Duhem and Quine, came to be known as the underdetermination of theory by evidence. It had great influence on pluralist philosophers of science in the decades to follow.

Kuhn also flipped the received view of the sciences' social influence on its head. Following the French historical epistemologists, Kuhn emphasised the effects that social settings have on scientific research, rather than the sciences' ability to influence political life. Insofar as the sciences are objective, it is not because they offer an external standard to correct the limitations of social life, but because social life enables them to reach beyond the limits of their current experience.[6] This idea, alongside his paradigm theory, branded him a relativist in the eyes of many, but nonetheless it gained purchase over time. For example, Paul Feyerabend argued that all attempts to find a common rational ruleset for scientific rationality fail against the historical record, where we see so much variation in arguments, techniques and standards. If the epistemology and logic of science were to be faithful to the success of science so far, then they must hold that "anything goes."[7] In

[5] Kuhn 1962, p. 144ff.
[6] Another influence for this view was Michael Polanyi's *Personal Knowledge*, Polanyi 1958.
[7] Feyerabend 1975, pp. 7-9.

a different vein, Jerry Fodor argued directly against Nagel's bridge principles in light of Kuhn. For him, the sciences could not be unified, because many sciences operate with special laws which cannot be reduced to more general laws without distorting or losing their content.[8] Lastly, Ian Hacking argued that scientific activities can create constitutive styles of reasoning which are incommensurable with other styles. They are constitutive in the sense that they shape and delineate a scientific discipline against other disciplines.[9]

Early scientific pluralists tended to write about biology, though not all philosophers of biology were pluralists. Where before the physical and exact sciences had dominated most discussions, by the 1980s there was a renewed collective interest in the life and social sciences. This is evident in the widely read works of David Hull, Philip Kitcher and Nancy Cartwright.[10] Reductionist and monist faced bigger challenges against examples from biology, since the theories and methods differed from physics, if they even had anything analogous to a physical theory. Biology and the social sciences tended to have many competing and alternative views existing alongside each other; studies of these examples fuelled pluralist ideas.

Scientific pluralism, then, descends from the reaction against logical empiricism, alongside relativistic views like Feyerabend's. But pluralists, unlike relativists, believe that the success of science is only possible because of epistemic constraints. It cannot be that anything goes, since then everything could count as successful, when in fact there is something distinctive about the knowledge we produce by scientific research. Science's distinctiveness is not accidental, but instead enabled by its epistemic constraints, so that we cannot hold science in equal regard (epistemically, anyway) with all other ways of producing knowledge.

How successfully have pluralists distinguished themselves from relativists? To answer this, we will look at the largest group of pluralist philosophers of science, which first coalesced in the "pluralist stance" of the mid 2000s, and continued developed further afterward. The pluralist stance synthesises various strands of

[8] Fodor 1974.

[9] Hacking 2002, pp. 178-200.

[10] Hull 1974, Kitcher 1982 and Cartwright 1989.

argument and research from the preceding decades. The programme for this synthesis was first laid out in *Scientific Pluralism*, a collection of essays published in 2006 and edited by Stephen Kellert, Helen Longino, and C. Kenneth Waters, all of whom had past or current affiliations with the Minnesota Center for the Philosophy of Science.

They, along with their many collaborators, pursued three primary aims in this text. They trace the historical development of pluralism, including the background we just sketched. They create a classification of types of scientific pluralism, with their different treatments of the metaphysical, epistemic, political, historical, and social elements of science. And they articulate the core commitments which any pluralist philosophy of science needs to hold, if it is to be pluralist rather than monist or relativist. The third aim most concerns us here, although we will necessarily touch on the other two at different moments.

We may paraphrase the core commitments as a list of three. These are core commitments because they are taken to be necessary to avoid monism and relativism.[11] First, the philosophy of science must adopt an empirical method, whereby its conclusions (including normative conclusions) are supported by history and scientific practice. Second, the philosophy of science cannot presuppose that the sciences must take a particular direction; philosophy has to let the sciences be and do what they will. Third, the sciences have been successful at producing knowledge, and the pluralist structure of science explains how that success is possible.

In the years following the 2006 volume, the pluralist stance underwent developments and criticisms which will be relevant to our discussion. In her book *Scientific Pluralism Reconsidered*, Stéphanie Ruphy has argued for a reform of the pluralist stance. She argued that it maintains a metaphysical commitment, that the structure of the world explains why science must be pluralistic. Ruphy favours a methodological pluralism which assumes less about the connection between scientific models and the world. This criticism is in line with the second core commitment. Another example is Hasok Chang, who in *Is Water H2O?* has

[11] This is a paraphrase because they do not list them as such. The commitments are drawn mainly from Kellert, Longino, and Waters 2006, Giere 2006, and Longino 2002. How these are drawn will be treated in more detail later.

argued that the pluralist stance is too passive. Rather than seeking to describe the pluralist structure behind science's success, philosophers should actively promote the further pluralisation of the sciences. This criticism is in line with the third core commitment. Even while Ruphy and Chang move beyond features of the initial proposal in *Scientific Pluralism*, they both agree on the three core commitments mentioned above.

The pluralist stance faces two problems, one theoretical and one empirical. The theoretical problem is that they struggle to distinguish their view from epistemic relativism. To avoid relativism, there must sometimes be conflict between scientific approaches. Otherwise, they would all have equal standing epistemically. A pluralist epistemology must therefore explain how to tell whether two scientific approaches are merely different, or whether they conflict and require a resolution.

Among adherents of the pluralist stance, Helen Longino and Hasok Chang each offer an epistemology of science with this issue in mind. On their views, communities or systems are the contexts in which conflict properly happens. Outside these contexts, differences become too large to constitute genuine epistemic conflicts. Communities and systems are also the units of success. To explain how they can be judged unsuccessful, Longino and Chang each distinguish between values internal to a particular system and those relevant to assessing the health or effects of systems. The latter explain what is distinctively scientific about these many systems, which otherwise may not seem to have much in common.

However, neither Longino nor Chang succeeds in eliminating epistemic relativism from their epistemology. The problem is that research communities judge success and truth by their own criteria, and therefore also interpret these supposedly external values according to their own internal ones. It is impossible to assess whether a system is epistemically healthy or beneficial without relying on the system's internal values. For similar reasons, judgements about which approaches conflict also have to be judged by internal standards. Since those standards must and do vary across research communities, there can be no clear line, even a clear but variable one, between conflicts and allowable differences.

As for the empirical problem, it is that the pluralist stance does not adequately mirror the norms of scientific debate and research in its epistemologies. The history and practice of science are in fact full of conflicts. By itself, this is not problematic, but in practice, the pluralist stance favours (when possible) interpreting conflicts as either unreasonable or unnecessary. When they interpret them as unreasonable, it is because the scientists were unduly committed to scientific monism, or else they just acted in too partisan a manner; either way, they failed to recognise the pluralist structure which made their work possible. Hasok Chang offers such an interpretation of the chemical revolution, as C. K. Waters does of gene centrism in biochemistry, Ruphy of galactic models, and Longino of behavioural science and evolution. These interpretations prioritise the third core commitment of the pluralist stance, that the pluralist structure of science is constitutive of its success, over the first core commitment, empiricism. Since the science in question was successful, the philosophers seek a pluralist explanation which bypasses or underlies the scientist's monist practices.

The issue is that this interpretation erases the friction and tension between different approaches which pervades scientific research. Ideas, models, methods and so on often act as rivals; there is real friction between them. This is true in practice, whatever we might want to say in theory. Tension and friction engender disagreement, which motivates scientists to improve their own approaches and carefully consider the merits and defects of their rivals. A philosophy of science committed to empiricism should capture this key element of scientific progress; failure to do this is an empirical problem.

To illustrate the empirical problem, we will use the evolutionary synthesis and its historical underpinnings. This historical narrative will include long term points of friction: debates carried over decades between naturalists and experimentalists in the late nineteenth and early twentieth century. It will also include a more detailed look at individual episodes: R. A. Fisher and Sewall Wright's debates about the forces behind species adaptation, and Theodosius Dobzhansky's arguments in favour of a synthesis of population genetics and evolutionary theory. This example is fitting for a few reasons. First, because of the importance of the philosophy of biology, many philosophers have examined parts of this period already, so it is easy to compare analyses directly. Second, the scientists working during the evolutionary synthesis were explicitly interested in questions of unification and diversity of approaches, so that we can see more clearly how their practices relate to these questions. And third, it constitutes a difficult test case for monism as well as pluralism, because the synthesis ultimately did not succeed in unifying biology into a single discipline governed by a single theory.

Having argued that the pluralist problem faces a theoretical and empirical problem, it makes sense to reappraise scientific monism, insofar as epistemic relativism is still unpalatable. We will then construct an alternative view, methodological monism. On this view, scientific practice works with a monist concept of scientific truth. Although it would be too much to develop an entire epistemology here, we will instead look at two desiderata which it should satisfy. These desiderata are two functions with this monist concept of truth must fulfill, drawn from C. S. Peirce and Huw Price. The first is enabling hypotheses: our epistemology must motivate hypothesising, the practice of generating serious candidate answers to a question. The second is disagreement: truth must motivate criticism between two different positions on the same question. Both of these together imply that differences cannot be a reason against pursuing scientific inquiry.

In constructing this view, we must learn from the lessons of the pluralist stance's first and second core commitments, while rejecting the third. We will see that David Hull and Philip Kitcher tried to take these commitments seriously, but, as Longino argues, ultimately failed in the end. They failed because they remained too committed to a uniform monist structure to the sciences, and to a standard of truth which comes from beyond scientific practice. So just as we should reject the view that pluralism in science is constitutive of success, we should also reject a commitment to a uniform monist standard of success.

A variable structure takes the place of the uniform one. Progress happens through recurring stages with quite variable structures, following nonlinear paths. They resemble relativism, pluralism and monism as philosophical views, but they are strictly speaking conditions of scientific practice rather than a theory of it. To distinguish them from the philosophical views, we will call the stages relativity, plurality, and unity respectively. This structure is variable and nonlinear there is no guarantee that scientists will reach the third stage, or that, once they have reached a particular stage, they will remain there. Scientific practice follows norms which tend toward unity, but this tendency is frequently frustrated and is by no means guaranteed. This allows us to account for the plurality we see in scientific practice while accounting for the monist norms scientists seems to follow in that stage. Thus methodological monism shares the pluralist stance's commitment to empiricism in the philosophy of science, and its commitment against determining a particular end to scientific inquiry.

Chapter 1

Between Monism and Relativism

Scientific pluralism brings the following problem to our attention: how different can two approaches to the same phenomena can be without conflicting? It is possible to interpret this problem descriptively and normatively. On the descriptive interpretation, the question is whether the sciences have allowed many different approaches to coexist, what the extent of those differences was, and whether there are any standards of interpretation (that is, interpreting scientific practice) on which they did not conflict. The normative interpretation asks whether it is ever rational for the sciences to entertain multiple approaches and, if so, which sorts of differences are allowable. Often, as we will see, these interpretations are taken to be exclusive. But another option is to reconcile the two, so that there is no divide between the human aspects of science and its rationality. Here, we will first see how the problem of differences and conflicts helps us understand what is distinctive about scientific pluralism and how to understand the variety of scientific pluralisms. Then we will follow one tradition in detail, the pluralist stance, which tries to reconcile the interpretations by giving epistemologies of scientific practice.

"Approach," as in "scientific approach," "approach to science," is a common term in this debate. It is a deliberately and usefully vague term. Pluralists and their interlocutors disagree about the nature of scientific knowledge and progress, and "approach" does not presuppose anything substantive which is up for debate. The sciences are incredibly diverse in their methods and results, so it would defeat the purpose to presuppose a notion of science which legitimates only some approaches. It is problematic, at least before any argument is given, to refer to theories to the exclusion of models, or propositional knowledge to the exclusion of material systems, or beliefs to the exclusion of actions, and so on.

Since we will use the term "approach," let us clarify what it means without adding any inconvenient presuppositions. An approach to learning about some scientific phenomena aims for a better understanding of those phenomena. Approaches to science, then, are *about* the phenomena being researched. Because of this aboutness, they are, in a very general sense, representations. Note that this sense of representation is not limited to producing structurally similar images of the phenomena, but simply learning about them and understanding them in some way.[1] Many parties in the scientific pluralism debate have already adopted this framing, because it allows for sharper argumentation between one another without narrowing the debate.[2] Sometimes, when it helps for clarity, we will refer to representations as parts of an approach.

A scientific monist characteristically requires scientific approaches to converge and unify in some way. An epistemic relativist characteristically denies that one scientific approach can be true at the expense of others, so they do not require that different representations converge or even that they cohere in any particular way. Pluralists, as has been said, seek a middle way. They want scientific approaches to be subject to epistemic constraints, where those constraints tolerate enough differences that they do not need to unify; they can say something substantively and irreducibly different about the phenomena. This gives rise to a natural question which pluralists have so far been unable to resolve. Which differences are the tolerable ones, and which ones are conflicts?

[1]This framing is influenced by van Fraassen 2008, Millikan 2017 and C. S. Peirce. Each of them gives a triadic view where a representation is not just related to an object, but also to someone (or something) interpreting it.

[2]For an analysis of the framing, see Ruphy 2016, p. 80ff. This is independent of a related debate in the philosophy of science about whether the aim of modelling is to depict or describe something, or instead to manipulate things and expand our technological capabilities. In the context of that debate, the former view is called representational, and the latter antirepresentational. For key contributions to that debate, see Hacking 1983, Giere 1988, Suppe 1989, Rheinberger 1997, Cartwright 2019, and Frigg and Nguyen 2020. On the general sense of representation we will us, that debate would be about what kind of representation a model is, rather than whether models are representational.

The argument will proceed as follows. First, we will become familiar with the pluralist stance. This will include becoming familiar with its definitions of monism and pluralism. These definitions give a framework for the volume *Scientific Pluralism*, but they also give us an initial classification of views, pluralist and otherwise, within the broader debate. Proponents of the pluralist stance claim that adhering to its commitments is the only genuine alternative to monism and relativism. We will accept this claim for the time being, after explaining what it means with examples.

We will then examine a question at the heart of the pluralism debate: when do representations (or approaches) conflict, and when do they merely differ? Our first exposure to this question comes from Ronald Giere's contribution to *Scientific Pluralism*, but it is central to pluralist works from before and afterward. For example, this distinction lies at the heart of two other classifications of pluralism, one from Michael Dickson (in his contribution to the same volume) and another from Stéphanie Ruphy's 2016 work, *Scientific Pluralism Reconsidered*.[3] These classifications focus on the sort of differences which are designated conflicts. They also raise epistemological questions about how differences and conflicts are found, justified or resolved.

These classifications will make it convenient to look at two arguments against the pluralist stance, one from Ruphy and another from Hasok Chang. Ruphy argues that the pluralist stance carries too many metaphysical commitments, since the editors explain the plurality of scientific representations by appeal to a fractured structure of the world. Chang argues that the pluralist stance is too passive; it encourages toleration and acceptance of pluralism in science, but it would be better (Chang argues) to encourage its active development. Both of these arguments are appealing, though they are not really criticisms of the pluralist stance's core commitments. Instead, they are criticisms of the editors' adherence to those commitments. Ruphy's argument is that the editors do not adhere to the second commitment, that the result of science should not be presupposed. Chang's is that the editors do not properly appreciate the third, that the pluralist structure of science enables its success. As such, we can label Ruphy and Chang as adherents to

[3] And later, also her Stanford Encyclopedia of Philosophy article with David Ludwig.

the pluralist stance. These two also illustrate why epistemological questions are central to answer the central question of scientific pluralism: when do different representations merely differ, when do they conflict?

After having become familiar with the pluralist stance, we will look at two epistemologies of science from its adherents. These views stand out because they directly address the question about conflicts and differences, and try to explain just how pluralism is supposed to enable scientific success. The first is Helen Longino's from *The Fate of Knowledge*. Her aim in this work is to dissolve the rational-social dichotomy. To accept this dichotomy is to suppose that monism and relativism are the only possible positions about scientific knowledge. In its place, she develops a pluralism epistemology. On her view, scientific knowledge encompasses all different types of scientific representation, whether exact or approximate, propositional or not. And it is organised into epistemic communities with their own interests, aims and values, which together constitute a local epistemology.

The second epistemology is Hasok Chang's from *Is Water H2O?*. He gives an operational analysis of scientific research, in which actions he calls operations coalesce into coherent epistemic activities, which then form systems of practice. Systems of practice have their own constitutive standards of success, which also serve as constitutive standards of truth. The shape of these standards depends partly on the system's operations, but also on its aims and values.

While these epistemologies easily distinguish themselves from scientific monism, they fail to distinguish themselves from epistemic relativism. Longino and Chang try to give global standards to evaluate epistemic communities and systems of practice, so that there are some constraints over and above their local interests and values. But these constraints, including their notions of truth and justification, are incomplete on their own. To be usable, they require interpretation within the framework of those very local interests and values. Judgements about epistemic communities or systems of practice are therefore relative to the interests and values of some community or system.

1.1 Pluralism and the Pluralist Stance

In 2006, the volume *Scientific Pluralism* was published with articles by many pluralist philosophers of science. The editors and contributors collected information about scientific pluralism which until then had been spread across many monographs and articles. Because of the volume's breadth, its contributors were able to articulate what pluralists share in common and also where they differ. The point of the volume was to sort through the existing views and give a programme for future development.

One part of that programme was to establish scientific pluralism's core commitments, which we can condense to a list of three. First, the philosophy of science must adopt an empirical method, where its conclusions, including normative conclusions, are supported by and inferred from the history and practices of the sciences. Second, the philosophy of science cannot presuppose that the sciences must take a particular direction; philosophy has to let the sciences be and do what they will. Third, the sciences have been successful at producing knowledge, and the pluralist structure of science explains how that success is possible. These commitments will become apparent as we go along.

Another part of the programme, which we will discuss now, was to argue against attempts to adopt some of these commitments while compromising with monism or relativism. The editors' argument begins with a definition of scientific monism.

> We take *scientific monism* to be the view that
>
> 1. the ultimate aim of a science is to establish a single, complete, and comprehensive account of the natural world (or the part of the world investigated by the science) based on a single set of fundamental principles;
>
> 2. the nature of the world is such that it can, at least in principle, be completely described or explained by such an account;
>
> 3. there exist, at least in principle, methods of inquiry that if correctly pursued will yield such an account;

4. methods of inquiry are to be accepted on the basis of whether they can yield such an account; and

5. individual theories and models in science are evaluated in large part of the basis of whether they provide (or come close to providing) a comprehensive and complete account based on fundamental principles.

From this we can glean that scientific monism says something about the metaphysics of the world, methods of inquiry, the methods and the representations they produce, and how to evaluate them. Each of these components, on the monist view, work in harmony together, forming one complete system of research and knowledge. Having defined their opponent, the editors now define pluralism negatively: it is the view that "there are no definitive arguments for monism and that the multiplicity of approaches that presently characterizes many areas of scientific investigation does not necessarily constitute a deficiency."[4]

Because these definitions of monism and pluralism are meant to capture a very wide range of views, and also because they include several clauses, the definitions admit a broad range of plausible interpretations. Let us begin by narrowing this range. Following the natural thought that monists and pluralists should have contrary opinions, we might read the definition of pluralism as denying the definition of monism, taken as a five part conjunction, after which they add that there is no reason to be distraught by the resulting disunified picture of the sciences. However, this reading is problematic. Simply denying the conjunction would mean someone could be a pluralist by believing that the aim of a science is to establish a single comprehensive account of its subject matter, as long as they deny that there are methods of inquiry that allow its aim to be realised. Or they could be a pluralist by believing that there are methods that would achieve that aim, but that the fact they have not been achieved is not a deficiency. Nowhere in the volume's contributions, or in the broader literature, do such views appear. The reason is simple: scientific pluralists are just as much champions of the success of science as

[4]Kellert, Longino, and Waters 2006, p. x.

monists are. The aim of science should be achievable, and if it is achievable, we should go about it.

This observation reveals some internal conditional structure to the definition of monism. The first conjunct should imply the second and third (in other words, the second and third are necessary conditions for the first). As for the definition of pluralism, the second part is no afterthought, but an affirmation that the pluralist vision of science is constitutive of successful science. All of this means that the first three monist claims, taken together, are false or lacking evidence, and the last two claims are false taken individually. The overall point is that monists have unrealistic standards for the sciences, which pluralism relaxes.[5]

The first clause of the monist definition refers to "the ultimate aim of a science." Using the indefinite article in "a science" means that even someone who believes that the sciences do not have a reductive, nesting structure can be a monist. They just have to believe that each special science has a set of fundamental principles which allow a single, comprehensive account of their domain.[6] Another example is the phrase "at least in principle" in the method clause. This would imply that even someone who agrees with a pluralist account of current or past science can be a monist, so long as they believe in the ideal of a unified method of inquiry.

Putative forms of pluralism which fall into these monist traps are called "modest pluralism." It might be clearer to call them cryptomonists, because the point is that they are not pluralists after all, but have lingering monist commitments. The editors cite two examples. One is Sandra Mitchell, who has argued that causal explanations involve idealisation and abstraction, creating a patchwork of

[5]Their sense of realistic is much the same as Susan Haack's, although she is not a pluralist: "neither too optimistic nor too pessimistic, fully acknowledging the achievements of science, but also the pervasive fallibility, the imperfections and flaws, the sheer untidiness, of this remarkable but thoroughly human enterprise." Haack 2011, p. 123. Of course, pluralists like Hasok Chang and William Wimsatt have also expressed similar ideas: "I want to propose a realistic kind of realism, close to what William Wimsatt has called 'realism for limited beings in a rich, messy world', which is 'a philosophy of science that can be pursued by *real* people in *real* situations in *real* time with the kinds of tools we *actually* have'." Chang 2022, p. 2, Wimsatt 2007, p. 5.

[6]Such views became relatively common after Jerry Fodor's criticisms of theoretical reductionism in Fodor 1974; in fact, Fodor's article was an important precursor to scientific pluralism. As for the view that there is only one true all-encompassing representation, the editors give it the name "fundamentalism." Kellert, Longino, and Waters 2006, p. xv.

different explanations. For example, there might be three selection models for ant behaviour: one based on chromosomes, another on colony structure, another on learning patterns. When these models are all correct, it is possible to integrate them to get an overall picture of the phenomenon.[7] The monist commitment behind this view is that these explanations can be stitched together. Integrating models allows scientists to build a single account of a natural domain, even if its structure does not fit the model of a theory of everything.

The second example is Phillip Kitcher, at least in his work of the 1980s and 1990s.[8] He accepts that researchers will have different interests in studying the same phenomena, which can lead to different representations, but requires that all true statements from one theory or model be translatable into true statements in another. The monist commitment here is intertranslatability, a much stronger condition than Mitchell's patchwork integration. Intertranslatability assumes that plurality is a temporary feature of our interests and schemes, and perhaps our ignorance and limitations too. The pluralist stance, by contrast, does not assume that plurality is a temporary feature of current interests.[9]

The working definition of pluralism, quoted above, says that multiple accounts do not constitute a deficiency. In light of pluralists' other commitments, this means that the success of science is compatible with multiple accounts, even without reduction, intellectual competition, completeness, intertranslatability, or integration. Without the context of those other commitments, however, there is another way to understand this: that there is no need for a normative epistemic evaluation of the sciences. This is the relativists' view. The editors call pluralists who veer too close to this view radical pluralists, who are in essence cryptorelativists. Their example is John Dupré, who advocates for conceptual relativity as a metaphysical basis for science. On his view, which he calls promiscuous realism, classification schemes and ontological commitments rely on our interests. There is an indefinite number of such schemes, and they all have equal claim to truth.[10] The problem with this

[7]Mitchell 2002, pp. 62-66.

[8]Especially Kitcher 1993. The Kitcher of the later 2000s onward, whom we will see later, changed his approach to these questions.

[9]Kellert, Longino, and Waters 2006, p. xii.

[10]Dupré 1993.

is that it makes scientific ontology too arbitrary; successfully articulating an alternative is enough to justify it.[11] In the context of this debate, relativism always means epistemic relativism where, in Martin Kusch's terminology, alternatives are equally valid, at least as far as epistemic norms are concerned. What makes something knowledge is therefore a matter of contingent, local, social factors, where these are understood to be held apart from epistemic norms.[12]

The point of these criticisms is that establishing pluralism as an independent position is difficult. The pluralist stance tries not just to modify a monist position so that it can accommodate moments of pluralism or new types of conflict in an otherwise monist narrative. Nor is this view about rejecting the old standards entirely in favour of an open, virtually unrestricted epistemology of science. Instead, the pluralist stance strives to keep robust constraints on research, thus avoiding relativism, but not so many that successful research is unrealistic, thus avoiding monism. The core issue with monism and relativism is that they are "metaphysical or ideological" positions, absolutist stances about what the sciences can be. The pluralist stance tries an empirical approach instead. It neither requires nor precludes that plurality is caused by the way nature is, nor does it say in advance which classification schemes are acceptable. Instead, they provide empirical case studies to gauge the scope and limits of scientific representation. They reject any assumption that it is always possible to render scientific representations (models, theories, measurements, and so on) consistent or commensurable with each other. And they reject the view that any representation can work well enough, as long as it serves someone's interests.

Yet their reasons are not entirely empirical. They also analyse the nature of representations. "All representations," they argue, "are partial in that any representation must select a limited number of aspects of a phenomenon (else it would not represent, but duplicate). This selective and partial character of representation means that alternative representations of a phenomenon can be equally correct." At face value, a monist or a relativist might be perfectly comfortable accepting this claim. The differences between the three camps arise from further assumptions.

[11]Kellert, Longino, and Waters 2006, p. xiii.
[12]Kusch 2020, pp. 2-3 and 18-19.

Monists, for instance, assume "that all such correct accounts can be reconciled into a single unified account or that there is a single perspicuous representation system within which all correct accounts can be expressed."[13]

Their argument against this is an abstract analysis of experimental practice. Representations focus on certain features of their target while distorting others. According to the editors, distortions are especially prominent when we "parse causes." To parse causes means to select features of a phenomenon or process to the exclusion of other features. In effect, it is a judgement about what is relevant; every causal explanation leaves some factors out as irrelevant. It is possible to have different causal explanations of the same phenomenon which focus on different features. They will often say things about the target which are inconsistent with each other, even while we are happy with the accounts considered separately. On the editors' view, this may sometimes be because of different interests (which determine what is relevant), or sometimes because nature is too complex to be captured by mutually consistent representations, or again because abstracting away from the phenomenon necessarily distorts something about it. [14]

The act of "parsing causes" is similar to what Mitchell calls "idealisations." Ken Waters' case study of molecular biology, later in the same volume, nicely illustrates this connection. Genetic theory, absent competing theories or models, dominates molecular biology. Some philosophers have argued that the genetic approach needs to be replaced because it leaves too much out of the causal picture of organisms, namely the effects of the environment on their genetic expression, and the effects of other systems on their phenotype. A proper theory of the organism would include the entire picture, even if that ends up being a patchwork of different models as on Mitchell's view. Waters counters that this is only necessary on monist assumptions about what a successful explanation looks like. The genetic approach offers perfectly workable explanations of biological processes, even if they are partial, and even if there is plenty of room for other approaches (possibly inconsistent ones).[15] Once again, pluralists are just as committed to the success

[13] Kellert, Longino, and Waters 2006, p. xv.
[14] Kellert, Longino, and Waters 2006, pp. xiv-xv.
[15] Kellert, Longino, and Waters 2006, p. xix and Waters 2006, pp. 199-201.

of science as monists are. Respecting, or preserving, the success of science means eliminating unrealistic standards for scientific representation.

If pluralists give decisive arguments that monist standards are unrealistic, then monists are faced with the choice either to settle for pluralism's revised standards or to adopt epistemic relativism. But what would it take for such an argument to be decisive? Waters argued that genetic research was successful, but only if we do not take it as a totalising view of the organism. But one person's *modus ponens* is another's *modus tollens*. Waters supposes that the genetic view of the organism was successful, so that we cannot take it be a totalising view. But we could satisfy that "only if" conditional equally well by affirming that the genetic approach was a totalising view and concluding that it was not successful. At issue here is not necessarily the evidence itself, but more importantly how to interpret and weight the evidence. Judgements of success are interpretations of some scientific research, but the effect these judgements have on an argument will depend on the philosopher's broader theoretical commitments. An epistemology of science which settles these prior questions is necessary.

1.1.1 Other Classifications of Pluralism

The only classification of views we have seen so far is polemical, in that the pluralist stance enjoys the status of true pluralism. The reason for this was that any deviation from the pluralist stance's core commitments led either to monism or to relativism. If this claim is correct, then it will also be apparent after looking at more neutral classifications; neutral in that they are not constructed to show that there is only one kind of scientific pluralism. This is what will do now, by looking at work by Ruphy, Ludwig, and Dickson.

Broadly, there are two ways to distinguish between types of scientific pluralism. One considers which aspects of science are pluralised (multiple theories, or multiple methods, and so on), whereas the other considers which kinds of difference are allowed or, more strongly, necessitated (contradictions, or incommensurable commitments, and so on). The first way prioritises ontological questions about science, what science is and consists of. The second way prioritises epistemological questions about science, what kinds of warrant scientific results require.

Ruphy offers a classification of the first kind, where pluralism can be ontological, methodological, or representational. Ludwig and Ruphy later revised this, using epistemic in place of representational.[16] Ontological pluralism is the view that phenomena and reality science explains are plural: the entire world perhaps, or genes, species, stars, galaxies, and so on. Ontological pluralism is closely related to pluralism about classifications; if there are multiple classifications of a species, then there is a plurality of kinds of that species.[17] Methodological pluralism is the view that there is no single overarching method which the sciences share, but instead there are multiple methods (whether they are shared or distributed piecemeal). Representational pluralism is the view that theories and models about the same phenomena must be plural; this is already familiar from our discussion of the pluralist stance.

These three views are not strictly orthogonal. For example, Ruphy classifies Kuhn's paradigm theory as a "ontologico-methodological pluralism," because each paradigm has its own metaphysical Gestalt and its own way of organising normal research. Ruphy cites Nancy Cartwright as an example of ontological pluralism due to her dappled world hypothesis, which is an argument against the uniformity of nature. But Cartwright spends just as much time arguing for pluralism about models and experimentation, since these are necessary if we are to study a dappled world.[18] Ruphy classifies the editors as representational pluralists (epistemic, on the revised classification). But as she observes, and as we have seen ourselves, their claims about justifying representations are often supported by ontological and methodological arguments. What this classification gives us, then, is not a strict partitioning of pluralisms, but a way to see which aspects of science a pluralist emphasises in their arguments.

As for the other way, distinguishing pluralisms by the kinds of difference they allow, Michael Dickson offers one in his contribution to *Scientific Pluralism*. He

[16] Ruphy 2016, pp. xv and 34, and Ludwig and Ruphy 2021. Ruphy's original category, representational pluralism, was influenced by the editorial introduction in *Scientific Pluralism*.

[17] Ereshefsky 2001, another adherent of the pluralist stance though not a contributor to the 2006 volume, argues for classificatory pluralism in biology.

[18] The relevant texts for Kuhn and Cartwright are Kuhn 1962 and Cartwright 1999. Cartwright focuses on models and experiment in Cartwright 1983 and Cartwright 2019.

distinguishes between three types of difference: “anomalous monism,” where representations can be understood and combined partially but not systematically; “incommensurability,” where representations are incomparable and cannot be combined in any sensible way; and “contradiction,” where representations are logically or practically incompatible. He notes that each of these has a normative and an empirical sense: whether we should tolerate such conflicts or whether they simply exist in scientific research without judgement passed on whether they should be tolerated or excised. Accounting for those interpretations gives a six way classification of pluralisms.[19]

As with Ruphy and Ludwig’s classification, these categories are not strictly orthogonal either. Trying our hand at this system, we could say that Mitchell aligns with tolerant anomalous monism, Kuhn with empirical incommensurability, and that Cartwright with tolerance of contradictions. But this is misleading if we take it to be an exact classification of these philosophers. There is a normative dimension in Kuhn, since each paradigm carries standards of progress. And Cartwright distinguishes between fruitful misrepresentation and absurdity, which carries definite normative connotations, and suggests that some contradictions are apparent rather than real.

These classification systems do not sharply distinguish kinds of pluralism. Instead, they tell us which aspects of science a philosopher emphasises in their arguments, or perhaps which aspects seemed most salient to the reader who classifies them. This is still useful, since making the right or wrong emphasis in an argument (or an interpretation) greatly affects its strength. More importantly for our purposes, these porous classifications suggest that pluralising any one aspect of science will implicate other aspects. Moreover, they suggest that an analysis of science may at once be empirical and normative. Both of these observations strengthen the pluralist stance’s claim to distill scientific pluralism into one coherent framework.

[19]Dickson 2006, pp. 43-44.

1.1.2 Different and Conflicting Representations

The primary task of scientific pluralism is to find a middle way between monism and relativism. Ronald Giere writes that "[scientific pluralism] is opposed to two extreme views. The one extreme is a (monistic) metaphysical realism according to which there is in principle one true and complete theory of everything. The other extreme is a constructivist relativism according to which scientific claims about any reality beyond that of ordinary experience are merely social conventions."[20] Kitcher describes the middle way as "the possibility of functional conflict. As a form of technology generates new problems, compromises sometimes have to be made because solutions to one problem interfere with solutions to others. [...] Often there are alternative ways of compromising, and rival traditions, fully aware of the desirability of overcoming a number of problems, set their priorities differently. They may progress indefinitely, continuing to improve with respect to all the desiderata yet never completely converge."[21]

The question is just how to go about this. Adherents to the pluralist stance make a strong case that their modest and radical colleagues do not succeed in striking this middle ground. The former are really monists, the latter really relativists. Does the pluralist stance itself succeed in doing so? In the end, the answer will be no: it tends toward epistemic relativism. To see why, we must first look more closely at how pluralists try to set epistemic standards, first with the notion of conflicts and mere differences, and later with their epistemologies of science.

A conflict, for our purposes, is a difference which poses a problem and therefore cannot exist stably. A mere difference, by contrast, is a difference which does not pose a problem and therefore can exist stably.[22] Although pluralists agree that there needs to be a distinction, they do not agree on how to draw it. Dickson's classification of pluralisms helpfully explains why, since the range of options is wide even before we start hybridising them. Perhaps because of this, the distinction

[20] Giere 2006, 26.

[21] Kitcher 2011, p. 58. Note that this later Kitcher differs from the 1990s Kitcher, in large part because of his engagement with pluralists.

[22] "Mere" will sometimes be dropped in practice. Also, note that "stably" is not just an empirical description, but also signifies acceptability. This duality will become clearer when we look at Longino and Chang's epistemologies.

itself is often muddied: sometimes a philosopher will say "conflict" but mean it benignly as a mere difference, or will reference a category like "contradictions" without qualifying when they are acceptable or unacceptable. To sort through the ambiguity and understand the stakes of the distinction, we will look at two important philosophical analyses of it: Giere's article in *Scientific Pluralism*, and the relevant sections of Ruphy's monograph, *Scientific Pluralism Reconsidered*.

Giere argues for the pluralist distinction with an analogy between perception, observation, and theorising. People with normal trichromatic vision do not see more correctly than those with monochromatic vision (a form of colourblindness). There are two reasons to say this. First, we cannot just dismiss monochromatism as defective without pronouncing the same judgement on trichromatism. After all, there are animals with completely different visual systems, such as tetrachromatic birds, who see a wider range of light waves than any human. Second, it is not possible to infer what a monochromat sees by "subtracting" some of the colours from a trichromatic perspective. Why? Because "the experience of the monochromat is determined by two things: the intensity distribution of the light reaching the retina and the relative sensitivity of the monochromat's pigment as a function of wavelength. ... But the intensity of the light reaching the retina cannot be inferred from the response of the trichromat for the simple reason that *various* intensity distributions can produce the *same* response in a normal trichromatic visual system. The relationship between intensity distributions and chromatic response is many-one." Similar arguments apply to other comparisons, like red green colourblindness and trichromatic vision. The point is that they are all different perspectives, not reducible one to the other.[23]

Giere asks a crucial question about these perceptual perspectives. "Granting that [they] are *different*, are they *compatible*?" As possible evidence that they are incompatible, he notes that "a monochromat might claim to see a rug as being of uniform brightness while a trichromat sees a red pattern on a green background," the contrast of foreground and background implying differences in intensity. Yet concluding they are incompatible on that basis would be a mistake, since the relationship between intensity distributions and different visual systems is many

[23]Giere 2006, pp. 26-29.

to one. There will inevitably be cases where one system sees a pattern that the other cannot. Whether there is really a green background is unanswerable if we require that the answer is independent of any particular perspective. Does this mean that perceptual judgement is relative, or arbitrary? Only if agreement in judgement is not robust within a perspective, says Giere. When there is internal agreement in judgement, there is some measure of truth and falsehood. When a perspective is suitably stable, it can be analysed objectively even from outside. Once we learn enough about a perspective, even one we do not share directly, we can judge what would be true and false within it. As for relativism between robust perspectives, there is no issue, he says, since they occupy distinct and complementary domains. A monochromat and trichromat's observations do not actually conflict with each other.[24]

The application of these arguments to the other parts of his analogy, observation and theorising, may already suggest themselves in outline. Giere's examples are instructive. On observation, he looks at astronomy and the use of telescopes. Optical telescopes produce greyscale visual light images of their field of vision, while infrared telescopes encode invisible infrared light and assign it false colours within the visible spectrum. Which image best represents the way that the Milky Way looks? Just as no single form of colour vision is universal, no way of observing or measuring phenomena in science can be universal either. "There is no such thing ... as *the* way the Milky Way looks. There is only the way it looks to each instrument. ... There just is no universal instrument that could record every aspect of any natural object or process."[25] From instruments and points of view, we construct models, which also inhabit a perspective.

As with observation, so with theories, which differ from observation only in that the models they support are more abstract. For instance, Newton's laws provide a mechanical perspective of the world, and Maxwell's an electromagnetic perspective. Each set of equations is a template for constructing more concrete models of physical phenomena, but they focus on different qualities.[26] The point is that, as "instruments" of a sort, neither theory can claim to be a complete

[24]Giere 2006, p. 29.
[25]Giere 2006, p. 30
[26]Giere 2006, pp. 31-32.

perspective. While a monist would expect that a complete theory is lurking in the future, a perspectival pluralist has no such expectation. Nor does this mean that a perspectivist immediately commits themselves to relativism; as Cretu has argued, instruments and their uses can persist or grow across changes in perspective. The long run history of an instrument tends toward aperspectivity even though each individual use is within a perspective.[27]

Giere considers another example, the use of instruments to build models of the Milky Way. Ruphy later studies this example as well. Images of the Milky Way (the whole galaxy) are not produced by telescopes directly, because we are inside the Milky Way, so we have no access to a perspective outside it. Researchers use computer models to work around this limitation, building models by inferring structures such as bulges, clusters, spirals or arms from their piecemeal measurements. Giere poses the same question of these scientific perspectives which he posed for perception: given that they differ, do they conflict? The answer he wants to give is, again, no. "The perspectives of the various instruments used to measure radiation from parts of the universe are likewise both consistent and complementary. ... Thus, the plurality of perspectives found in scientific observation does not generate an undesirable relativism."[28] But Ruphy concludes that our images of the Milky Way are simulations, where the two most popular models are both extremely accurate (relative to the available data) yet "give us incompatible descriptions of what they take to be the main structural components of our galaxy."[29] Therefore they can hardly be consistent or complementary.

This conclusion poses a problem: how do philosophers make sense of such contradictions? Are they differences or conflicts? Ruphy first gives a typology of responses from scientific practice. One possibility is that the contradictions exist early in a research tradition, competing until one is resolved and the tradition matures. In this situation, scientists treat them as "*tentative* accounts of observed features of a system, rather than tools of representation that provide reliable insights into its underlying physics." But they can remain as alternative approaches over a longer span of time without showing any tendency to resolve. Here she gives

[27] Cretu 2022, pp. 528-529.
[28] Giere 2006, p. 31.
[29] Ruphy 2016, p. 104.

two examples, citing Longino on behavioural science, where "four approaches currently compete in the study of the causal mechanisms," and Margaret Morrison on nuclear physics, where contradictory models of the atom focus on different structural properties. "Adopting only one model (and rejecting the others) is not the implicit horizon of the work of the modelers" in this second type of response. Scientists instead use the protracted rivalry to motivate new research.[30]

The question, though, is how to respond philosophically, not as working scientists.[31] Ruphy sees three options: we can adopt perspectivism (like Giere), or we can tolerate contradictions as healthy competition "while awaiting good reasons to favor one over the others or to integrate them," or we can seek a deeper understanding on which they do not represent the same target and so do not conflict. Here, in effect, she develops another classification of scientific pluralisms, along the same lines as Dickson.

She then tests each option against her case study on stellar models. Giere's perspectivism fails the test, because if we take our models as perspectives on the Milky Way, then they directly conflict and cannot reasonably coexist. The second option, tolerating them as healthy competition until there is a victor, amounts to a patient form of monism like Kitcher's, since they are only tolerated insofar as there is no clear way to access the true picture. Ruphy therefore opts for the third. Our stellar simulations, she argues, do not represent the Milky Way as a map or a photograph would by offering a perspective on what it looks like. They represent a hypothetical galaxy which resembles the Milky Way in certain respects. The reason we study this hypothetical galaxy is that it is easier than studying the one we inhabit. There is no need, on her view, to anchor plurality in metaphysical assumptions about the complexity of the world, on a theory of parsing causes, when we can anchor it in methodological assumptions about the

[30] Ruphy 2016, pp. 85-88.

[31] This difference, respected by most of the pluralists we will encounter, has been codified by Martin Kusch as the distinction between engaged and detached perspectives. The engaged perspective is the scientist's, acting within the institutions of science, while the detached is the philosopher's, who is more of a visitor. See Kusch 2017.

best way to expand our knowledge.[32]

On the former view, many correct representations of the galaxy would of course conflict at face value, because the galaxy is enormously complicated. The problem for Ruphy is that the conflicts have to go deeper than face value, because they imply contradictory descriptions of the actual structure of the galaxy. There may be an arm in one model where there is none in the other. Taken as descriptions of the world, then, they are not mere differences, they are true conflicts. Only if the models are simulations, lacking any descriptions of the real galaxy, can we then construe them as merely different.[33]

So far, we have accomplished two things. First, we examined classifications of scientific pluralism and located the pluralist stance within them. These classifications distinguish emphases rather than completely different views, but they help explicate the core commitments of pluralism. Second, we have seen that pluralism relies on a distinction between mere differences and conflicts. This distinction is expressed in many ways, but it is key to distinguishing pluralism from monism and relativism. But since the distinction relies on our interpretation of contradictions, types of propositions, and their justification, it cannot be clear without embedding it in an epistemology of science.

1.2 Pluralist Epistemology and Relativism

Distinguishing between mere differences and conflicts is in itself not pathbreaking. Every philosophy of science does this insofar as it evaluates or describes scientific debate, unless the philosophy in question denies the existence of one or the other category. Scientific pluralists need to justify a version of the distinction which classifies many things as differences which a monist philosophy would classify as conflicts, and as conflicts many things which a relativist philosophy would classify as differences. A pluralist epistemology needs to explain how substantial differences in representations can exist without posing theoretical or empirical problems. It

[32]Ruphy 2016, pp. 88-92. Incidentally, this is another example where the classifications of pluralism blur their boundaries. Both the editors and Ruphy emphasise epistemic aspects of science, but the editors justify it ontologically while Ruphy justifies it methodologically.

[33]Ruphy 2016, pp. 106-109.

also needs to set definite constraints, beyond which representations conflict and pose genuine problems.

Within the pluralist stance and its immediate orbit, two philosophers have developed an epistemology of science with this specific end in mind: Helen Longino and Hasok Chang. In each case, their strategy is to show how scientific knowledge exists in specific locales with different values and different representations, yet at the same time there is no blanket epistemic requirement that these locales conflict.

But, as it happens, their epistemologies slide them closer to epistemic relativism rather than a middle way. Longino argues that knowledge comes from epistemic communities, whose interests, aims and values partly constitute a local epistemology. They do so only partly, since Longino also introduces a broadened notion of truth which she calls conformation. She also argues for a set of standards which measure the health of an epistemic community, standards by which any community can judge another or itself. But because each epistemic community may have drastically different values, interests, aims and methods, the interpretation of these standards is too underdetermined to be objective. Instead, each community has its own standards by which it judges the health of an epistemic community. Everything is judged at one level, relative to the community, so that there is no higher level where communities are judged.

Chang argues that success is prior to truth. To judge whether some scientific approach is true, we first have to ask whether the practical operations which constitute it have been successful, where success is measured by the interests and aims of the scientists conducting the practice. Only after determining that a practice is successful can we then talk about what kinds of truth it gives us. Like Longino, Chang argues that all practices have an interest in accomplishing some benefits of toleration and interaction with each other, but as with Longino, the interpretation of these benefits (both what they are and when they are accomplished) is open, and happens at the level of each practice.

1.2.1 Longino's Conformation and Local Epistemologies

Before looking at Longino's epistemology itself, we should first see how she lays the groundwork for a middle way. This informs her analysis of differences and conflicts later on. Monism and relativism have dominated the epistemology of science, argues Longino, because science studies have been shaped by a dichotomy between the rational and the social. This dichotomy shapes their views about epistemology, and in so doing makes pluralism seem impossible. The epistemology of science works on three dimensions: who (or what) the knower is, which practices they are engaged in, and what warrants (or justifies) calling some knowledge successful. Adherents of the dichotomy, whom she calls dichotomisers, believe that their view about each dimension mutually imply and reinforce each other. This is the key premise supporting the dichotomy. Longino urges us to reject this premise and the dichotomy to make room for a pluralist epistemology.

Rationalisers believe that scientific knowledge is held by individuals, that their beliefs are knowledge only if they match a uniquely correct account, and that success is judged by a standard of epistemic warrant independent of the social trappings of science. Longino labels these respectively as individualism, monism and nonrelativism. Sociologisers believe that knowledge is not held by individuals (but by groups, possibly including machines and instruments), that accounts do not have to be uniquely correct, and that success is internal to social and institutional standards. Longino's labels for these are nonindividualism, nonmonism, and relativism.

Dichotomisers are right to think that their views are mutually exclusive, but wrong to think that their views are jointly exhaustive. "[They] seem to think that ... nonrelativism implies ... monism with regard to content and individualism with regard to agency, while ... nonindividualism implies ... nonmonism with regard to content and relativism with regard to warrant." Kitcher, for instance, uses an argument against relativism to refute nonindividualism, and in so doing he relies on this dichotomy.[34]

[34]Longino 2002, p. 90, citing Kitcher 1993.

	The Dichotomizers' Way		The Nondichotomizer's Way
	Rationalizers	**Sociologizers**	
The Knower	Individualism	Nonindividualism	Nonindividualism
Content	Monism	Nonmonism	Nonmonism
Warrant	Nonrelativism	Relativism	Nonrelativism

The dichotomy seems tempting because of ambiguities latent in the negative options (nonindividualism, nonmonism and nonrelativism). Rationalisers assume their positive account of warrant is precisely what nonrelativism means; likewise, sociologisers think that their positive account of content is what nonmonism means. To overcome the dichotomy, Longino demonstrates that there are multiple positive options latent in each negative category. A rationaliser's nonrelativism is absolutism, the view that there are fixed external standards for warranting scientific knowledge. Longino opts for contextualism instead: standards of warrant depend on local features of our epistemic communities. Likewise, instead of simply being a nonmonist, a sociologiser is an antirealist, since they believe that scientific approaches are social constructions or conventions. Longino opts for plural realism, the view that different approaches can simultaneously give us true knowledge, even if that knowledge is partial. Finally, the sociologiser's nonindividualism is wholist, so that the group or the entire system of science knows rather than any individual. But Longino labels her view as socialism, "an interdependence interpretation of nonindividualism" where social institutions and communities are necessary conditions for individual knowledge.[35]

This conceptual analysis is, again, the background Longino needs to show that a pluralist epistemology is possible. Having shown that it is possible, she needs

[35] Longino 2002, pp. 91-93. See the table for a summary of these categories, which is adapted from two smaller tables in this section of Longino's book.

And for completeness, here is the list of disambiguations for each negative view. Nonindividualism can be wholist: groups, not individuals, know; eliminativist: there is no knowing subject at all; or socialist: knowledge is held by individuals, but only because of social relations and interdependencies. Nonmonism can be antirealist: many accounts are compatible with our experience and none of them refer to anything real; eliminativist: there is no correct account at all; or plural realist: theories and models can only represent partially, but they can in fact represent. Nonrelativism can be absolutist: justification is entirely independent of context; eliminativist: justification is impossible or unnecessary; or contextualist: justification is objective, but its norms and very possibility depend on local context and social institutions.

to elaborate upon it and argue for it. Longino has simply shown that a pluralist option exists. Let us first consider her argument for contextualist warrant. Warrant here means justification, but without the connotation that knowledge is propositional. Contextualism blends normative features of the rationaliser's view with empirical features of the sociologiser's. Sociologisers try to keep a strictly empirical notion of warrant; standards of warrant have no necessary force, and are to be explained sociologically rather than being justified with an argument. An epistemic practice, on this view, produces knowledge when it fixes belief according to such internal standards. For rationalisers, it is not enough that a community has fixed a belief according to its standards, because to claim the belief as knowledge requires that they justify it and that it be true. The empirical fact of producing and offering so-called knowledge is barely relevant; what we need is an external standard.[36] Longino's contextualism the very practices which govern when a community accepts beliefs also warrant those beliefs, so that "that the success picked out by the normative concept [of knowledge] be explicable for the empirical social concept."[37]

Contextualism about warrant means that scientists rely on features of their social context to gain knowledge. Likewise, socialism about the knower means that individual scientists rely on a community and institutions to learn new things. To discern the import of a measurement or visual representation, scientists raise provisional interpretations, and they assess an interpretation's promise through discussion and further experiment.

To illustrate this, Longino discusses examples from the sociology of science on galactic imaging and genetic analysis.[38] "Not only is the reading of a film [that is, a microscopic image] social ... but the final representations, the ones used in publication, are constructed by synthesizing and abstracting the common features of a number of films thus interpreted." Our understanding of a representation is iterative, moving from production to hypothesis and over again until there is enough clarity. Viable observations "have a stability that allows their transfer

[36]Longino 2002, pp. 78-85.
[37]Longino 2002, p. 97.
[38]Her references are Annis 1978, Cohen 1987, Lynch and Edgerton 1988 and Amann and Knorr-Cetina 1990.

from one laboratory to another, or comparability between different field sites," or else it is too ephemeral for systematic study. Observations are not justified, do not count as scientific knowledge, unless they are subject to social processes. Among those is reasoning, both the creative "[combination] of ideas or information to produce new ideas, [or] ... the other, justificatory sense, ... to support some other idea." Warranting knowledge is social, even to oneself, since it is "located in the interactive context of challenge and response," whose norms are social. The individual scientist and scientific communities are interdependent.[39]

So far, we have discussed Longino's views about knowledge and warrant, and her critique of the rational-social dichotomy. What we still have left to do is discuss with her views about truth and its relationship to epistemic communities and their values. This is key to understanding how Longino tries to avoid epistemic relativism.

Rather than using the word "truth," she opts for "conformation." She reserves "truth" for propositions whose content corresponds to the world, which is only one kind of conformation. Conformation is a disjunctive notion, having several senses which apply to different cases. A representation conforms to its object either when it is isomorphic or homomorphic to its object, or it is true of it, or approximates or fits it, or bears some similarity to it. Conformation is flexible. Laws which are false when interpreted as propositions can still conform by fit or homomorphism. Statistical claims which correspond to no particular fact can still approximate something.[40]

Longino's view on knowledge tried to marry empirical and social features, so that the normative criterion of success is derived from social standards. Conformation also behaves this way. Decisions about structural fit, approximation, and son on are socially negotiated. They do not mean anything concrete unless placed in the social context of research. But to say that a representation conforms is not just to report what scientists say. Granted, sometimes the object may be socially determined (like population), but conformation is not just a matter of community

[39] Longino 2002, pp. 99-103 on observations, pp. 103-107 on reasoning, and pp. 107-108 on interdependence of agents.
[40] Longino 2002, pp. 115-116.

acceptance. It has normative content as well, namely that this representation is trustworthy as a guide for action. "The sociologist treats knowledge as what is accepted in a community as knowledge. The sign of such acceptance is use. Theories and models are not treated as knowledge ... unless they conform sufficiently to enable their users to interact successfully with the domain."[41]

Epistemic communities are where conformation gets its significance. An epistemic community is a collection of scientists who research together, and who share some values, aims and interests. These constitute the community's epistemic standards. Communities assess and accept scientific claims according to those standards; practices produce conforming representations only because of how the community's values shape its notion of conformation; and someone knows something only when their claims survive the scrutiny of their community. This matrix of relationships between the scientists and their values, aims and interests is a local epistemology.[42]

There are many different communities with different local epistemologies. However, not all of these communities are equal. Some, by closing off avenues of criticism and sidelining too many perspectives, are afflicted with dogmatism. Longino's idea is that a dogmatic community's practices cannot reliably produce conforming representations, so we cannot count what they accept as knowledge. It would seem to follow that anything such communities produce which differs from the products of a healthy community would be in conflict. But it is important to remember that this conflict arises not from the differences themselves, since healthy communities can differ from each other even while they both produce conforming representations. Epistemic conflict comes from the parts of the local epistemology which produced the differences. No other representations need to induce cross-community conflict, as far as the epistemology of science is concerned. This is Longino's strategy to develop a normative epistemology of science which is sensitive to the empirical and social aspects of research. Her epistemology of science "requires pluralism for the conduct of inquiry,"[43] where "the *kind* of knowledge

[41] Longino 2002, pp. 119-120.
[42] Longino 2002, pp. 136-138.
[43] Longino 2002, pp. 140-141.

a community seeks, the *purposes* for which it seeks it—that is, the uses to which knowledge will be put—guide the development of the community's standards."[44]

However, by extending her line of thought further, we can see that Longino's view has not escaped epistemic relativism. Each community has its own local epistemology, shaped by the values, aims and interests of the community. The local epistemology completes the meaning of conformation, which on its own, divorced from a community, means little to nothing. It is only within a local epistemology that we can judge which kind of conformation should apply to a given situation, such as whether galactic models are images which resemble a real target or simulations which give fit solutions to equations and correspond to measurements. Likewise, it is only within a local epistemology that we know what it means for a model to fit its target, for an approximation to be within an acceptable error bound, and so on. These notions will depend on the community's values, interests and aims. Every so far is an intended feature of Longino's view. "A monist or unificationist philosophy requires that all these examples of difference or dissension be ultimately resolved in favor of one account, or that models ... be restricted in their scope to eliminate conflict."[45]

What happens when two communities have completely different local epistemologies? Insofar as their values, interests and aims are incommensurable, so are their standards of conformation. Two such communities cannot fruitfully criticise each other, so long as they do not adopt or construct a shared standard. Criticism requires friction between different ideas which motivates objections, replies to those objections, and so on. Friction can have both a logical and a psychological sense. In its logical sense, it is a lack of accord between two representations because they offer contrary descriptions of the same target. In its psychological sense, it is a feeling that something is wrong or incomplete.[46] Yet pluralism is

[44]Longino 2002, p. 188.

[45]Longino 2002, p. 197. Remember that "conflict," "inconsistent," and "incompatible" have varying usage among pluralists. When Longino says "eliminate conflict," she means eliminating difference as well as dissension.

[46]This use of "friction" is inspired by Price 2003 and Atkin 2015. Price argues, against Rorty, that justification cannot be all there is to epistemology, because then there is nothing to motivate disagreement between justifications of differing views, not matter how different. Atkin discusses the occurrence of this idea in Peirce's thought. Peirce will become relevant to us later, when we come to discuss methodological monism.

partly about downplaying the conflict between different representations.

In cases without aligned interests or shared standards of some kind, there is no friction between different ideas, so there can be no productive disagreement either. Likewise, there are no grounds to criticise another community's standards of conformation unless we already have some shared values, interests or aims. Sometimes, as in the evolutionary synthesis, different communities become entangled in debates which eventually draw them together. In such cases, scientists construct new standards which encompass or reject parts of the old ones. But to motivate such debates, scientists must believe that their differences pose some empirical or conceptual problems, that their differences are epistemically problematic. Longino's account of knowledge allows that healthy communities might engage in debate with one another, and that once they engage, they must be open to one another's voices, but it is equally fine if they remain indefinitely tolerant.

Ultimately, Longino's pluralist epistemology does not escape relativism. The values, aims and interests of Longino's epistemic communities shape the norms which govern of their research, including what it means for a representation to conform. First of all, a community's aims and interests determine which of the several kinds of conformation are relevant to them. Even after having made that choice, the aims and interests further determine, say, how approximate a model fit must be, or which structures should be preserved by a homomorphic representation, or which kinds of similarity and dissimilarity are acceptable between representation and object, and so on. Fixing these also fixes the standards of warrant, of justification. Thus judging whether a community is healthy and whether their research is successful depends on the community's values and interests.

1.2.2 Chang's Truths Internal to Systems of Practice

The second proposal for a pluralist epistemology of science comes from Hasok Chang. Longino's view emerged by dismantling the rational-social dichotomy. Chang's comes from an operational analysis of scientific practice, and the relationship between truth and successful practice. Pluralists, as we have already seen, take it for granted that much of science is successful. But science in actual practice is messy, so pluralists must show that there is order to the mess, some constraints that keep research in check. A theme of Chang's work is that epistemic constraints alone are not enough. Success is a brute fact about a practice, which depends only on its structure (as an action) and the aims of the researchers who perform it.

Chang's focus on action goes beyond the granular level. It motivates his entire philosophy, which he calls active pluralism. "Science should strive to maximize our contact with reality in order to learn as much as we can, ... [so] we should pursue *all* systems of knowledge that can provide us an informative contact with reality."[47] This has led to arguments with Longino and Waters about whether Chang is part of the pluralist stance. Chang's argument is that the pluralist stance is inherently passive, that it is about tolerating difference rather than maximising it. However, the thrust of this argument is that the pluralist stance is not faithful enough to its third core commitment: that the pluralist structure of science is constitutive of its success. Chang otherwise shares with them a commitment to empiricism, evidenced by the original historical research in his work. He also shares the view that philosophy cannot decide in advance what the structure of successful science will look like. And his epistemology of science is motivated to find a middle way between monism and relativism. Thus we can say that Chang is part of the pluralist stance.

The key concept in Chang's epistemology of science is the system of practice. Systems of practice are composed of epistemic activities, which are composed of operations. Let us look at these in reverse order. An operation could be computing an integral, copying a measurement, writing a report, or calibrating an instrument. They are actions scientists perform while working. Chang sets no limits on how long such an action should take, or how extended in space it is, or how many people are involved, in a deliberate effort to avoid metaphysical debates about the nature of action. Any action which scientists take while researching is an operation.

An epistemic activity is "a more-or-less coherent set of mental or physical operations that are intended to contribute to the production or improvement of knowledge in a particular way, in accordance with some discernible rules." He gives many examples: "Describing, predicting, explaining, hypothesizing, testing,

[47] Chang 2012, pp. 290-292. Alison Wylie has developed a similar view, which she calls dynamic pluralism in Wylie 2015, p. 195ff.

observing, detecting, measuring, classifying, representing, modeling, simulating, synthesizing, analyzing, abstracting, idealizing."[48] Many of these examples could also be considered operations within a larger activity. This is by design; Chang wants to avoid a reductive framework where we partition a domain into strict parts and wholes with their own unique levels of description.

A system of practice is "a coherent set of epistemic activities performed with a view to achieve certain aims." They can exist on a large scale: Molecular biology is a system of practice, and so is quantum mechanics (understood not just as a theory). They can also exist on a smaller scale: Mass spectrometry and archaeological digs are systems of practice. "The aims of a system of practice define what it means for a system to be coherent."[49] Coherence is therefore judged by internal, not external, standards. For example, we can judge that an archaeological dig is incoherent if they chose tools that are so crude that they would break the artefacts they are trying to uncover. The incoherence in that case comes from a conflict between two incompatible parts of the system: the operation of using the unsuitable tools, and the epistemic activity of observing the artefacts. If the parts of a system are compatible, so that they are mutually reinforcing rather than undermining, then the system is coherent. If it coherent, then it is at least possible for it to be successful.

Truth does not set any global constraints on systems of practice. In Chang's words, "if we are talking about 'Truth with a capital T' that is not at all dependent on the system of practice one works in, it is doubtful that there are any actual scientific activities that are concerned with it. The success of each activity or system needs to be judged first of all in terms of how well it achieves the aims that it sets for itself; in addition, we may make judgements on the value of the aims themselves."[50] Notice the analogy between systems of practice and epistemic activities. The activities which constitute a system are part of its aims, because the point of a system of practice (at least in part) is to perform those activities. Likewise, part of the point of an activity is to performs its operations. These aims,

[48] Chang 2012, pp. 15-16.
[49] Chang 2012, pp. 16-17.
[50] Chang 2012, p. 18.

together with the intent to improve our knowledge, are internal to systems and activities.

A monist might suppose that true systems will be successful, but Chang flips this on its head. "Truth as I conceive it means correctness as judged within a specific system of practice, and a decision to adopt a system of practice is determined by its successfulness." On the one hand this emphasises that truth is relative to systems of practice. On the other hand, against the relativists, not just any system of practice will be truthful, because not every system will successfully achieve its aims. We first learn which systems of practice are successful, then we see what is true. Systems of practice are not themselves true or false, because they are activities rather than beliefs. Since knowledge is built from operations into epistemic activities into systems of practice, "knowledge is not a matter of truth, strange as it may sound."[51] Knowledge also admits of degrees and approximation, where truth does not. Recall that Longino had similar concerns and opted to expand the notion of truth to conformation. For both Longino and Chang, scientific representations can achieve the status of knowledge in several different ways. Recall that, for Longino, scientific laws can be false but have a good approximate fit; images or models might not bear truth values at all but instead be homomorphic to a target. And for Chang, all propositional belief is secondary to activities, and any successful activity will in some sense be true, even if not in the narrower, propositional sense of truth.

More precisely, truth follows from success because when a system of practice is successful, its internal standards of truth are reliable. To argue for this, Chang distinguishes five senses of the word "truth," later showing how they relate to each other. First, it can mean honesty, that what we have said is earnestly linked to what we think. Second, something can be true by convention, because we "construct, judge and maintain [it] by making, using, and enforcing definitions." Third, there are presuppositions, such as axioms, postulates, or regulative principles (as an example, Einstein's postulate that the speed of light was constant everywhere). Fourth, a proposition can be true because we have deduced it validly from true premises. And fifth, something can be true "if it passes, *contingently*, the tests of

[51] Chang 2012, pp. 213-217.

correctness operative within that system. Is it true that the atomic weight of chlorine is roughly 35.5? Within a specific system of atomic chemistry that we operate in, we have specific procedures for assessing atomic weights."[52] The fifth sense of truth is "scientific," and it subordinates three of the other sense. For example, atomic weight could be true by definition (true in the second sense), but definitions are contingent on tests of correctness: their actual use in scientific practice has to help advance the aims of the system of practice. So within a system of practice, truth in the second sense depends on truth in the fifth sense. The same holds for deductions and presuppositions, the third and fourth senses. Anything we deduce from the speed of light in general relativity could be abandoned if the presupposition (the speed of light given by the theory) fails a future test of correctness. Truth is plural in two senses, then: There are distinct kinds of statement that can be true in different ways, and scientific truth is relative to systems of practice.

How do we choose between systems of practice? When Chang says we can judge the value of aims, are these judgements made from the perspective of another system, or some external standard? Chang does not give a clear answer. The only criterion is success at realising our "epistemic values." Whereas coherence was an internal standard related to achieving aims, success does not have a clear status. Some epistemic values are the same as the aims of a system of practice, but Chang implies there could also be external values.[53] "It is futile," he claims, "to define 'success' in any one-dimensional way. ... The 'success of science' can only really mean the achievement of whatever we value in science ... accuracy, consistency, simplicity/elegancy, scope/completeness, unifying power, explanatory power, fruitfulness, testability, and even conservativeness. No single one of these is a value that overrides every other."[54] To complicate matters further, there is an important caveat. Chang speaks of choosing between systems, but he does not believe an exclusive choice is always necessary. He is a pluralist, after all. Sometimes choosing systems means cultivating rivals or alternatives, due to the "benefits of toleration" and "benefits of interaction" that they can produce. For example, tolerating multiple systems can cover more phenomena than one system alone, or

[52] Chang 2012, pp. 241-242.
[53] Chang 2012, pp. 207-208
[54] Chang 2012, p. 230.

satisfy more aims. And they can lead to a later integration of knowledge, or even without integrating, they can increase the value of research by competition.[55]

There is a fundamental problem, or open question depending on the perspective, with putting success before truth. Whatever epistemic values are and wherever they exist, it is clear there can be an array of them, pursued for many reasons. Even when the same values are in play between two groups, they can weight and rank those values differently, so that the success of a system of practice is relative to our values and our ranking of those values. How do we choose between epistemic values? That is, how do we choose what to count as knowledge?

Chang answers that the question commits a category mistake. Neither monism nor pluralism address how to choose between theories, systems of practices, epistemic values, or whatever else. They answer a different question about how much difference we can tolerate between systems, theories, or values. A monist says all theories or systems or values must be compatible with one another and intertranslatable. Meanwhile a pluralist makes no such requirement. A monist and pluralist could agree on the method of evaluation and selection, with the sole difference between them being how many "winners" they are willing to admit. "If you want to be a pluralist, do whatever you would do in choosing a winner in the monist scheme, and just pick *two* winners at the end, or put in a second prize." Whether we cut off admission at one or many, "neither monism nor pluralism delivers us from the responsibility of judgment." It is fine to distinguish between choosing the number of "seats at the table" and choosing "the guest list," as he puts it. But in his own account, those questions are related.[56]

This answer is unsatisfying. Pluralism may not answer how to choose between systems, but it does raise the question. Chang has already argued that truth, in science at least, is internal to systems of practice. He also argued that we should allow many systems to flourish. So he is both a pluralist about truth and, in some sense, a relativist about truth. The question is whether his relativism is vicious; whether, to allude to Longino again, Chang's view of truth renders warrant

[55] Chang 2012, pp. 270-284.
[56] Chang 2012, p. 261.

arbitrary. And this depends on the role that values play in deciding which systems we adhere to. It is not at all a question that Chang can avoid answering.

Sometimes his view seems viciously relativist. At one point he anticipates an objection, that multiple systems representing the same domain differently will paralyse our decisions because they will give conflicting advice. But in that case, "those who need to use the knowledge can and will make the choice. Plurality in science provides opportunities rather than hindrance, as long as those who apply science are willing to make their own judgements." Science consists of systems of practice that enable new opportunities, new choices. Systems are not true or false, since truth is internal to systems. And we decide between systems based on the epistemic values we adhere to. If someone ranks the conservation of old knowledge over testability, they might make different medical choices. "One may go to the hospital or the acupuncturist for a sprained ankle; it is not uncommon for terminally ill patients to turn to traditional remedies; there is nothing incoherent about these decisions."[57] The use of "incoherent" is striking, since earlier Chang had described the limits of coherence for a system of practice. On some prior weighting of epistemic values and nonepistemic interests, acupuncture may not be an incoherent system of practice. At the very least, we can settle the question by examining the system. But there is no similar mode of objective evaluation for those values and interests themselves.

It is possible to object that there is such a mode in the benefits of toleration and interaction, since these benefits do not just help justify a particular system of practice, but also give structure to the interaction between such systems. They therefore stand outside concerns about whether one system is compatible with another on some ranking of epistemic values. The problem with this objection is that the benefits still appeal to epistemic values in some way, both in their interpretation (what it means to confer such a benefit, what the threshold is) and their application (which benefits actually exist in a specific situation). If multiple systems coexist, then we can satisfy different aims at the same time, something Chang calls a "pluri-axial regime" of science. Under such a regime, different systems contribute toward different epistemic goals. The list of goals is familiar:

[57] Chang 2012, pp. 264-266.

"problem-solving ability, ... accuracy, consistency, fruitfulness, and scope; ... elegance, simplicity completeness, unifying power and explanatory power, and ... empirical adequacy."[58] Systems of practice which represent the same targets differently cannot conflict directly unless, at a minimum, they are trying to contribute to the same epistemic goal, so that they become part of a larger system shaped by that shared aim.

1.3 The Pluralist Stance as Epistemic Relativism

Scientific pluralism comes in many varieties, though its adherents generally share an aim: overcoming the faults of monism and relativism. Modest pluralists do this by expanding the kinds and duration of conflicts in the sciences. Where someone like Karl Popper would insist that a theory must be discarded immediately upon encountering a contradictory observation, modest pluralists are more flexible about how scientific approaches are whittled down. Radical pluralists, on the other hand, adopt relativist views in metaphysics or epistemology and move beyond the older epistemological views. The pluralist stance makes a plausible case that neither modest nor radical pluralism truly overcomes the faults of monism and relativism. Neither is a true middle way. However, it turns out that the pluralist stance has its own struggles.

The pluralist stance wishes to set constraints on scientific research which are strong enough to avoid relativism but lenient enough to make the success of science realistic. To achieve the former, they need reason to say that some differences in representation amount to conflicts. To achieve the latter, some differences must be allowable beyond what a monist view would licence. This is what Longino and Chang's epistemologies set out to achieve. These two wishes are in tension, pulling in opposite directions. Only a clear distinction between differences and conflicts can resolve the tension without pulling the entire view apart. We have not been given such a distinction.

[58]Chang 2012, pp. 273-274. He draws the first set from Kuhn 1962, the second from van Fraassen 1980.

The pluralist stance easily distances itself from monism. No monist would allow multiple conflicting representations to coexist without the expectation that they will resolve. Even a monist who agrees that local context matters, that many direct contradictions are illusory upon analysis of their contexts, would expect that the two contexts can somehow be combined, which the pluralist stance denies. Relativism has posed more of a challenge for Longino and Chang. This is because the relationship between values and epistemic norms is circular in a subtle way. Although both Longino and Chang try to develop external epistemic standards (that is, external to particular communities and systems), it is impossible to interpret and apply them without appeal to internal standards. Insofar as we have an external standard at all, it is a template which admits of indefinitely many completions.

This challenge comes from a tension between two core beliefs in the pluralist stance: that an epistemology of science should make no assumptions about the ultimate outcome of scientific research, and that avoiding such assumptions means adopting the pluralist stance. In her discussion of epistemic communities and conformation, Longino argues that "finding a uniquely correct account would require identifying the uniquely correct starting point in each of these reservoirs of plurality." If two different representations arose from parsing causes differently, for example, then a monist would need to be able to say which way of parsing was correct, before they could say which representation is uniquely correct. "While contradictions within an approach require resolution, ... there's no a priori requirement that either different approaches to studying the same general area yield compatible observation statements or measurements[,] or only one of those different approaches can be correct."[59] But this follows only if we accept that such requirements would have to be a priori. Why not a regulative or pragmatic requirement, for example? Without ruling out these options, it is unclear why scientists cannot imagine a provisional end point where scientific consensus grows to include all.

Chang might attempt to overcome this with his active pluralism, and his maxim

[59]Longino 2002, pp. 200-201.

to "let a hundred flowers bloom."[60] But the point is to avoid a bias toward monism by ensuring that there are always many parallel systems. One of the lessons Chang wishes to draw from the chemical revolution was that, without active encouragement, the sciences move from partisanship and exclusion to become monopolies. This biases the outcome of the sciences by sabotage and force rather than evidence and epistemic success. But again it is unclear why a monist regulative ideal would have to be built on sabotage and partisanship, even if in some historical examples there is a case for it. Not only is it unclear why monism cannot have a rational basis of this kind, but ruling it out has made it difficult to avoid relativism. Since pluralists need to allow different approaches to remain apart indefinitely, or even to diverge further over time, their epistemologies have been unable to avoid relativist consequences.

So far, we have become familiar with scientific pluralism and the pluralist stance. We have seen that the pluralist stance's core commitments are a plausible heuristic to guide what counts as pluralism, so that wide deviations from them amount to a form of monism or relativism. However, we have also seen that the pluralist stance struggles to answer a key question without itself lapsing into relativism: when do representations merely differ, and when do they conflict? Neither epistemology of science advanced by this philosophical tradition has managed to draw the distinction without making scientific knowledge, and the standards for judging such knowledge, relative to a community or system. This is a theoretical problem for scientific pluralism. But it is also a problem for anyone interested in the history and philosophy of science. Scientific pluralism has many valuable lessons to teach us about the sciences, including the importance of its social context. As we move on to look at the pluralist stance's empirical difficulties, we should bear in mind the question of how to address these criticisms without losing those lessons.

[60] Chang 2012, p. xx.

Chapter 2

Conflicts in the Evolutionary Synthesis

The pluralist stance's theoretical problems with relativism are compounded by an empirical problem. To justify their position, pluralists cite or create analyses of historical episodes, including normative assessments of those episodes. Pluralists have argued that the structure of scientific practice is pluralistic even if the scientists did not realise it in their individual actions. Because scientists rarely speak with perfect tolerance about other approaches to their discipline, pluralists have also argued that the scientists should have tolerated these other approaches, and that their science, though successful, would have been more so if they had been more tolerant. Chang has argued as much in his case study of the chemical revolution; Waters with genetic science; Longino with the behavioural sciences and evolution; and Ruphy with cosmology and nuclear science.[1] In their interpretations of these historical episodes, pluralists respect the distinction between engaged and detached perspectives. The engaged perspective is that of a working scientist immersed in the day to day routine of research, while the detached perspective is that of an outsider trying to understand it.[2]

[1]The relevant works are Chang 2012. Waters 2006, Longino 2002 and Ruphy 2016. The arguments span much of these books and articles, but we will also refer to specific parts of some of them later when we contrast the pluralist stance with methodological monism.

[2]This distinction between engaged and detached perspectives follows Kusch 2017, who adapted it to epistemology from its use in ethics by Wong 2006.

The distinction is an important one, and there will be no argument against it here. However, pluralists are too hasty when they dismiss scientists' intolerance, whether they say it is structurally unimportant or an impediment to scientific progress. Longino and Chang's epistemologies lapsed into relativism because they could not motivate the friction between different representations. In their epistemologies, the more two representations of the same target differ, the less reason there is to interpret them in conflict. But in normal scientific practice, the reverse is true: two very different representations of the same target will raise a lot of questions and debate. There is no shortage of such friction in scientific practice, even when representations of the same target come from different interests and contexts.

Pluralists try to interpret this feature of scientific practice so that it is not structurally important. The reason why is simple: if it were structurally important, then scientific practice would be monist. But they have strong theoretical and empirical arguments against scientific monism, understood along the lines of the definition we discussed earlier. It would be a serious problem to hold that scientific practice is monist and that scientific monism has serious theoretical and empirical problems. We will tackle that problem when we discuss methodological monism. The task now is just to argue that scientific practice is monist, insofar as different representations tend to have friction between them; that is, insofar as scientists interpret differences as conflicts until there is a positive reason to believe that they are compatible. Scientific practice is procedurally monist. If this is correct, then to insist on pluralism in the face of these facts is to impose an external ideal on scientific practices which do not conform to them. In this respect, pluralism would be no different than the monism it criticises.

Arguing this case requires an extended scientific example. We will look at the evolutionary synthesis and the historical development of the biological sciences in the decades prior to it. This span of time stretches from the 1859 publication of Darwin's *Origin of Species*, through methodological and theoretical debates between naturalists and experimentalists in the late nineteenth century, to the rediscovery and acceptance of Mendelism and its conflicts with Darwinism, to the evolutionary synthesis itself in the early to mid twentieth century. We will spend most of our time on two episodes during the evolutionary synthesis itself, centred on Sewall Wright and Theodosius Dobzhansky, two key figures in the development of modern evolutionary theory. This theory, developed in the early to mid twentieth century, marries genetics and evolutionary science together, where before they had been taken to conflict. The reasons for this conflict go back to the debates between naturalists and experimentalists which form the background to our central episodes.

As for the episodes themselves, the first is a debate between Wright and R. A. Fisher. These geneticists both developed statistical methods to describe the genetics of entire populations, but they differed on which forces were operative in adaptive evolution. Wright's shifting balance theory included many different causal factors, most controversially genetic drift, and tried to account for when these factors were most active. Fisher's selectionist theory was built on the conviction that natural selection was the only adaptive force in evolution. Their theories of evolution were based on different sets of evidence and made different assumptions about the nature of populations. But they still took their theories to be comparable, so that they could compare theoretical prediction directly and discuss the relevance of experiments.

The second episode is the publication of Dobzhansky's monograph, *Genetics and the Origin of Species*, in 1937. In it, he argued that the new population genetics allowed Mendelian genetics and Darwinian evolution to be brought together. Dobzhansky worked as a field and laboratory scientist in Russia and, later, in America under T. H. Morgan (one of the early discoverers of the chromosome). In the 1930s, Dobzhansky paused his experimental work to write his monograph, because he became convinced that the supposed conflicts between evolution and genetics could be resolved.

2.1 Historical Background: Naturalism versus Experimentalism

Before going into the case studies of Wright and Dobzhansky, we need to put them in their historical context. Biology in the modern era has given central importance to the problem of heredity. How do species pass on their traits over generations? How do new species arise? Historians have classified the methods scientists used to answer these questions, during the nineteenth and early twentieth centuries, into two broad categories (though of course each had different theories advanced under it): naturalist and experimentalist.[3] Naturalist methods proceeded by observing many organisms and fossils in their natural environments, while experimentalist methods proceeded by controlled tests meant to establish underlying mechanisms. In many respects, the scientists following each method were doing something different: offering different kinds of explanation for different phenomena, and so on. But they shared certain key questions in common, including the problem of heredity.

These methods had conflicting presuppositions. The naturalist method presupposed that variation in nature was continuous and worked over a long period of time (what we would today call evolutionary or geologic timescales). The study of variation and heredity therefore needed to happen in nature, since studying long term variation out of context was bound to lead to error. The experimentalist method presupposed that variation in nature was discontinuous, working in discrete leaps over a single generation. Therefore research was best done in a laboratory, where the mechanisms of these sudden mutations could be reproduced in controlled settings. Scientists in each camp would argue for their favoured presuppositions not just with theoretical arguments, but also with "strategies of exemplification," pointing to organisms they thought worked as favourable cases in favour for their preferred approach.[4] This may seem strange to us today, since these presuppositions are not at odds and have not seemed so for several decades. We accept that variation can happen quickly and slowly under different conditions,

[3] Allen 1979, p. 179 and Morange 2021, p. 131ff. I use Allen's terminology here; Morange instead uses the more contemporaneous "*Naturphilosophie*" and "experimental method."

[4] Lipphardt 2016, p. 114.

and that continuous, long term change happens because of small, discrete changes in genes, developmental processes, and the environment. This, however, is not a self evident truth; it was an intellectual achievement with many actors and a long history, culminating in the evolutionary synthesis.

The evolutionary synthesis happened roughly between 1920 and 1950.[5] Before this period, there was no stable consensus among biologists about the forces behind biological diversity or heredity. Instead, there were roughly two camps, those who believed in continuous variation and those who believed in discontinuous. Ernst Mayr has written that "the radically different thinking" of these two camps showed "in their studies of causation (proximate vs. ultimate), in the level of the evolutionary hierarchy with which they were concerned, and in the dimensions they studied. They represented two very different 'research traditions'."[6]

Naturalists, especially in morphology, tended by the early twentieth century to think about change as a shift within populations over time. This view was influenced by Darwin, who had sought a secular explanation, via natural selection, for the apparent designs of nature. Experimentalists, who conducted their research in the laboratory, rejected outward structural descriptions in favour of internal mechanisms, especially germinal and (later on) genetic mechanisms. Mechanisms, they argued, could be isolated and studied systematically, while the study of structural change in populations relies on historical speculation and teleological principles. Naturalists were equally dismissive of laboratory methods, which they deemed too artificial and divorced from the environment to explain what happens in nature. "Experimentalists found evolutionary questions tiresome and basically unanswerable by traditional methods of descriptive morphology; at the same time naturalists

[5]The periodisation of the synthesis varies somewhat, but generally speaking historians agree that it occurred after the First World War and ended by the mid 1950s. For example, Smocovitis 1996 emphasises the years 1918-1945, while Provine 1983 delineates a slightly later period from 1930 to 1950, and more recently Morange 2021 favours two separate periods, an interwar rise of population genetics 1918-1932 and the evolutionary synthesis proper 1937-1950.

[6]Mayr 1980, p. 40. Mayr was a systematist and zoologist who worked during the synthesis and who later pioneered the historical study of the period. "Research traditions" refers explicitly to Larry Laudan's theory of scientific rationality through puzzle solving in Laudan 1977, which takes the unit of scientific rationality to be a tradition with its history of progress (or failure) on empirical and conceptual problems, instead of a single theory's verification (or falsification). Thus the description is fitting, even though, as we will see, there were many theories proposed within each tradition.

found problems of genetic transmissions virtually irrelevant to the broader issues of phylogeny and evolution."[7]

The methodological standoff and the conflict between rival approaches to evolution reached a tentative peace in the evolutionary synthesis. The synthesis began after the development of chromosomal theory and population genetics. The former came from the discovery of chromosomes and how they help transmit genetic information (though we have to remember that this was still decades away from the discovery of DNA and the rise of biochemistry). The latter was a statistical formalism which allowed scientists to describe and predict the genetics of an entire population over time. This formalism, alongside many experiments and theoretical discussions, created a context where the presuppositions of naturalist and experimentalist methods could be mutually reinforcing rather than conflicting.[8]

Understanding how this shared context emerged is important to then understand Wright's debate with Fisher and Dobzhansky's monograph. We will do this in two segments. First, we will follow the general historical background, namely the waxing and waning dominance of morphology after Darwin, and the development of genetic theory with the rediscovery of Mendel's work around the turn of the century. This will help us understand what the naturalist and experimentalist approaches to biology were, and why scientists took them to conflict. We will end this first segment with T. H. Morgan's shifting views on evolution and genetics in light of his experimental work. Morgan was an important mentor to Wright and Dobzhansky.

Second, we will follow the immediate background of our two case studies, namely the development of population genetics by Wright and Fisher, Wright's shifting

[7]Allen 1979, pp. 179-182.

[8]Though this tends to be the consensus among historians today, as in Morange 2021, pp. 297-298 and Tamborini 2020, pp. 211-213, it is worth noting that sociologists in the 1980s often argued that the synthesis proceeded by coercion rather than convincing reasoning. That is, that genetics systematically drove naturalist disciplines out of research institutions (such as universities and museums), thereby forcing the adoption of genetic methods. See, for instance, Sapp 1983. This externalist explanation was then countered in the 1990s by Cain 1993 and Smocovitis 1996, not by turning exclusively to internal factors, but by treating institutional and social change as part of the intellectual development of the sciences rather than an alternative current.

balance theory of evolution, and Dobzhansky's experimental work in the Morgan lab. After this, we will move on to the case studies themselves: the Fisher-Wright debate, and Dobzhansky's appeal to unify the genetic and naturalist approaches to heredity. Finally, we will consider the relevance of the historical cases to the philosophical debate between monists and pluralists.

2.1.1 Morphology and Genetics, 1859-1916

Following the publication of Darwin's *Origin of Species* in 1859, biological classification became a matter of phylogeny, the determination of descent. Structural similarities between species, which had before been used as evidence of a common archetypal form, was now used as evidence of a shared effect of natural selection, whether inherited from a common ancestor or caused by similar environments. Morphology, the study of the forms of organisms, began adopting these new phylogenetic ideas and, beginning in 1860s until the 1880s, became the largest biological science. Being a large science, it included many subdisciplines, like comparative anatomy and the fledgling disciplines of ecology, paleontology, cytology, embryology.[9]

Morphology was not all encompassing. The medical sciences and physiology remained distinct traditions, with aims and methodologies which conflicted with morphology's.[10] Morphology inherited the tradition of natural history, so morphologists sought to describe features of the present which could help them reconstruct a lost past. This was true even of morphologists who performed dissections and vivisections. Experimentalists, however, wanted to discover the hidden mechanisms which produced organisms and caused them to have their traits. The naturalists adhered to a distinction between homologies and analogies: homologies are traits which come from a common ancestor, while analogies are accidental similarities. Classifying organisms based on function (such as the biblical classification of beasts of the Earth, the sea, and the air) could obscure the underlying tree of

[9]On the eighteenth and nineteenth century origins of these subdisciplines, see Morange 2021, p. 106ff. On ecology and morphology, see pp. 200-203. For the latter three's relationship with morphology, see Allen 1978, pp. 2-5. On the contrast with morphology before Darwin and issues of teleology, see Ariew 2007, pp. 177-179.

[10]Allen 1978, p. 19 and Bordoni 2017, pp. 41-56.

life. Mechanistic explanations could therefore lead to fallacious inferences about organisms, since that mode of explanation was functional. Meanwhile, the naturalists' historical reconstructions seemed fanciful to the experimentalists, since they seemed speculative, without a firm basis in experience.

These methodological traditions, naturalism and experimentalism, conflicted not just because of the tastes and philosophies of their adherents, but also because they gave different answers to shared scientific questions. At the centre of their common programmes was heredity. Darwin had drawn a distinction between external factors which affect selection and internal factors which generate the organism, but how are selected traits actually passed down?[11] The experimentalist tradition favoured mutationist explanations, which sought an underlying mechanism in the organism's body which changed (mutated) suddenly during germination and gestation. This included Mendel's work in the 1860s, to which we will turn shortly. The naturalist tradition favoured gradualist explanations, natural selection among them, according to which interaction between populations and the environment shaped the species over many generations.[12] Of course, like any historical dichotomy, this neat and tidy division had a beginning and an end. August Weismann (1834-1914) straddled both traditions with his comparative studies of embryos, though he had a mixed reception. Embryos were generally relevant to morphology because their stages of development revealed the organism's natural history, mirroring what paleontologists would find in the fossil record. Besides his interest in those phylogenetic questions, Weismann was also interested in germination due to his debates with Lamarckians, especially Herbert Spencer. He proposed a theory of germ particles, but had no way to isolate them experimentally to test his ideas about cell specialisation.[13]

Weismann's research was not alone in its experimental defects, and this pattern became a catalyst for change. Younger morphologists turned to experimental philosophies and methods beginning in the 1880s until the 1910s. But this was not a unifying period. The conflict was simply transposed from being about which

[11] Ariew 2007, p. 178.

[12] The emphasis on populations over parent-child transmission is key. See Rheinberger and Müller-Wille 2016, pp. 157-159.

[13] Allen 1978, pp. 7-8; Depew 2017, pp. 39-42.

method was correct (naturalistic or experimentalist) to being about the nature of experimentation and what role naturalistic methods had relative to experiment.[14] Several definitions of experiment existed side by side. Some included naturalistic experiments lacking an artificial design, while others excluded anything that lacked isolated controlled factors; some subordinated experiment to testing hypotheses, while others used experiment to gather observations prior to forming a hypothesis. Still, experimentation, however understood, became the norm in several disciplines like cytology and embryology, where researchers became interested in precise mechanistic explanation. Jane Maienschein draws attention to the utopian language of this period. Cytologist Edmund Wilson, writing in 1915, dreamt that experimental methods would obliterate disciplinary boundaries and incite a "revolt" against speculation. Importantly, this did not mean that experiment and mechanism should replace descriptive methods. Some prevailing notions of experiment were little more than the successors of descriptive methods, as if changing clothing to survive the tides of change. Even where there was a more radical break, experiment, as necessary as it was, was only one part of a larger picture.[15]

Even in this shifting intellectual environment, scientists continued to feel the relevance of the dichotomy between naturalism and experimentalism. It continued to shape their institutions and therefore their research. William Bateson (1861-1926), writing in 1894, observed that the two "classes of men" were "in tastes and temperament distinct, each having little sympathy or even acquaintance with the work of the other." He continues:

> Disgusted with the superficiality of 'naturalists', [the experimentalists] sit down in the laboratory for the solution of the problem [of heredity], hoping that the closer they look the more truly they will see. For the living things outside, they care little. With [naturalists] it is the living thing that attracts, not the problem. To them the methods of the first class are frigid and narrow. Ignorant of the skill, and of the accurate

[14] Morange 2021, pp. 183-185 and Allen 1978, pp. 8-9.
[15] Maienschein 1986, pp. 180-181 and Tamborini 2020, pp. 211-212. See also Ghiselin 1980.

> final knowledge that the other school has bit by bit achieved, ... 'naturalists' hear only those theoretical conclusions which the laboratories from time to time ask them to accept.[16]

Of course, the same evidence showing that the dichotomy was entrenched also shows that it was itself a point of tension and target of criticism, and that some (including Bateson) wanted to move beyond the dichotomy.[17]

After the turn of the century, as these debates about evolution and heredity were in full swing, the neglected work of Gregor Mendel (1822-1884) received new attention. A botanist and Augustinian friar, Mendel worked mainly on plant hybridisation. In an 1866 paper, based on his work with pea plants, he outlined a theory of factors and two laws governing their inheritance (familiar today from grade school biology). The first law, segregation, distinguished between dominant and recessive factors and described their appearance in future generations. The second law, independent assortment, states that the factors of different simultaneous traits (such as seed colour and leaf texture in plants) are independent.[18] This research had been neglected for decades. It is important not to exaggerate Mendel's originality. He invented new techniques and laws, but his research came from a long tradition of mechanistic research on development. Nonetheless, Mendel's work had tangible effects on existing debates. As Michel Morange writes, "The rediscovery of Mendel's laws, and the rapid development of genetics caused the balance to tip in favor of those who supported a discontinuous conception of evolution, as opposed to the gradualist concept of Darwinism, which had been picked up and modeled by biometricians."[19]

One of Mendel's rediscoverers was Hugo De Vries (1848-1935), a botanist himself who also worked on plant hybridisation. When he encountered Mendel's work, he had already been working on a theory of evolution by mutation, which he published in 1901. De Vries designed his theory to overcome specific problems which faced the gradualist, Darwinian approach. Darwinians had difficulty explaining

[16] Bateson 1894, pp. 597-598.
[17] Müller-Wille and Richmond 2016, pp. 371-374.
[18] Olby 1966, pp. 124-128
[19] Morange 2021, p. 297. By "biometricians," he means scientists like Francis Galton and Karl Pearson, who developed new statistical methods for naturalistic research.

how small variations could persist and accumulate into distinct species without being diluted by crossbreeding. In De Vries' theory, there is no need to explain accumulation; change happens in discrete events over one generation, with new forms of life arising suddenly. Darwinians also had trouble explaining how natural selection was supposed to work; it was understood as a negative force, something which hindered the unfit. But what caused the change in the first place? Mutations provided a positive explanation for change. Owing to their sudden and cumulative nature, mutations could also account for gaps and progressions observed in the fossil record. Besides these virtues over the Darwinian theory, De Vries' theory had the added benefit that it focused on mechanisms of heredity amenable to experimental research.[20] As a final distinction from Darwinism, the shift toward hereditary mechanism was simultaneously a shift away from phylogeny. By the turn of the twentieth century, biologists held less stock in the explanatory power of species classification. This was because phylogenetic classification was speculative, in that it could not be tested experimentally. Species classification might be convenient, even practically indispensable, but it explained little at a theoretical level.[21]

If the divide between naturalists and experimentalists had become murky around the turn of the century, it reestablished itself soon afterward as biologists organised around Darwinism and Mendelism. But in a twist of irony, Mendelism came under some of the same attacks that had been levied against morphology and Darwinism.[22] The embryologist Thomas Hunt Morgan (1866-1945), whose work was primarily on fruit flies (*Drosophila*), worked throughout this shift, and his own views changed drastically over its course. Early on, he was a Mendelian. His 1903 monograph *Evolution and Adaptation* devotes several chapters to the evidence for Mendelism and arguments against Darwinism. The key problem is its appeal to speculation:

[20] Stoltzfus and Cable 2014, pp. 507-517, Morange 2021, pp. 297-298. See also Allen 1978, pp. 10-16 and Turner 1983.

[21] Provine 1986, pp. 222-223.

[22] Morange 2021, pp. 298-299. There is an early thorough study of this turn away from morphology in Allen 1979, pp. 186-194, which documents the training, education, and published opinions of dozens of biologists between 1880 and 1920.

> Nowhere is [speculative reasoning] more apparent than in the writings of many of the followers of Darwin in respect to the adaptations of living things. To imagine that a particular organ is useful to its possessor, and to account for its origin because of the imagined benefit conferred, is the general procedure of the followers of this school. Although protests have from time to time been raised against this unwarrantable way of settling the matter [of adaptive change], they have been largely ignored and forgotten.[23]

However, there was also speculative reasoning in Mendelian research. That is, some Mendelians reasoned about purely hypothetical structures which had no empirical basis in their experiments. By 1909, Morgan had turned to criticising this tendency in his colleagues:

> In the modern interpretation of Mendelism, facts are being transformed into factors at a rapid rate. If one factor will not explain the facts, then two are invoked; if two prove insufficient, three will sometimes work out. The superior jugglery sometimes necessary to account for the results are so often excellently explained because the explanation was invented to explain them. [...] I realize how valuable it has been to us to be able to marshal our results under a few simple assumptions, yet I cannot but fear that we are rapidly developing a sort of Mendelian ritual by which to explain the extraordinary facts of alternative inheritance.[24]

There were problems beyond the multiplication of hypothetical, rather than experimentally accredited, factors. Mendel's paper was about plant hybridisation in one genus (pea plants), so it was not clear that his ideas applied to the vast diversity of life. For example, pea plants are not sexually dimorphic, but most animals and some plants are. How would Mendel explain inheritance of sexual traits in terms of factors? With an equal ratio of males and females, there was no evidence that one trait is dominant, the other recessive. Even in pea plants, there was more variation in traits than binary factors would suggest.[25]

[23] Morgan 1903, p. 453.
[24] Morgan 1909, p. 365.
[25] Allen 1978, pp. 52-54 and Mayr 1980, p. 17.

Similar problems were raised independently by the botanist William Johannsen (1857-1927). He formulated it as an ambiguity in the term "factor," which referred both to the germ particle and to the adult character that this particle produced. Johannsen's criticism was informed by his research on bean plants between 1900 and 1903. He bred two lines of bean plant from an original heterogeneous population. One line was selected for large seeds, the other for small. Once he reached a point where there were two recognisably distinct populations, he tried to inbreed them to yield yet larger and smaller seeds, with no success. Two hypotheses struck him: first, since further inbreeding did not produce new results, the lines must be entirely pure with genetically identical members; second, since these pure populations showed a normal distribution in seed size rather than being uniform, the genetic factors cannot have wholly determined seed size. Clearly, then, it was inappropriate to use the same term, "factor," to refer to both genetic material and observable characters. Some years later, in 1909, he coined the (now familiar) genotype-phenotype distinction to clarify the difference, a distinction which Morgan put to use in his later work.[26]

The genotype-phenotype distinction and the ambiguity it resolved reveal something about the dichotomy between naturalism and experimentalism after the turn of the century. Naturalists focused on continuous variation in observable traits, which is phenotypic; experimentalists on intrinsic, hereditary characters whose mutations cause discontinuous variation, which is genotypic. Naturalists were interested in perceptible variation within a population over time; experimentalists in the internal mechanisms that lead to mutations in each instance of reproduction. Johannsen is an example of a broader trend: "there was a tendency among naturalists to see phenotypic differences as important, and among experimentalists to see genotypic differences as important, for understanding the process of evolution."[27] To recognise that tendency is another way to see what Bateson saw about the state of biology in his day.

By the early 1910s, Morgan had made breakthroughs on the chromosome which

[26] Allen 1979, pp. 196-199. Note, however, that Johannsen was not a Darwinian, but a mutationist; Morgan was adapting ideas from the mutationist milieu which he had been part of. See Stoltzfus and Cable 2014, pp. 533-539 and Bonneuil 2016, pp. 213-226.

[27] Allen 1979, pp. 199-201.

resolved his concerns about Mendelism and renewed his interest in Darwinism. The 1915 monograph *The Mechanism of Mendelian Heredity* drew together existing research on the cell and inherited traits to argue that genotypic factors were located in paired chromosomes, and that the number and kind of chromosomes differed between species. Morgan had begun his *Drosophila* research to test whether and how mutationist theories, like those of De Vries and Mendel, could apply to animals as well as plants. While breeding his flies, he found that certain nonsexual traits were sex segregated, and he hypothesised, based on recent work in cytology which found that sex characteristics were linked to inheriting a normal or odd shaped chromosome (X and Y, in today's terminology), that *Drosophila* eye colour was influenced by these special chromosomes.[28] This evidence provided an experimental, rather than speculative, basis for mutations in a wider class of species. It also, Morgan thought, provided a new angle of evidence for Darwin, though a novel one. He argued as much in his 1916 *A Critique of the Theory of Evolution* (which is more a constructive critique than adversarial):

> It is in this sense that the evidence from comparative anatomy can be used I think as an argument for evolution. It is the resemblances that the animals or plants in any group have in common that is the basis for such a conclusion: it is not because we can arrange in a continuous series any particular variations. In other words, our inference concerning the common descent of two or more species is based on the totality of such resemblances that still remain in large part after each change has taken place.[29]

Morgan's research, compiled in the 1915 and 1916 monographs, encouraged a large and fruitful programme to marry the Darwinian and Mendelian perspectives on inheritance, a programme of which E. B. Wilson and Theodosius Dobzhansky were a part. Morgan's work on the chromosomal basis of Mendelian genetics supplied resources that made Darwin's ideas newly viable.[30] However, evolutionary theory was not where he focused his energies, since he felt it was too much to hope

[28] Morgan et al. 1915, pp. 6-20. For information on the cytological research which had influenced Morgan, see Weinstein 1980 and Maienschein 1983.

[29] Morgan et al. 1915

[30] Smocovitis 1996, pp. 126-127. See also Allen 1980.

for a quantitative analysis of genetics in populations. Instead, Morgan's focused his efforts, alongside colleagues in Germany and America, to unify the new science of genetics with embryology.[31]

2.1.2 Population Genetics and Evolution, 1922-1937

Others undertook the effort to establish population genetics. Sewall Wright (1889-1988), J. B. S. Haldane (1892-1964), and R. A. Fisher (1980-1962) developed it independently. Here we will focus on Wright, due to his significance for Dobzhansky. Wright's work relied on three key assumptions: first, the power of natural selection to alter gene frequencies in a population; second, the relative weakness of mutation effects compared to selection effects; but also, third, natural selection's inability to account for nonadaptive variation. The first two assumptions he shared with his colleagues, but the last was his own, and later became a source of tension with Fisher, to which we will return.[32]

In 1925, Wright drafted an article entitled "Evolution in Mendelian Populations," which he would continue to rework before publishing in 1931.[33] It contained his reflections on genetics, specifically the mathematical properties of dominant and recessive transmission within a population over time. His work had two important influences. The first was similar work by Fisher; Wright had begun to consider these matters after reading Fisher's 1922 paper, "On the Dominance Ratio,"[34] an event that also marks the beginning of a long correspondence. The contents of their letters are as adversarial as they were constructive, for though were both Darwinians, Fisher insisted more bullishly on the second of the three key assumptions. He argued that natural selection overpowered every other force to such an extent that they were negligible and therefore unnecessary in evolutionary theory, a view known as panselectionism. Wright, by contrast, felt that the power of natural selection depended on synergy with other forces. Natural selection without random mutation, genetic drift, and environmental effects would

[31] Reid 1985, pp. 218-219 and Maienschein 1983.
[32] Morange 2021, pp. 300-303 and Provine 1986, pp. 232-233.
[33] Wright 1931
[34] Fisher 1922

be inert. Biological change was caused by a shifting balance between these factors. Accordingly, he dubbed his view the shifting balance theory of evolution.[35]

The second influence, part of the driving force behind his reply to Fisher, was contemporary systematics. Somewhat unusually for a laboratory scientist at the time, Wright read field researchers widely, and not only their most recent work but also from decades prior. These the entomologist Vernon L. Kellogg (1867-1937) and the eminent evolutionist (and fish expert) David Starr Jordan (1851-1931), who cast doubt on the adaptive value of many variations in nature; and neo-Lamarckians Wilfred Osgood (1875-1947) and Francis B. Sumner (1874-1945), whose work was on the genus *Peromyscus* of deer mouse.[36] Wright never abandoned his feeling that natural selection was the principal cause of life's diversity. Still, as he had no firsthand scientific knowledge of natural populations, he relied on the weight of testimony from systematics to conclude that purely selectionist theories of evolution could not work.[37] In this respect, Wright stretched the naturalist/experimentalist dichotomy.

Wright continued to work on the 1931 paper as he engaged with Fisher. Its publication was marked by positive but mystified reactions. At the time, mathematical approaches to genetics were rare, and it was rarer still to see mathematical genetics combined with evolutionary theory. In particular, Wright had no forerunners in America; the two others to independently come upon mathematical population genetics, R. A. Fisher and J. B. S. Haldane, were British. One of the aims of Wright's paper, an aim he shared with Fisher and Haldane, was to undermine De Vries' influence in genetics. Using a mixture of thought experiment and deduction, he argued that in large enough populations, isolated from each other, small changes in traits passed over generations could result in speciation events. The speed of these accumulating changes was largely a function of the intensity of the

[35]Provine 1986, pp. 238-243.

[36]Sumner, incidentally, was a generational pioneer in blurring disciplinary distinctions. He primarily worked with natural populations, but he would also do controlled experiments, especially involving breeding, and he was abreast with the latest developments in genetics. See Provine 1979 and Maienschein 1986.

[37]Provine 1983, pp. 50-57, which also includes several other examples of field work that Wright read.

selection pressure, the size of the population, and the genetic diversity of the initial population.[38]

Wright published several more papers in the 1930s and 1940s, honing the mathematics to account more precisely for all the important factors, clarifying the causes and units of selection, and, just as importantly, supplying nontechnical explanations so that most biologists could follow his work. These papers were spurred by criticism and discussion from 1932 onward. Haldane drew attention to the incongruence of the mathematical model with the qualitative picture of shifting balance theory. For example, the mathematical model did not allow different genetic loci to interact or influence one another directly, whereas the shifting balance theory assumed that genes always interact with each other to some degree.[39] Fisher also continued to be an encouraging critic during these years, as was Morgan's student E. B. Ford. Together, they pressed Wright to clarify the role of genetic drift and random mutation compared to selection. They alleged that on Wright's theory, despite his best intentions, genetic drift was an alternative to natural selection.[40] Wright felt obliged to publish a reply; we will turn to their exchange later.

These years also occasioned fruitful collaboration with Theodosius Dobzhansky. The Russian naturalist and geneticist began his career researching natural populations of ladybugs, turning later to fruit flies. There was a tension in his research: his fieldwork was almost never informed by genetic experiment (because his available subjects and their environments were too complicated to control), and his laboratory work was almost never supplemented by surveys of natural populations. He felt that deeper questions of evolution, which ultimately motivated him, could not be answered without resolving the tension. In 1928, he arranged to join Morgan's lab in America, where he worked under Sturtevant. There he was able to work both on laboratory populations of *Drosophila melanogaster* and field populations of *Drosophila pseudoobscura*. It was in 1932, as work on *D. pseudoobscura* was beginning in earnest, that he met Wright. Dobzhansky attended a conference where Wright gave an extended presentation of the shifting balance theory in both

[38] Provine 1986, pp. 277-287.
[39] Haldane 1932, pp. 212-213 and Provine 1986, pp. 304-307.
[40] Provine 1986, pp. 287-291.

mathematical and qualitative forms.[41]

Dobzhansky was enthusiastic about Wright's theory. Because of their acquaintance, Wright also began visiting Morgan's lab to collaborate with Dobzhansky on issues of experimental genetics and the theory of evolution. Between 1932 and 1936, the lab made discoveries about the genetic causes of sex ratios and the role the chromosomal variation (the arrangement of chromosomes apart from differences in genes) plays in trait differentiation, among other things. After those years of intensive experimental work, Dobzhansky began two large writing projects: a series of articles, called *Genetics of Natural Populations*, on what the Morgan lab's research can contribute to the theory of population genetics, and the manuscript to *Genetics and the Origin of Species*, in which he tied together work in systematics, genetics, and Wright's theoretical work. The latter began as an invited lecture series on evolutionary theory and its relationship with genetics, delivered in autumn 1936. Afterward, it morphed into a book, published in September 1937.[42]

2.2 The Fisher-Wright Debate and Dobzhansky's Synthesis

The foregoing historical background focused on three themes in the period preceding our case study: shifting tensions between naturalist and experimentalist methods, shifting fortunes of gradualist and mutationist explanations of species change, and shifting boundaries between biological disciplines, especially after the emergence of genetic theory.[43] Whenever an approach to any of those questions

[41] Provine 1986, pp. 327-334 and Smocovitis 1996, pp. 119-122.

[42] Provine 1986, pp. 334-342 and Cain 1993, pp. 9-11.

[43] There was also much not covered, like the controversy between mechanists and vitalists, the specialisation of other areas of natural history such as geology, the development of the Pavlovian school of physiology and psychology, the rise of eugenics and social Darwinism, and so on. This was necessary given the scope of the present aim, to give enough background to the conflicts relevant to Dobzhansky and Wright's synthetic work. On mechanism and vitalism, see Morange 2021, pp. 110-150. On psychology and psychiatry, see Allen 1978, pp. 73-112 and Porter 2016. On geology and its relationship to natural history and the emerging biological sciences, see Sulloway 1979, Winsor 1979, Gould 1979, Gould 1980 and Hodge 1983. On eugenics and social Darwinism, see Norton 1983 Paul and Spencer 2016. On the genetics of sex and gender, see Satzinger 2016.

started to become dominant, another conflict sprung up. There seemed to be no lack of space for new and rival approaches, and no stable ground for one to take hold. Yet these debates were structured in such a way that the approaches were rivals, and tolerance was extended only insofar as nobody had a definitive argument in favour of their answer.

We will now look more closely at Wright's theory in the context of an experimental and theoretical debate with R. A. Fisher and E. B. Ford.[44] Afterward, we will look at Dobzhansky's methodological synthesis in *Genetics and the Origin of Species*. These cases are useful to us because they are examples of scientists earnestly trying to unify their discipline. They give clear, explicit arguments for their preferred approach to unification. Fisher and Wright wrote back and forth in journal articles spanning a few years, in which they tried to home in on exactly which assumptions set them apart and exactly how their views could be distinguished experimentally. That is, they worked hard to articulate exactly what it is that caused the friction between their views, despite the different contexts for their theories. Dobzhansky likewise showed a deep understanding of the friction between genetic and naturalist approaches to evolution. Using this understanding, he drew from a vast amount of experimental and theoretical research to resolve the conflict. In both cases, as in the historical background, friction is present between different ideas. They are taken to conflict until there is a positive reason to see how they can coexist.

2.2.1 Fisher versus Wright on Evolutionary Forces

Wright and Fisher, pioneers in population genetics, each sought to overcome persistent divisions within their discipline: how to explain adaptive change, which for both of them is the driving force behind evolution. They differed markedly on how to do so. Wright believed that it was futile to emphasise only one evolutionary cause over all others; there should be a systematic study of how all such forces interact. Fisher offered a panselectionist theory where natural selection was always the predominant factor in adaptive change. Dobzhansky, as we have already

[44]Unfortunately for Ford, Fisher has stolen the spotlight, since this debate is almost universally called the Fisher-Wright debate.

learned, was frustrated with the methodological limitations that a divided biology imposed. He believed that the biological sciences would be improved if only they could become more integrated. Since we have already looked at his experimental background, we will focus here on his theoretical and methodological arguments.

Besides their extensive private correspondence, Fisher and Wright also engaged in public debates both in print and in person. Here, we will examine one of those controversies: a print debate (in a series of articles between 1947 and 1951) about the theory of evolution. First, some preliminary scene setting is in order. Fisher and Wright's mathematical representations of population genetics were formally equivalent, in that given the same initial conditions, they produced the same results.[45] Population genetics, however, is a tool to bolster evolutionary theory, not the whole of evolutionary theory itself. It describes the statistical propagation of genes over generations given different assumptions about the genome and the environment. The point is to establish that genes are the internal mechanism which the forces of evolution act upon. It says nothing overtly about what those forces are; that is the task of the broader theory of evolution.

On the theory of evolution, Fisher and Wright differed markedly. Some differences arose because they believed in different evolutionary forces, which was expressed in conflicting interpretations of experiments (of which we will see one shortly). Other differences are expressible in terms of the initial conditions they put into the population genetics formalism. They used "adaptive landscapes" to model the fitness of a genome as it mutates. An adaptive landscape is not a physical environment, but a mathematical space of as many dimensions as there are genes in the genome. It measures the fitness in a population's genome as it changes,

[45] Provine 1985, p. 198 and Skipper 2009, p. 309. Part of the reason for this is that they, as well as Haldane, built on earlier mathematical work by G. H. Hardy. Hardy had described the proportions of genes over successive generations in idealised conditions, where the population is isolated, randomly breeding, and initially differs only in one gene (a dominant form AA and recessive form aa). If q is the proportion of AA individuals in an unmixed population (so that, by definition, $(1-q)$ is the proportion if aa individuals), then in the next generation Hardy's formula says the proportions will be $q^2AA : 2q(1-q)Aa : (1-q)^2aa$, where Aa is the heterozygous gene. Fisher, Wright and Haldane each developed formulas for a wider range of conditions, including populations differing in multiple genes, the effects of selection on a gene, and the reversibility of mutations. They happened to agree independently on how all of that should be modelled. There is a mostly qualitative discussion of this in Dobzhansky 1937, ch. 5, while Provine 1986, ch. 8 has a technical discussion.

whether inherently or because of the external environment.[46] The inherent fitness of a genome depends on factors like how prone it is to mutations (too many or too few would be unstable). In his theoretical papers, Wright discussed adaptive landscapes in the simple case of two dimensions in his papers, so that changes to the genome could be represented graphically as a topographic map with peaks and valleys. The idea is that there are many combinations of genes that are adaptive in a particular environment. Mutations represent a move up a peak or down into a valley. A key problem for the theory of evolution, on this model, is to explain how a population can shift from one adaptive peak to a higher one. It is difficult to see how natural selection alone could explain a process whereby a population improves its fitness by first becoming less adaptive. Fisher, on the other hand, represented adaptive landscapes as one peak with a series of long ridges radiating from it. On this model, the theory of evolution needs to explain how a population can avoid extinction by moving up the ridges and peak. Natural selection is well suited to that task.[47]

Different idealising assumptions led to these different models. Both agreed that the larger the genome in question (with all its possible combinations), the less likely a mutation is to be advantageous. However, Fisher made the idealising assumption that populations were infinitely large. Thus it was plausible that the adaptive landscape for a population, as the number of varying genes increases, dissipates into a single peak. On this picture, the selective value of a gene is enough to alter the population's evolutionary fortunes. Wright made a very different idealising assumption, that the environment remains fixed for a given population. He figured that it occurs often enough in nature to make it a reasonable simplification of the mathematics. From this it follows that, normally, populations become more adaptive by some supplementary factor other than fortuitous environmental change. That factor is the partial geographic isolation of subpopulations.[48]

The specific debate in question, between 1947-1951, was about the interpretation of an experiment. It began with field work by Fisher and Ford on a colony

[46]The distinction between external and internal factors was taken directly from Darwin's *Origin*. See Ariew 2007, pp. 178.
[47]Skipper 2009, pp. 308-309.
[48]Skipper 2009, pp. 303-304 and 309-311 and Provine 1986, pp. 271-276.

of Scarlet Tiger moths (*Panaxia dominula*) near Oxford. The colony was geographically isolated, hardly ever breeding with others of its species; therefore they took it to be a good case to test the relative merits of Fisher and Wright's theories of evolution. They examined generations of the moth over several summers for changes in its wing coloration, in particular the frequency of a heterozygous variant *medionigra*, which lacks a yellow spot in the centre of its hind wings. To judge the long term change in frequency, they compared their field data with available historical records, especially specimens from local collectors and museums.[49] Through a series of statistical arguments, they claimed that population size and the frequency of the *medionigra* variant were not correlated, and that the overall frequency of the variant increased substantially between 1900 and 1947.

In a section discussing the significance of their study, they contrast their findings with Wright's theoretical claim that subdivision into smaller subpopulations can have an adaptive benefit. On their reading, Wright claims that "Subdivision into isolated groups of small size is favourable to evolutionary progress, not, as others have thought, through the variety of environmental conditions ... but, even if the environments were the same for all, through the non-adaptive and casual changes favoured by small population size."[50] For Wright, those changes are favoured due to sampling effects: subpopulations will have different proportions of genes compared both to each other and to the overall population, so the genome changes faster than if the population was not subdivided. Against this view, Fisher and Ford argued that every population, regardless of its size, is subject to selection pressures, and that the effects of such pressures produce changes which no random process could account for. They take the large increase of the *medionigra* variant to be empirical evidence of this. By a sophisticated statistical argument, they conclude that the increase of the variant was over a hundred times greater than chance, a fact "fatal to the theory which ascribes particular evolutionary importance to such fluctuations of gene-ratio as may occur by chance in very small isolated populations."[51]

[49]They assumed, plausibly, that because collections emphasise a diversity of specimens, they would exaggerate the rates of the *medionigra* variant, which did not affect their argument. See Fisher and Ford 1947, p. 149.

[50]Fisher and Ford 1947, pp. 167-168.

[51]Fisher and Ford 1947, p. 171.

Wright published a reply in 1948. In his reply, he sought to clarify his position, which he felt Fisher and Ford had misrepresented, and he offered an argument against their interpretation of the experimental data. They misrepresented him by exaggerating the importance of isolation and insisting on the wrong kind of isolation (total instead of partial). The upshot is an "antithesis," an exclusive dilemma, between adaptive gene fluctuations being caused by "accidents of sampling" or by differential selection. On the contrary, Wright's view is that sampling effects from partial isolation are a precursor (in fact, one of many contributors) to intergroup selection, which is distinct from the selection of differentiation in individual genomes favoured by Fisher's panselectionist theory.[52] As for the experiment itself, Wright pointed out that there are significant time gaps in the population data, and that after each gap there is a marked reduction in the frequency of *medionigra*. Though Fisher and Ford's conclusions are compatible with the data, these gaps mean that they cannot rule out that drastic population declines during the unmeasured periods were the primary cause of the altered gene frequencies.[53]

Wright's replies are connected in the following way. Fisher and Ford's argument has the form of a disjunctive syllogism: either the changes are due to random sampling, or they are due to selection; but they cannot be due to sampling; therefore, by process of elimination, they are due to selection. Notably, Fisher and Ford provide no positive evidence for selection effects. Wright's criticism of the statistical data and analysis means that they did not properly rule out the alternative; their study is inconclusive.[54]

The discussion degenerates somewhat in the two further replies, though it is still useful for mapping the differences in representation between both sides. Fisher and Ford did not discuss criticisms about the experimental design or statistical analysis in their brief 1949 reply. Rather, they focused on sharpening the theoretical

[52]Wright 1948, p. 281. See also Wright 1951, p. 454.

[53]Wright 1948, pp. 283-285.

[54]As it turns out, later studies found that neither selection nor genetic drift were the cause of the increase, but that the finding was a mixture of poor sampling, miscounting variants, and changes in environment that caused changes in coloration. The environmental changes contributed to the sampling and counting issues by making some variants resemble each other more closely. See Skipper 2009, pp. 314-316 for discussion and references.

differences and offering a separate argument against genetic drift. They make two points. First, gene frequency fluctuations occur in populations of all sizes, large and small. Therefore, other things being equal, whatever effects we ascribe to such fluctuations in small populations we must also ascribe to larger populations. Second, Wright misread them when he outlined the form of their argument (what Wright called antithesis): "Not only do we presume throughout that accidents of sampling produce their calculable effects in causing fluctuations in gene ratios, but we take some care to evaluate them."[55] In fact they do, but, as Wright points out in the final part of the exchange in 1951, his problem was not that they allowed no calculable effects, but that they dismissed the evolutionary significance of such effects prematurely, before they had properly vetted their statistical analysis for biases and errors.[56]

Wright identifies several points of agreement and the source of their disagreement. The crux of their common ground is that gene frequencies, fluctuating by natural selection, are the primary basis of evolutionary change, and that small scale mutations in large populations furnish material for selection to act upon, while large mutations or mutations in completely isolated populations are not productive. They disagree on whether "control by selection is so direct and complete as to preclude any significant role of random processes above the level of mutation," such as partial geographic isolation.[57] While Wright agrees that large populations see drift in gene frequencies, his point is that partially isolated subpopulations with different samplings of genes will drift in slightly different directions. The gene pool is thereby rendered more diverse in the overall population, allowing for more rapid evolution when those subpopulations meet and interbreed.[58]

Here then are two very different representations of evolution. The source of that disagreement came despite their agreement on the equations necessary to model population genetics. They made different assumptions about initial conditions. Fisher supposed populations were infinite while Wright distinguished cases for different populations sizes and structures; Wright supposed that environments

[55] Fisher and Ford 1950, p. 118.
[56] Wright 1951. p. 457.
[57] Wright 1951, p. 455.
[58] Wright 1951, p. 458.

were fixed while Fisher accounted for changes in the environment. These are empirical differences of a subtle kind. The point is not that populations are actually infinite, nor that environments are actually fixed. If that were the point, then the disagreement could be settled simply by pointing out that populations are finite or that environments change. Rather, it is that population size and environmental cause are much less causally important than other factors.

As a test of which representation is correct, the experiment on *Panaxia dominula* did not accomplish much. This is true even by contemporary standards, upon reexaminations of the evidence and from surveys done on the same colony since.[59] It was not what philosophers would call a crucial experiment, which finds some piece of observable knowledge that is categorically compatible with one theory but not the other. It did, however, sharpen the disagreement between Fisher and Wright, allowing them to articulate the assumptions behind their models and methods.

If the *Panaxia dominula* experiment was not a crucial experiment, maybe no decisive result was forthcoming at all. A pluralist might say that Fisher and Wright have "parsed causes" differently, emphasising "some causal aspects of the situation while obscuring others,"[60] so that their theories applied to different situations. In that case, following Chang's analysis of the chemical revolution, these biological theories would belong to different problem situations which, though they overlap somewhat and bear a family resemblance, are ultimately incommensurable.[61] On a pluralist interpretation, each model has its strengths: Fisher's in large, randomly breeding populations with a single peak in their adaptive landscape, Wright's in subdivided populations with hilly adaptive landscapes. Since their models serve different purposes, they do not necessarily conflict. It may happen that a conflict will be discovered at some point, but there is no reason to assume it exists at the outset.

[59]It is noteworthy, however, that contemporary research has resolved some of the other debates between Fisher and Wright. For instance, Fisher's theory of the evolution of dominant traits has been abandoned, while Wright's ideas on dominance have lived on. For details on these more recent experiments, see Skipper 2009, p.304ff.

[60]Kellert, Longino, and Waters 2006, p. xiv.

[61]Chang 2012, pp. 58-59.

Of course, this flies in the face of the actual debate. Neither Fisher nor Wright intended their theory to apply only to some kinds of population and environmental conditions. The point of Fisher and Ford's experiment was to show that panselectionism could handle small, isolated populations. Wright urged that his theory applied to populations of any size, and that the fixation on small populations was a misinterpretation. Moreover, because they agree on the actual mathematics, it was entirely possible to compare their models to one another by accounting for the effects of their assumptions (mathematically, their initial conditions). Things are slightly more complicated at the level of theory, as opposed to mathematical models. It is simple enough for Wright's theory to encompass the effects of Fisher's in specific cases where the balance of forces overwhelmingly favours natural selection. In that sense, it is possible to translate Fisher's theory into Wright's as a special case. The converse is not true, however, since to support his theory, Fisher needs to show how various forces in Wright's are not productive. For example, since Fisher discounted genetic drift as a force for adaptive change, there is a large class of cases from Wright's theory which cannot ever be included in Fisher's as a special case.

At this point we should remember the distinction between the engaged and detached perspectives. We have been following Fisher and Wright's engaged perspectives as scientists engaging in experimental and theoretical research. This debate, as with the preceding history of the discipline, was structured so that differences between views caused friction, which raised questions and problems. But it is possible to interpret the philosophical significance of this differently. Waters, for instance, relies implicitly on this distinction when he dismisses Morgan's intolerant attitude toward other approaches: "Readers might complain that I've been too generous to Morgan and subsequent gene-centrists. After all, Morgan was not tolerant of diversity. [...] But that doesn't mean *we*, as philosophers or historians, shouldn't adopt the pluralist stance when we try to understand ... their science."[62] So it remains to be shown why it matters that biologists, whether in this debate or in any of the debates described above, acted as though their views conflicted. Why could we not say that they were engaged in "senseless controversies that do

[62]Waters 2006, pp. 210-211.

not lead to progress"?[63] We will come to this question later.

For now, let us note that there is a sense in which the evolutionary synthesis was pluralist: scientists entertained different causal mechanisms which applied to the study of different phenomena in unique ways. However, this sense of pluralism is quite different from the philosophical one, and scientific pluralists sometimes equivocate between the two to support their view. The clearest expression of causal pluralism in the synthesis comes from the biologist and historian Stephen Jay Gould, who is often cited in the works of scientific pluralists.

Because panselectionism focuses on only one causal mechanism, Gould called the move toward panselectionism in the 1940s and 1950s a "hardening" of the synthesis away from a more pluralist beginning. On Gould's gloss of the history, the early synthesis was about denying the distinction between microevolution and macroevolution, or between short timescale changes which genetics can explain and geologic timescale changes which it cannot. It was therefore also about dissolving the disciplinary distinction between naturalist and experimentalist research, "to render all of evolution by *known* genetic mechanisms that could be studied directly in field and laboratory."[64] Advancing different mechanisms of selection was permissible, since it was unclear which mechanisms existed in nature or which theoretical assumptions would bear the most fruit. Wright, a Darwinian at heart, nonetheless believed that a theory of evolution should incorporate as many factors as possible, giving each its due weight in different contexts. Eventually, the panselectionists won out. If we understand this period as Gould suggests we should, then it makes little sense to say that Fisher and Wright were parsing causes in different but more or less acceptable ways. Instead, Wright was trying to build a framework that showed in which contexts different causal factors were most important, while Fisher was trying to show that natural selection is always the most important factor.

Gould's evidence makes it clear that Wright's approach to evolution was pluralist in some sense, but it is not the philosophical sense. Wright's shifting balance theory aspired to be a complete representation of all the relevant evolutionary

[63]Kellert, Longino, and Waters 2006, p. xv.
[64]Gould 1983, p. 74.

forces. His view was that none of the causal factors which explain biological diversity should conflict with one another or be incommensurable. This vision was philosophically just as monist as Fisher's, albeit with a different theoretical goal. Wright makes a comment to this effect during the debate.

> Science has largely advanced by the analytic procedure of isolating the effects of single factors in carefully controlled experiments. The task of science is not complete, however, without synthesis: the attempt to interpret natural phenomena in which numerous factors are varying simultaneously. [...] My own studies on population genetics have been guided primarily by the belief that a mathematical model must be sought which permits simultaneous consideration of all possible factors. Such a model must be sufficiently simple to permit a rough grasp of the system of interactions as a whole and sufficiently flexible to permit elaboration of aspects of which a more complete account is desired.[65]

Wright's argument for multiple causes is not that we need separate accounts parsing each cause individually, so that we can then understand each cause's relevance to the phenomena we are studying (which was Waters' conclusion about gene-centered biology). His argument was that studying multiple factors simultaneously is necessary if we are to get a more complete account. According to this argument, Fisher's theory cannot be true, whereas it could be on the philosopher's argument.

The debate between Fisher and Wright had monist presuppositions. First, its participants, and the broader community around them, sought a complete account of adaptive evolution. Second, the differences between their views were a source of friction and tension, which motivated debate. Insofar as the science of this period was successful, we have an example of successful monist science. This contravenes the third core principle of the pluralist stance, that science's pluralist structure is constitutive of its success.

The evolutionary synthesis was motivated by dissatisfaction with the state of

[65] Wright 1948, p. 279

present knowledge. That much the pluralist stance can accommodate; communities may have an interest in learning new things, in gaining further precision or clarity. What it cannot accommodate is that differences should imply conflicts Fisher and Wright criticised each other on the grounds that their views could not both be true, because of the differences between them. And they expressed these criticisms without, as the historical background makes clear, the sort of shared context which Longino, or in another form Chang, would require: they consulted different sources and evidence, they appraised what common sources they had differently, they valued different qualities in a theory, they made different assumptions about nature, and they parsed the causal mechanisms differently. Partisans in these debates did not consider the ideas they advanced merely different, as potentially conflicting if only they could discover how to interpret them in a common setting. It was just the reverse: the differences between their ideas and their settings were the source of the conflict.

2.2.2 Dobzhansky's Argument for Synthesis

Dobzhansky's project in *Genetics and the Origin of Species* is to synthesise the methodologies of genetics and evolutionary science. The first edition was published in 1937, five years after Dobzhansky had met Wright in 1932, and ten years before Wright's debate with Fisher. Unlike the Fisher-Wright debate, Dobzhansky's argument is conciliatory rather than adversarial. He believed that recent breakthroughs in genetics could help naturalists and experimentalists set aside their longstanding conflict. These breakthroughs included Morgan's work on the chromosome, Dobzhansky's genetic research on natural populations, and the population genetics of Fisher, Wright and Haldane. Dobzhansky was also impressed by the early stages of Wright's shifting balance theory of evolution. All this, he thinks, makes the conflict between naturalists and experimentalists mistaken. However, he makes this argument not because those two approaches could always have set aside their differences, but because new evidence showed that their approaches could be complementary rather than conflicting. There were new positives reasons to believe that their differences were not conflicts.

The book does not include any new studies; it digests the vast existing literature so that he can reflect on the state of the biological sciences. He cites an appropriately enormous amount of research from fields like systematics, zoology, population genetics, and experimental genetics (including his own pioneering work on the genetics of natural populations with the Morgan Lab).[66] He aims to undermine the common assumption that continuous and discontinuous evolutionary explanations were categorically different and could not mix. Dobzhansky aims for unity, guided by the conviction that the different biological sciences must have some bearing on one another, and that the differences between them are complementary, not adversarial.

He opens his monograph by describing two facts which underlie the study of life. The first is the diversity of living things, the sheer number of individuals and species with unique qualities. The second is the discontinuity of variation, the tendency of living things to cluster into kinds which resemble each other far more than they resemble other kinds. He illustrates this with two closely related species, the domestic cat and the lion: "Any two cats are individually distinguishable, and the same probably holds for any two lions. And yet no living individual has ever been seen about which there could be a doubt as to whether it belongs to the species-cluster of cats (*Felis domestica*) or to the species-cluster of lions (*Felis leo*)."[67] Crucially, he holds that these clusters are not simply a reflection of our language. Rather, the opposite is true: our taxonomic language is an attempt to find "modal points," "statistical abstractions" which represent the clustering of the actual organisms they refer to.[68] This idea had general purchase in biology already. For example, Morgan uses it in his 1903 work, *Evolution and Adaptation*.[69]

[66]Burian 2004 pp. 104-107.

[67]Dobzhansky 1937, p. 5.

[68]"Modal" is derived from "mode," the statistical measure of the most common value of a variable. Although Dobzhansky does not cite De Vries here, this is a rebuttal of De Vriesian scepticism about taxonomy's theoretical usefulness, which we discussed above. Dobzhansky goes on to tackle this topic explicitly this in Ch. 3.

I also cannot help indulging in an aside. There is a striking resemblance between Dobzhansky's discussion of species and a direct reference view of language. For example, a similar clustering metaphor is in Millikan 2017, who uses clusters to give an account of the reference of common nouns, though both the clusters and the nouns can shift boundaries over time. Dobzhansky's remarks here also echo Peirce's remarks on observation in the 1898 Cambridge Conference Lectures: the construction of "skeletonized idea[s]" expressed with words and diagrams which "respond to object[s] of observation." Peirce 1898, p. 182.

[69]Morgan 1903, p. 340ff.

Dobzhansky recognises two broad methods to study biological diversity. These are familiar to us already from our survey of the historical background: the naturalistic and experimental methods. He uses the terms morphological and physiological instead, which highlights their respective disciplinary origins. Morphological methods operate by a "generalising induction," an "order-creating and historical [process]" which models the natural history of organisms (including the change in their modal points over time) constructed from observing many examples. Physiological methods operate by an "exact induction," experimentation on a controlled factor, where the experimenter seeks to establish underlying mechanisms and laws of dependency.[70]

With those preliminaries settled, Dobzhansky lays out his thesis: those two methods are reconcilable and complementary. To see why, he gives a three part definition of evolutionary theory, whose study requires a division of labour between both methods. First, present living things are descended from different past living things; second, discontinuous variation (clusters) arose gradually by continuous means; and third, the causes of past variation continue to operate today and can be studied experimentally. The morphological method deals with the first and second aspects of evolutionary theory, while the physiological method deals with the third. Evolutionists and geneticists, he says, have failed to rally around a unified theory of biological diversity because they have failed to imagine how their studies could relate as parts of a whole, rather than as rivals.[71] This point is perhaps too polemical; we have already seen, from the historical background, that there was a tradition of viewing evolution and genetics this way, running from Johannsen to Morgan to Dobzhansky himself. However, we can interpret Dobzhansky to mean that this tradition was not given as prominent a place as it should have had, given the new evidence.

That they are parts of a whole would become clear, so he continues, if scientists would appreciate the significance of population genetics. Most geneticists had concerned themselves with problems of ontogeny, which is genetic transmission at an individual level and changes through mutation, chromosomal rearrangement,

[70]Dobzhansky 1937, pp. 6-7.
[71]Dobzhansky 1937, pp. 7-8.

and so on. But there are two other levels to the genetic study of evolution: the statistical regularities of populations, and the mechanisms which prevent fusion and breeding between taxonomic groups.[72] Notice that this is not really a theory. He has not described any causes or given the facts behind his statements about diversity. This is a description of evolutionary science at a methodological level.[73]

The rivalry between continuous (gradualist) and discontinuous (mutationist) explanations was bolstered by two beliefs: first, that mutations caused sudden, large, almost monstrous differences, and second, that mutations could therefore not account for the natural history of species. The distinction was plausible given the available evidence in the nineteenth and early twentieth centuries. As Richard Burian observes, it was difficult at that time to identify (let alone study) more subtle genetic mutations; it became possible only once the more obvious mutations had been well understood.[74] Dobzhansky devotes an entire chapter to the cutting edge research (including his own work in Russia and America) on those subtler, less monstrous mutations. But that alone is not sufficient to dismantle the division between continuous and discontinuous explanations. By Dobzhansky's day, it had morphed from a hypothesis tied directly to observation into a methodological principle which scaffolded the institutional structure of entire disciplines. Dobzhansky therefore had to argue against the distinction on methodological, not just factual, grounds.

First, he offers reasons to believe that "any difference between individuals and populations which can be expressed as a function of gene differences is to that extent ... due to mutational changes." The reasons follow from some definitions: his three part description of evolutionary theory implies that "organic diversity may be described simply in terms of ... observable morphological and physiological differences," and his definition of "genic differences" implies that any altered

[72]Dobzhansky 1937, pp. 11-14.

[73]There is a contrary opinion in Darden 1986, pp. 113-115, who says that this exposition outlines the "synthetic theory of evolution," but the consensus over time, as in Gould 1983, Provine 1983, Smocovitis 1996 and Skipper 2009, is that the synthetic theory came later in the 1940s, during the same period as the Fisher-Wright debate. Dobzhansky's project in the first edition is methodological. He hoped that it would become common ground and lead to a unified theory later.

[74]Burian 2004, pp. 106-107.

character with a genetic basis is due to mutations.[75] He knows, though, that the argument is not ironclad just because it is deductive. There is an alternative explanation which he cannot erase out of existence with definitions: "that diverse genes and gene allelomorphs have always existed in nature," so that "evolution could be conceived as a result of recombination and permutation of the [existing] gene-elements" instead of newly mutated forms. Dobzhansky admits that there is nothing contradictory about this theory. He says only that it "impinges on one of the main principles of scientific methodology," the simplicity of hypotheses. Since mutations exist and are heritable, it would be odd if there were an entirely separate genetic process which caused speciation. Much as Morgan argued against the multiplication of Mendelian factors without experimental evidence to support it, here Dobzhansky argues against positing an indefinite number of immutable gene elements.[76]

He later uses the same strategy, an appeal to simplicity, to argue for applying laboratory results to the study of natural populations. As we have seen, this was a contentious move quite apart from concerns about the monstrosity of mutations. Laboratory research happens at a human timescale, while evolutionary change is on a geologic timescale, and the two scales are orders of magnitude apart. The natural environment is also much more complex than the controlled setting of a laboratory. Dobzhansky admits that "there is nothing to be said against such a criticism, except that an unnecessary multiplication of unknowns is contrary to accepted scientific procedure."[77] Except there is something to say, even if it seems somewhat peripheral. He distinguishes between the origin of species and the origin of variation. He allows many times throughout the text that there is much more work to be done toward a theory of the origin of species. But genetics, through both laboratory and field work, has created a robust theory of the origins of variation in populations. There is no reason to doubt, he says, that the mechanisms geneticists have isolated, tested, and observed in nature do not actually produce variation in other natural populations. The problem is just that

[75] Dobzhansky 1937, p. 39.

[76] Dobzhansky 1937, p. 40. He cites Henry Fairfield Osborn, the American paleontologist and eugenicist, as an example of someone who holds this view.

[77] Dobzhansky 1937, p. 119.

the origin of variation is one piece of the puzzle; the origin of species also involves the study of selection, breeding, and environmental factors, all at the population level. By a series of expositions and thought experiments, Dobzhansky wants to convince his readers that mathematical population genetics gives explanations (both actual and potential) of those other factors.

At the time of his writing, genetics gave an account of hereditary variation but not speciation; likewise, natural selection gave an account of adaptive speciation but not heredity. This bifurcation, again, existed at the methodological and institutional level. If it were irresolvable, it would be "the most serious objection" to a synthetic approach. So Dobzhansky must criticise the disciplinary divisions between selectionists and mutationists. The basic mistake of this dichotomy, he says, is to confuse levels of operation. Because while it is true that De Vries showed that hereditary variations arise from germinal mutations, and that Johannsen showed selection can only be effective in mixed populations, it was a mistake to infer a theory of evolution by mutation. Genetics and selection are two parts of a larger process: genetics explains the origin of variation, while selection explains how adaptation arises from variation. Dobzhansky surveys a wide range of experimental and natural evidence from genetics, systematics and paleontology to illustrate these complementary roles.[78] Much of this evidence was not well known across the biological sciences, whether because of disciplinary reading habits or international barriers (language and politics).[79]

His argument had mixed results in practice. Early on, naturalists read Dobzhansky as a defender of morphological methods against the experimental hegemony, while geneticists read him as an advocate of genetics to replace the outmoded natural history. The disciplinary prejudices Bateson described thirty years earlier had not abated. Dobzhansky's point was not widely received and understood until the late 1940s.[80] His point was philosophical: that, as remarkable as it might sound, the simplest way to overcome the conflicts in the biological sciences is to

[78] Dobzhansky 1937, pp. 149-158 for the initial discussion and experimental evidence, and pp. 158-171 for the naturalistic evidence.
[79] Ceccarelli 2001, pp. 37-38. Ceccarelli's evidence includes an analysis of bibliographies, and the review literature in the years after Dobzhansky's book was published.
[80] Ceccarelli 2001, pp. 50-56.

see that those conflicts were artificial. But they were not artificial because it was always possible to make progress by operating independently without taking differences to imply conflicts. Rather, they became artificial because there were positive reasons, arising from the scientific research, to believe that the differences were complementary.

2.3 Settling Differences and Resolving Conflicts

The foregoing case studies pose serious problems for the pluralist stance. The Fisher-Wright debate was predicated on the search for a complete account of adaptive evolution, where differences of epistemic context enlarged the friction between ideas rather than insulating them. Dobzhansky's conciliatory and synthetic arguments relied on positive evidence to show that differences in approach were complementary rather than conflicting. Moreover, these features of the case studies were not isolated, but had a long historical precedent. As Gould argued, the early synthesis, encapsulated in the first edition of *Genetics and the Origin of Species*, was causally pluralist. It just happens that it was causally pluralist in a synthetic, rather than pluralist, manner. The early synthesis entertained multiple theoretical proposals, of which Wright's shifting balance theory and Fisher's panselectionism are two examples. Though these theories represented different approaches, but they did not exist in separate epistemic realms. There was constant friction between them. They coexisted because there was no consensus on which approach was correct, and what the end result of the synthesis should be.

However, it would be naive to close the book on the argument now, and say that scientific pluralism has nothing more to say. Although Dobzhansky was "by any measure ... among the central and most influential participants in this period of evolutionary studies,"[81] it would be misleading to think that his programme was adopted by all or adopted in a uniform way. The same goes for the other synthetic efforts in the mid twentieth century. Each had a varied reception with rival interpretations. Dobzhansky's monograph was understood to vindicate morphology by some, and by others to show that morphology depends on the priority of genetic

[81] Cain 1993, p. 9.

science. The same was true of other works which we have not yet discussed, like the paleontologist G. G. Simpson's *Tempo and Mode in Evolution* and the naturalist Julian Huxley's *Evolution: The Modern Synthesis*. Different disciplines understood synthetic proposals in rival ways. Behind the rhetoric of cooperation and unity lay the brute fact that the synthesis was divided into different programmes.[82] Let us consider briefly how these other two works were received.

Huxley was content with causal pluralism in evolution. They were all willing to survey the theoretical alternatives with a sense of nonpartisan detachment.[83] For the sake of variety, and to reinforce the discussion about the two more detailed cases, let us look briefly at Huxley's attitudes. On his view (in *Evolution: The Modern Synthesis*), different disciplines and scientists will advocate for one evolutionary force or another depending on their evidence and the context of their research. That being given, there were two important aims of the synthesis. The first was not that one of these forces or theories should win out, but that the community would gain a better understanding of the big picture by piecing everything together. It is in this spirit that he approvingly cites Wright's shifting balance theory and Dobzhansky's methodological innovations in genetics.[84] The second aim was to pair evolutionary studies with a genetic theory of heredity, in order to rule out the nonmechanistic views of neo-Lamarckians and orthogenisists.

Huxley is certainly a pluralist about mechanisms and forces. But just as with Wright and Dobzhansky, that does not necessarily imply pluralism about theories. His second aim makes that clear enough, but Huxley also spelled it out explicitly. He writes that the synthesis follows "a period in which new disciplines were taken up in turn and worked out in comparative isolation," so that now there is a "movement toward unification."[85] Throughout the book, especially from the second chapter onward, he insists that the various forces and processes are complementary parts of a whole, rather than rival explanations which need to conflict. This must, however, be understood in the context of his unificationist leanings.

[82] Cain 1993 discusses all three works (by Dobzhansky, Simpson, and Huxley) and their institutional context.

[83] Huxley 1942, p. 125.

[84] Of course, it is also why Wright and Dobzhansky themselves, at least before the hardening of the synthesis in Dobzhansky's case, were opposed to the panselectionist view.

[85] Huxley 1942, p. 13.

This cuts against Longino's reading of Huxley, which emphasises the statements of complementarity while deemphasising those about unification.[86]

The fractured nature of biological research was a problem to be overcome for Huxley as for Dobzhansky; the differences between disciplines and explanations were symptoms of partisanship and conflict. Dobzhansky argues for a standardised weighting of values on which the theory of evolution, married now to modern genetics, is the only promising avenue to explain speciation. His appeals to simplicity are meant to seal the fate of the rival approaches. It is the same sort of rhetoric which Chang laments in the chemical revolution, where different approaches borrow from each other but remain antagonistic.[87]

On Simpson, the historian Joseph Cain argues that there were three different interpretations of his work. Some took Simpson to mean that paleontology was a deductive extension of genetics to a new domain; others that paleontology was consistent with existing genetic theory, allowing for possible applications; and still others that they were independent sciences whose principles were nonetheless complementary.[88]

Longino, citing Cain, argues that these three interpretations created a "productive equivocation," because each of these visions had some merit, and their coexistence enabled separate disciplines to work on common problems without total consensus. That panselectionism won out in the "hardening" of the synthesis was merely an unfortunate mistake for Longino, rather than a sign of monist structure. Even though "individuals were certainly within their rights to accept panselectionism, ... the community was not."[89] As it happens, the panselectionist consensus did not survive. Research on morphology and ecology in the late 20th century reopened the whole question of the synthesis, and it remains open today.[90]

It might seem, then, that the synthesis was not a monist project. Its reception

[86] Longino 2002, pp. 191-192.
[87] See Chang 2012, pp. 29-50.
[88] Cain 1993, pp. 16-18.
[89] Longino 2002, pp. 190-193.
[90] See Reid 1985 and Edlredge 1985 for contemporaneous discussions, and Huneman and Walsh 2017 and Martin-Duran and Vellutini 2019 for more recent perspectives.

was too varied, the approaches were too many, to support any unified interpretation of biology. There was, in Longino's terms, a disparity between individual warrant and community warrant, where individuals had reason for firm convictions while the community did not, where the community is taken to encompass all of the biological sciences. Or, in Chang's terms, different benefits (of toleration or interaction) justified pursuing these approaches, and there was no reason to set all but one approach aside. This poses a problem for a monist interpretation of the synthesis, since it has not ruled out that the project of unification, with differences creating friction and needing positive reasons for tolerance, is based on prejudice rather than sound science.

Such a sceptical argument might raise problems for a monist interpretation, but it does so equally for a pluralist interpretation. It suggests that the very procedures which led to progress, the friction which motivated conflict and the assessment of positive reasons which motivated synthesis, were mistakes. Fisher and Wright acted on convictions about the best way to explain adaptive evolution, to the exclusion of their rival's explanation (even if they respected and learned from each other). Their debate was not structured with a distinction between two levels of warrant, where they were able to take their ideas a true regardless of objections from outside. The rival interpretations of Simpson were likewise exclusionary and conflicting. Even as biologists advanced many theories and approaches, they did so in a context where their proposal had rivals with which they conflicted.[91]

Likewise, Dobzhansky argued for a standardised weighting of epistemic values and evidence, on which the marriage of evolution to modern genetics is the only promising avenue to explain adaptive speciation. His own experimental work and methodological arguments carefully stitched the different approaches together. He was only able to do this successfully because he understood the scientific landscape he was trying to reshape. However, for the same reasons which show his sensitivity to the many existing approaches, it is difficult to interpret his work such that warrant is relative to a local epistemology, that truth is internal to systems of practice, or and that success is prior to truth. Dobzhansky's methodological arguments are not against the existence of some approaches in favour of one; he

[91]Cain 1993, p. 23, including n. 68.

argues, instead that there are positive reasons to believe that the approaches are complementary as parts of a whole.

Biological practice was monist, even if much of the time there were many active approaches. However, a pluralist might say, whatever biologists may have thought, they had no good reason to offer their individual views for universal adoption. This is in fact Longino's judgement about the evolutionary synthesis when she says that historical research on the synthesis "cohere[s] with a social and pluralist approach to the episode."[92] The same pattern of reasoning happens in Chang's judgement about the chemical revolution, or Ruphy's about galactic models, or Waters' about gene centrism in microbiology. The idea is simply that such episodes cannot be evidence for monism, because there was no complete account available, and the varied practices and approaches were ill suited to finding such an account. After all, even Dobzhansky's widely admired thesis, that genetic and naturalistic methods are complementary, was subject to rival interpretations and shifted over time as he published new editions of his monograph.[93]

However, setting aside all issues about the reception of synthetic proposals, pluralists have to explain why scientists could have disregarded the friction between their approaches even without a positive reason. It is difficult to see how they could explain this while remaining true to the pluralist stance's first core commitment, that the philosophy of science should adopt an empirical method which captures scientific practice. The evolutionary synthesis was not an isolated event. It follows a long history of conflicting naturalist and experimentalist approaches. Nor was synthesis an entirely novel idea in this history; there was a desire among some scientists to see naturalism and experimentalism joined, because each perspective seemed limited on its own. But at no point in this history did biological practice allow them to coexist without conflict, until there were positive reasons to believe that they cohered together.

This is the second problem for the pluralist stance, an empirical problem. The pluralist stance requires that different approaches be benign unless there is a positive reason to suggest that they conflict, while in practice scientists treat different

[92] Longino 2002, p. 190.
[93] On this last point, see Gould 1983, which compares the contents of the different editions.

approaches as conflicting unless there is a positive reason to believe that they are benign. A pluralist might object that biologists should have allowed these approaches to coexist, that biological practice was mistaken. But that would be to impose an external normative standard on the sciences, whereas the point of the pluralist stance is to base our philosophical norms on an empirical analysis of scientific practice.

Chapter 3

Methodological Monism

So far, we have posed a theoretical and an empirical problem for the pluralist stance. The theoretical problem is that they could not draw a clear answer to Giere's question, how to distinguish between mere differences and conflicts. Localising the epistemology of science to communities or systems of practice seemed at first to provide a criterion for each community and system. Conflicts arise within a community or system whenever representations cross a boundary set by the local values, aims and interests. Between communities or systems, representations are not comparable enough to come into direct epistemic conflict, though they may have some heuristic or practical value for each other (as in Chang's benefits of interaction). The distinction between mere differences and conflicts is relative to the standards of the community or system. The problem is that so is conformation or truth. It is not just that different communities or systems will have different standards for judging the adequacy of their representations, while having some core usable notion of truth in common. Instead, outside a local community or system, the very meaning of conformation or truth is indeterminate, in the sense that it has no application to scientific practice until it has been interpreted and supplemented by local standards. Thus another way to put the theoretical problem is that the pluralist stance does not avoid epistemic relativism.

The empirical problem is that scientific practice, during the evolutionary synthesis and its historical precedents, did not treat differences and conflicts the way the pluralist stance would require it to. Different approaches, though they had no coherent framework necessary (on the pluralist view) to compare their views directly, had friction between them, so that they were treated as conflicts. Insofar as scientists remarked upon this before the evolutionary synthesis, they emphasised the conflict and sought a positive path toward resolution. Dobzhansky's arguments in *Genetics and the Origin of Species* presented a positive argument, inspired by his experience with the cutting edge of genetics, and synthesising the results of research from several fields over decades. Crucially, his argument was not that these approaches were separate and so the conflict need not arise, but rather that they could be unified. He articulated a methodological programme to construct that whole. The Fisher-Wright debate consisted of two sides, Fisher and Ford on the one and Wright on the other, with two different approaches to the theory of evolution. By all rights, they parsed the causes of adaptive evolution differently. They nonetheless saw their theories as conflicting. Their motive was not to treat special causes very well, and tolerate any awkward but harmless differences between their special theory and the others. Instead, they sought complete theories, a panselectionist theory in Fisher's case, and a shifting balance theory in Wright's. Different approaches conflict unless there is a positive reason to suggest they cohere with each other.

What becomes of the pluralist stance's criticisms of scientific monism? The flaws of the pluralist stance's positive programme do not automatically invalidate its criticisms, but criticisms are easier to address and understand if they are set within such a positive programme. How much of it remains salvageable?

We should remember what commitments the pluralist stance and scientific monism have in common. They both believe that the sciences offer objective knowledge, even if they necessarily differ in what it means for science to be objective. They also believe that the sciences have been successful, so that we have actual examples of such objective knowledge. Together, these two commitments imply a belief in progress. These two commitments are the source of their opposition to relativism, which rejects the progressivist ideals of objectivity and success.[1]

Anyone advancing either view, monism or pluralism, has to explain how progress is possible. A key lesson from the pluralist stance is that we must not reject

[1]For an example of an epistemic relativist who rejects such ideals, see Kusch 2017.

everything that relativists have said, since they also have important lessons to teach. The empirical basis of these lessons, especially from the sociology of science, is too strong to warrant wholesale rejection. If progress, success and objectivity happen in science, then the social context of scientific research must play some role in that. Soon, we will see Longino's criticism of two monist philosophies, one from David Hull and another from Kitcher, which take empirical studies of science seriously but which fail to explain progress in terms of the social context. Instead, they explain it in terms of an external normative standard.

The pluralist stance and scientific monism are not, strictly speaking, contradictory views. If two views are contradictory, then when one is true, the other is false, and when one is false, the other is true. While the pluralist stance and monism cannot both be true at the same time, there are circumstances where they could both be false. The reason why is simple: they share some commitments in common, so if those commitments are false, both views are false. One of those common commitments, which we have just revisited, is a belief in scientific progress and objectivity. We will not abandon that here. But they share another commitment which we should now question: that scientific practice should have a uniform superstructure which enables its success.

Here, we should understand scientific monism according to the definition offered by the editors of *Scientific Pluralism*:

> We take *scientific monism* to be the view that
>
> 1. the ultimate aim of a science is to establish a single, complete, and comprehensive account of the natural world (or the part of the world investigated by the science) based on a single set of fundamental principles;
>
> 2. the nature of the world is such that it can, at least in principle, be completely described or explained by such an account;
>
> 3. there exist, at least in principle, methods of inquiry that if correctly pursued will yield such an account;

4. methods of inquiry are to be accepted on the basis of whether they can yield such an account; and
5. individual theories and models in science are evaluated in large part of the basis of whether they provide (or come close to providing) a comprehensive and complete account based on fundamental principles.[2]

From this definition, it is clear that scientific monism believes in a uniform superstructure. It consists of the aim given by the first point of the definition (and validated by the metaphysics of the second point), and the standards of method and evaluation given in points three through five. We will see that Hull and Kitcher fit this well enough to count as examples. As for the pluralist stance, the superstructure is the plurality of approaches with its local standards and assumptions, encoded in the third core commitment. Relativism is not committed to such a uniform superstructure, for two reasons. First, there need not be anything uniform about how scientific practice becomes successful. Second, there is no reason to believe that scientific practice is especially successful compared with other social practices.

Because monism and pluralism are not strictly contradictory, there is room for further views. In what follows, we will explore one such view, which we might call methodological monism. On this view, there is no uniform superstructure to scientific practice which secures progress. Instead, progress happens through recurring stages with quite variable structures, following nonlinear paths. These stages resemble relativism, pluralism, and monism as we have so far known them. To distinguish them from the philosophical views, we will call the stages relativity, plurality, and unity, respectively. The first stage, relativity, places approaches on equal epistemic footing, since relativistic practices lack standards to decide whether to trust one approach over others. In the second, plurality, approaches become more ordered at a local level, adopting local standards to judge themselves and other approaches, but without a tangible method to judge their own standards.

[2] Kellert, Longino, and Waters 2006, p. x.

In the third, unity, there exist tangible methods to judge between approaches, so that with effort it is possible to plot a trajectory toward the truth.

This view is methodological because it shares the pluralist stance's second core commitment against presupposing that the sciences must take a particular direction. There is no guarantee that scientists will reach the third stage. There is also no guarantee that, once they have reached that stage, they will remain there; this tiered superstructure is nonlinear and variable. Nor does this view make metaphysical assumptions about nature which the sciences must conform to. It does make the following methodological assumption: that scientific practice should orient itself toward the third stage. This assumption comes with an empirically testable commitment: insofar as the sciences have been successful, it is because they have been organised this way. Thus methodological monism also shares the pluralist stance's commitment to empiricism in the philosophy of science, the first core commitment.

Substantiating this view fully would require an epistemology of science. That will not be possible here. Instead, we will discuss two desiderata for such an epistemology, namely two functions which its concept of truth must fulfill. The first function comes from C. S. Peirce, who connected monism to abduction, the logic forming hypotheses and answering questions. Our concept of truth must be such that it motivates answering questions, and does not dismiss any question a priori. The second function comes from Huw Price, who argued that truth creates friction between different ideas, so that interlocutors take differences to motivate disagreement. We may think of the first function as positive, since it is a motivation to create, and the second as negative, since it is a motivation to criticise. If the concept of truth governing an inquiry has these two functions, then questions about different representations and approaches will raise empirical and conceptual problems. By tackling these questions, inquirers can (in principle) move beyond the first stage to the second, beyond the second stage to the third.

The argument will proceed first by considering the monist philosophies of Kitcher and Hull. They are both careful scholars of scientific practice and the history of science, so in that sense they provide a model for methodological monism. But we will also consider Longino's objections to their philosophies. She argues that insofar as they inject an external normative standard to the sciences, they fail to account for the social nature of science. The point here will be to highlight the lessons we must learn from the pluralist stance, and later to differentiate these monist views from methodological monism.

Second, we will revisit the issue of empiricism in the philosophy of science, including the distinction between engaged and detached perspectives. We will hear an important objection, that the pluralist stance, with its detached perspective, does not have to accept monist practices at work in the evolutionary synthesis; it might be better to reject those norms and substitute them for something else. But the problem is not that the pluralist stance adheres to the distinction full stop, but the way the distinction is drawn. They draw it so that philosophical conclusions may be independent of the practical concerns of scientific research. But empiricism in the philosophy of science should mean that the normative standard of progress is inherent to the sciences. It should arise organically from scientific practice, rather than being external to it.

Finally, we will discuss methodological monism itself. First, we will define and explain the two desiderata for an epistemology of science, the two functions which a concept of truth should fulfill. Next, we will define the three stages of inquiry and, in light of the two functions of truth, explain how they form a variable superstructure for scientific practice. We will also appreciate the consequences of failing to meet these desiderata, namely a lapse into relativism, which will tie together the earlier discussion of Longino and Chang's epistemologies.

3.1 Philosophical Precedents and Their Shortcomings

Let us consider two philosophies of science which offer monist theories of progress, from Hull and Kitcher. They share some commitments with the pluralist stance[3] Hull and Kitcher's work is often empirical, drawing on the history and sociology of science and from the sciences themselves. Their work also aims to show that

[3]Though we should remember that these works from 1983 and 1993 predate the pluralist stance, so the latter was influenced by the former.

the social structure of science is constitutive of its success. On both empiricism and sociality, however, Longino argues they fail. Her criticisms are sound, so methodological monism must offer an improvement on this score.

Before going into the details, here is a summary of Hull and Kitcher's views, and Longino's replies. Hull argues that cognitive change by conceptual evolution. This is a social process of selection. The units of selection are concepts, which manifest as beliefs, goals and values. They transmit by speech, writing and observation, which are of course social processes. However, fitness does not normally correspond to truth, and conceptual evolution is not progressive. Truth is therefore not an emergent property of conceptual evolution, but is instead an external normative standard. Science, on Hull's view, is a special environment where conceptual fitness corresponds to this external standard.[4] As for Kitcher, he argues that scientists should adopt methods which approximate an unlimited, ideal agent. Such an agent can simply intuit the truth directly and decide to adopt "adequate concepts and correct schemata."[5] For this to make sense, norms of truth must be entirely independent of social institutions and practices, even if actual research cannot be independent of them. After all, immediate intuition is a private affair. This creates an opposition between scientific practice and the truth, since practice is necessarily social, and its limitations drive us away from the truth. The problem with such views is that, as pluralists argue, they offer unrealistic standards which are divorced from scientific practice. This happens precisely when they break from a commitment to empiricism.

3.1.1 Hull's Conceptual Evolution

David Hull, in *Science as a Process*, gives an evolutionary account of scientific research after a commanding study of twentieth century systematics. Like Longino, he is critical of dichotomies between the rational and social or the internal and external factors in scientific development. Science develops via social processes, and these are brought about by both individual and collective elements. In particular, there is no way to reach scientific objectivity without social institutions.[6] Some

[4] Hull 1988; more specific references will come when we discuss the view later.
[5] Kitcher 1993, p. 188.
[6] Hull 1988, pp. 3-5.

of these social processes might seem counterproductive: "infighting and personal vendettas ... factionalism, social cohesion, and professional interests." This does not mean that the social must be excised, but only that we must take care to learn what a healthy scientific community looks like.

In this, Hull seems close to the pluralist stance. However, unlike the pluralists, Hull believes that general epistemic norms (such as truth, justification, progress) should constrain the norms of local communities (their values, aims and interests). Local norms cannot be primary, so that we rely on them to understand what it means for scientific findings to be true or for a research community to be healthy. "All the time that they spend running experiments and making extensive and careful observations is inexplicable on the assumption that knowledge is in any significant sense socially determined." For Hull, knowledge is socially determined when the ultimate results of research depend on the local, rather than general, interests. Interests are what motivate people to transmit concepts by speech, writing, or observation. Local interests are specific to environment where the research takes place, like a particular community's assumptions or needs. General interests point beyond limitations of the environment. When general interests are given their due place, science is a process which moves us toward greater truth and objectivity, not despite being social, but because of it.[7]

There are two levels to scientific progress for Hull. One, which we might call the higher level, is the actual practice of science in human institutions. The other, lower, level he calls conceptual evolution. It is analogous with biological evolution. Rather than genes, the replicating units are beliefs, goals and values, transmitted by speech, writing and observation. Concepts evolve gradually, spreading or dying according to the environment.[8] To understand what he means by environment, imagine that a time traveller brought quantum mechanics to medieval universities. They would make little progress convincing anybody, since they would lack any of the instruments they need to demonstrate it experimentally, and the concepts used to articulate the theory are far removed from what medieval academics are used to.

[7]Hull 1988, p. 26.

[8]Hull 1988, on replicators, pp. 432-434; on fitness, pp. 457-468.

The mark of science, as an environment, is that it selects for all and only the true concepts. Conceptual evolution is a necessary condition for successful scientific research, but it is not sufficient. Conceptual evolution on its own is only locally progressive, catering to whatever selection pressures (assumptions, needs, and so on) exist in the local environment. So conceptual evolution is not science itself. Science itself happens in a system of social institutions which are oriented toward general interests and to objective progress, where ideas live or die because they are true or false, rather than solely due to local interests.[9]

From this, we can draw a sharp contrast between Hull on the one hand, and Longino and Chang on the other. The latter make scientific truth a function of success at achieving local interests. For Hull, it is just the reverse: judgements of success depend on a prior notion of scientific truth. In the scientific environment, truth and progress are the measure of an idea's fitness, and an idea can only be successful if it is fit. If a scientific belief is false, or a goal or value misleading, then its conceptual foundations will erode when it interacts with other (scientific) conceptual systems, or it will fail experimentally. Hull's theory relies on empirical support; it stands or falls with its historical analysis and with how well it fits scientific practice. If the best science we have does not mirror the monist ideals of Hull's theory, then his theory fails.

Longino briefly criticises Hull on empirical and theoretical grounds.[10] There is a tension, she argues, between the nonepistemic processes of conceptual evolution and the epistemic successes of the sciences. Hull assumes that the sciences will elevate conceptual evolution by creating an environment where it can produce progressively truer explanations. The difference is supposedly that, in science, there is an objective goal, truth, by which all progress can be measured, whereas in other settings, concepts are left adrift in whatever local interests happen to surround them.

The problem is that Hull never explains how we get progress from what is, at bottom, an "anti-intentional selectionist framework." What she means is that selection processes have nothing to do with truth; as Hull admits, the scientific

[9] Hull 1988, pp. 472-474.
[10] All quotes by her on Hull are from Longino 2002, p. 53, including n. 22.

environment is special in that it manages to transcend the local concerns which normally dominate selection. But to admit this is to admit that there is no evolutionary explanation for how science progresses. Instead, an epistemology is brought from outside to create an environment with the right sort of teleology. Moreover, Hull underspecifies the teleological structure of science. "For simple selection to work the selective environment would have to include the goal of uncovering laws of nature either as a feature of the scientific community, or of the society that supports the activity of scientists. But then, why laws of nature? Why not pragmatically useful models, and why only one goal?" The point about Hull's view is well taken; he wants to be a monist, with truth as an external objective standard regulating scientific research, but his theory struggles to rule out the pluralist stance except by fiat.

3.1.2 Kitcher's Theory of Limited Rationality

Philip Kitcher's offers a theory of progress and rationality in *The Advancement of Science*. He tries to account for humanity's limited rationality by comparing it to an ideal, unlimited rationality. He offers ways to approximate that ideal, which is the method by which science makes progress. Kitcher is dissatisfied with the same dichotomies as Hull and Longino; he stresses that both individual and social cognition drive the sciences, and that social values and goals are key to scientific progress. Like Chang, he also stresses the importance of concrete practices. These include "consensus practices," which do not differ between individuals in the same community and so are not part of any ingroup conflicts.[11]

He draws on two forms of empirical support. The first is the history of Darwin's evolutionary research, as well as its growth, postpublication, into the field of morphology. The second is an economic analysis of scientific institutions and decisionmaking.[12] The historical case study offers examples of progress (and failure), while the economic analysis offers a framework which makes tangible predictions.

[11] He gives an extensive discussion of these issues; Kitcher 1993, pp. 58-89.

[12] The historical account is in Kitcher 1993, pp. 11-57, with further details in pp. 263-290. The economic analysis is in pp. 303-398.

For Kitcher, significant truth, as opposed to an accumulation of routine truths, is the measure of progress. But he analyses progress into two kinds, conceptual and explanatory, which have different but complementary ends. Scientists should strive for a convergence in conceptual language: convergence both toward distinct natural kinds (as referents) and toward a common language rather than claves of unrelated concepts. Explanatory progress means correcting previous falsehoods and covering more ground (becoming more complete) than previous explanations.[13]

It is worth explaining how these two forms of progress are complementary. Earlier, we noted that Kitcher believes that scientific theories covering the same domain should be intertranslatable. We can now see that, for Kitcher, belief in intertranslatability amounts to a belief in progress. If two scientific practices were totally incommensurable, they would be incapable of conceptual progress by definition. And a practice incapable of conceptual progress would face barriers to explanatory progress: where it cannot expand its vocabulary, it cannot correct falsehood or become more complete. A similar argument would show that barriers to a theory's explanatory progress would impede its conceptual progress.

As with Hull, we can read Kitcher as distinguishing local and general interests. Human beings are limited beings, so we "suffer a contrast between rational strategies and goal-attaining strategies." Whereas an unlimited being would "adopt adequate concepts and correct schemata, pose significant questions, [and] accept true answers," limited human beings cannot just decide to do any of that. Unlimited beings can reason directly toward the structure of nature, immediately intuiting adequate concepts. Limited beings begin with limited resources, flawed background beliefs, and a set of nonepistemic goals and needs which weigh on their epistemic decisions. These limitations make a human being's starting point imperfect. When human beings pursue truth through scientific research, they need to use proxies for correctness: heuristics like criteria of significance or particular research methods. Scientists make progress by moving beyond flawed proxies and adopting better ones.[14]

[13] His account of progress is in Kitcher 1993, pp. 90-112.
[14] Kitcher 1993, pp. 188-192.

This is a social process, which takes place in human institutions. This social process works well if our decisionmaking follows what Kitcher calls the External Standard. The External Standard is itself a methodological principle. It demands that we only adopt a new scientific practice if it objectively improves upon its predecessor and is better than its rivals, regardless of the initial practices and available empirical information. This is a rigorous standard; human practices cannot hope to be optimal, as Kitcher admits, but it is possible to weaken the conditions of the External Standard to measure approximate rationality. Approximation is the best that the social process of human research can do. The perfect strategies of unlimited beings function as an ideal, while scientific practice approaches it in a convoluted way.[15]

Longino also criticises Kitcher, targeting the External Standard and the empirical grounds for his view. The External Standard commits him to a dichotomy between the rational and the social. We approximate the result of unlimited beings only when we overcome, gradually, the shortcomings of our starting point. On this view, nonepistemic goals introduce distortions and barriers which we must overcome to pursue the truth. "Because the social (as bias) is represented as working against the true, and the true is identified with nature, any constructive role that the social might play is eclipsed." This leaves us with a "Manichean representation" of the social and the rational, where the rational is a perfect order which we can only hope to reach by transcending our material limits. Thus, like Hull, it is unclear how Kitcher's normative commitments follow from his social process of scientific change through growing consensus practices. Looking solely at the analysis of practices, "why can we not also see the social as facilitating the selective absorption into consensus practice of some (nature-induced) differences in individual practices over others?"[16] He can rule such pluralist moves out only after imposing the External Standard. Since he imposes it on his theory, rather than inferring it from his history or analysis, it does not have an empirical grounding.

[15]Again, Kitcher 1993, pp. 188-192. He discusses in quite some detail what optimal means, and how to weaken the standard to make it more sensitive to local resources, but we can forgo those details here.

[16]The quotes are from Longino 2002, p. 59, but her discussion of these themes begins on p. 54.

It is not just Kitcher's standard of progress which fails, so Longino argues, to have an empirical basis; even if he dropped the External Standard, the very idea that science makes cognitive progress also fails in this way. Progress means emulating, or at lest approximating, the strategies of unlimited beings. But Kitcher's description of those strategies presupposes that nature can be captured by a unified set of adequate concepts. "Monism, then, is a given. But the belief in monism is not grounded in any empirical data," it is an assumption imposed from above. The empirical data, Kitcher's history and economic analysis, do not rule out pluralism; communities could simply tolerate multiple approaches. And even if some "communities tend to converge to one theory or approach," it is possible that we have gained genuine knowledge only about "aspects of the phenomena that are of interest to us."[17]

Here then are two views, from Hull and Kitcher. They offer a model for methodological monism, but they also stand as warnings, thanks to Longino's criticisms, about what to avoid. The dichotomy between the rational and social is the basis for their monist principles, even though they wanted those principles to follow from their empirical analysis. Despite their care with the empirical data, Hull and Kitcher have created uniform monist superstructures, which have difficulty accounting for the social realities of scientific practice.

3.2 Sensitivity to History and Practice

There are two ways to be sensitive to the history and practice science. One way is to do scientific research, to inhabit its practices and become part of its history by moving it along. Another way is to reflect on scientific research, not as a practitioner, but as a third party trying to understand it. This is the distinction between the engaged and detached perspectives, with which we already became familiar.[18] As a tool to understand the difference between practicing and reflecting on something, the distinction is useful. But it is misleading insofar as it uses boundaries between disciplines to enforce boundaries between types of questions.

[17] Longino 2002, pp. 64-67.

[18] As a reminder, this usage follows Kusch 2017, who adapted the distinction to epistemology from its use in ethics by Wong 2006.

The distinction reflects a difference between the roles of the scientist and the philosopher of science, but this difference is historically contingent. The roles of scientist and philosopher are different because of features of contemporary academia, namely specialisation and disciplinary boundaries. These institutional features did not always exist, as a look at ancient philosophy will attest. When they have existed, they have not always taken the same form, as a look at Early Modern natural philosophy will attest. We therefore have to be careful, when we use this distinction, not to reify contingent features of contemporary academia.

Contemporary scientists pose and answer philosophical questions in their research. These questions can arise, for example, when scientists are faced with new instruments, techniques, or theoretical proposals. They have to decide what these new instruments and techniques show, or what the empirical content of these theoretical proposals is. Those questions are adjacent to questions about the nature of observation and measurement, about empiricism and reason, about methodology and evidence; scientists do sometimes attend to these philosophical questions.

The difference between the scientist's and the philosopher's philosophical questioning is its practical stake. The scientist's philosophical questioning arises from their research, and bears on how they will continue that research.[19] The philosopher, however, stands as a third party. They review the results and practices of the sciences, but their answers do not bear on any scientific practice. This is true even if they intend their philosophy to offer advice or to say how the sciences ought to be. The questions are not part of a living scientific practice.[20]

Chang's active pluralism might seem to break this distinction. He calls for philosophers and historians to practice complementary science, the recreation of historical scientific practices which have been abandoned. However, even this respects the distinction.[21] The point of complementary science is twofold: first, to give evidence of historical contingency and plurality in the sciences, and second,

[19]There is no issue interpreting "their research" as a community rather than personal project. In fact, this would be more accurate.

[20]It is also worth noting that the same person can occupy these perspectives on science at different times. Kuhn worked as a physicist before he switched to history and philosophy; his historical and philosophical questions did not bear on any active physical research agenda.

[21]Chang 2012, pp. 288-289.

to advise scientists to adopt or adapt old approaches in contemporary practices. The recreations are complementary precisely because they give a view outside current scientific practice, in order to analyse and advise the sciences as a third party. Complementary science encourages interdisciplinary collaboration, but it still recognises and respects the boundaries of specialisation. The thought is just that scientific disciplines, if they are to push the boundaries of existing practice and pluralise, need input from outsiders.

This distinction can form the basis of an objection to the empirical argument about the evolutionary synthesis. The argument was that the pluralist stance did not account for the monist practices of discussion and criticism. Those practices were important not just to the Fisher-Wright debate or to Dobzhansky's work, but also to the historical development of evolutionary biology. As for the objection, Waters invokes it in his comments about gene-centered biology. "Readers might complain that I've been too generous to Morgan and subsequent gene-centrists. After all, Morgan was not tolerant of diversity. [...] But that doesn't mean *we*, as philosophers or historians, shouldn't adopt the pluralist stance when we try to understand ... their science."[22] And we have noted that Longino, Chang, and Ruphy make similar invocations in their case studies or examples.

It must be allowed that scientists can have intellectual prejudices and mistaken standards, even over long stretches of time. It is difficult to disagree with this. The only sensible reply is that the practices in question are not prejudices, but integral parts of successful scientific practice. Shortly, when we turn to methodological monism, we will hear a case for this. More generally, the objection is this: scientific practice is not synonymous with what scientists do and believe. Scientific practice is the structure which organises research toward an objective understanding of the world. The pluralist stance's contention is that this structure is plural rather than unified; local interests, diverging values, and separate approaches are what give us an objective understanding. Insofar as Morgan's research was successful, it is because he participated in such a pluralist structure, whatever else he may have said or thought.

[22]Waters 2006, pp. 210-211.

But this objection reifies disciplinary boundaries; it detaches philosophical reflection on science from the practical reasoning of scientists. This is a problem even from the pluralist stance's own point of view. An epistemology of science, as Longino has argued, has to show how the social practice of fixing belief gives warrant to those beliefs. Practices of fixing belief include how ideas are communicated, challenged, and applied to further research. So if Morgan, or Dobzhansky, or Wright, or Fisher, or whoever else was intolerant of the views they were arguing against, and if such intolerant processes helped fix beliefs, then we have two options. Either their epistemic communities were unhealthy and not conducive to conformation and truth, or the intolerant processes help create an objective understanding of the world.

Adherents of the pluralist stance have fallen short in their commitment to empiricism in the philosophy of science. This has a clear source: detaching philosophical questions about the sciences from the engaged practice of the sciences. This is not to say that philosophers must be scientists; the problem is not that philosophers, as people with a detached perspective, are asking questions about the sciences. The problem is that the questions themselves are taken to be separate. Judgements about success should arise from scientific practice. This claim might sound familiar, since Chang founds his epistemology of science on an analysis of scientific practice which takes success (and truth) to be inherent to practices. Like Longino, then, his view also has resources to see the problem with the pluralist stance's detached structure and external standard.

3.2.1 Internal Standards for Scientific Practice

Pluralists, like the monists they criticise, impose an external standard of success on the sciences. In doing so, they prioritise the third core commitment of the pluralist stance over the first two. That is, they give precedence to the pluralist structure of science over an empirical philosophical method, and over the need to avoid determining the end result of research.

Consider their approach to the second commitment, that the philosophy of science should not determine the end result of research. Kellert, Longino and Waters have said that this implies that the epistemology of science cannot bias our empirical analysis in favour of eventual monism or pluralism. Ultimately, whatever happens in the sciences will depend on scientific practice, not on philosophical assumptions.[23] For Chang, it means that philosophers and scientists should "let a hundred flowers bloom" in the sciences, not just tolerating but actively encouraging many scientific approaches to see where success may take hold.[24] Monists (like Hull and Kitcher), when interpreting scientific practice, have imposed external standards which make pluralism seem impossible in the long run, even if they allow some sort of pluralism locally and in the short run. Whatever philosophers make of the current state of the sciences, they should leave their interpretations open to either outcome.

The point, then, is to give priority to scientific practice to determine its own ends. However, as we have seen, pluralists have not avoided imposing external standards of their own. The empirical evidence for pluralism tends to be that there are often many approaches to a given scientific question, and that it is easy to go astray interpreting a scientific domain as having only one theory, or one concept, or one standard. This evidence itself is not problematic. A problem arises when pluralists then argue that the evidence points to a deep pluralist structure rather than a surface structure. The empirical evidence itself is mixed on this score, since pluralists are forced to acknowledge and explain monist structures.

So let us reflect on what kind of evidence would help us identify an internal, rather than external, standard of scientific practice. First of all, any standard of success presupposes an aim. Pluralists have argued, correctly, that scientists have many aims in mind when they do their research. Of course, not all of these are epistemic. A given scientist might do their research for honour, money, friendship, amusement, and so on. But the actual operations and activities of science create representations; that is, they create theories, or models, or descriptions, or methods of measurement, and so on. Scientific practices are composed of what Chang calls epistemic activities. What scientists create when they engage in epistemic activities has many possible uses, but these uses all depend on successfully completing the activity. The aim of an epistemic activity, what makes it count as

[23]Longino 2002, p. 140 and Kellert, Longino, and Waters 2006, p. xxvi.
[24]Chang 2012, p. xx.

completed, is the creation of a good representation; good, because it gives us a better understanding of the target. Other aims are external insofar as the epistemic activity can be completed successfully without engaging in those other aims.[25] It is no accident, then, that Longino and Chang take pains to place conformation and truth, respectively, at the centre of their epistemologies. What makes a representation good is that it conforms or is true.

Whether a practice is judged successful from an engaged or detached perspective, the judgement has to rely on empirical evidence. To be relevant, this empirical evidence has to connect the practitioner's attempts to an internal aim. This is true of any practice, not just scientific practices. Suppose someone says that a musician has made progress learning a piece of music. The musician solved technical and interpretive problems which had challenged them, allowing them to play the music more fluently and compellingly. Both the prior conditions (their difficulties) and the new conditions (their more convincing performance), along with the process leading to that change, are relevant empirical evidence. Without such information, we could not say whether they succeeded, or failed, or progressed, or regressed at anything. But equally, this information needs to presuppose and point toward an internal aim of the practice, such as fluent and compelling playing.

Pluralist arguments, insofar as they aim to convince monists, rely on a shared conviction that modern science is successful. By emphasising that shared conviction, pluralists seek to show that there is another way (besides monism) to defend the sciences from relativism, and that another way is necessary, since monist's standards are unrealistic. That is the point behind Waters' argument against a monist interpretation of genetic science, for example. He argues that genetic science is successful and the gene-centered approach viable, which a monist reader would find sympathetic. But he also argues that a monist interpretation leads to genetic determinism, which is untenable because it is beset with too many scientific and philosophical problems. To stick to this untenable view would undermine the success of the gene-centered approach. Insofar as Waters is correct that genetic determinism is untenable, the only remaining options are pluralism or relativism,

[25]This distinction between internal and external aims is related to two distinctions in ethics: internal and external goods, and internal and external reasons. See MacIntyre 1984, p. 188ff. and Williams 1981, p.101ff.

and only pluralism preserves a belief that the gene-centered approach is successful because of its epistemic status.[26]

Just as judgements about practices rely on empirical information and identifying an internal aim, so in particular does an epistemology of science. It relies on empirical information insofar as we need historical and social knowledge about scientific practice to properly define what our epistemology is about. And it relies on identifying an internal aim, because an epistemology does more than just describe what scientists do, it also says what they should do and, to an extent, how they should do it. If an epistemology of science judged this by an external aim, then it would not really be an epistemology of science, which is principally about how scientific practices produce good representations.

Recall how Longino and Chang framed the internal aims of scientific practice. A cornerstone of Longino's epistemology is accommodating different representations: scientific representations are partial and selective, houses within distinct epistemic communities with their own local epistemologies. Local epistemologies exist because what it means to conform cannot be the same across approaches.[27] For Chang, practical reasoning, which is about the achievement of an activity's ends, is prior to theoretical reasoning about what is true. A system of practice is adopted on the basis of what scientists can do with it, which is itself based on what they want to do. But what scientists want to do can vary drastically, so our epistemology must accommodate a plurality of systems of practice.[28] But they could not accommodate this plurality while maintaining a position against relativism.

Pluralists are in the same rhetorical bind which they had hoped to put monists into. Either their pluralisation of the internal aims of science is misguided, or scientific practice is relativistic. However, as we have observed earlier, there are more options than the three framed by the pluralist stance. It would be naive, then, to turn back to monism as articulated in the editors' definition, or as present in Kitcher and Hull. It is time to articulate a different option, methodological

[26]Waters 2006, p. 191
[27]Longino 2002, pp. 176-177.
[28]Chang 2012, pp. 231-233.

monism, which learns from scientific pluralism's insights while avoiding its rhetorical bind.

3.3 The Variable Structure of Scientific Practice

As far as we have seen up to now, scientific monism attributes a uniform epistemic structure to the sciences. And as we have discovered, the pluralist stance does the same. Our discussions of the pluralist stance's theoretical and empirical problems brought this to light. Pluralist epistemologies have failed to articulate a clear distinction between different and conflicting representations, because they attributed the plurality at work in scientific practice to the structure of warrant and truth. And pluralist studies of scientific practice have dismissed monist practices which lead to unnecessary tension and friction between approaches; unnecessary, that is, according to their pluralist epistemological views.

What we need to do instead is account equally for practices which evidence plurality and which evidence unity. This would commit us to empiricism in the philosophy of science, and to an open trajectory for scientific practice, that is, to the pluralist stance's first and second core commitments. To do so, we will also reject the third core commitment, that plurality is constitutive of success in the sciences.

Instead, methodological monism claims the following. First, truth is the internal aim of scientific practices, insofar as they are epistemic practices. Though we will not develop an epistemology of science here, we will give two desiderata which would help it avoid the pluralist stance's problems. Truth in such an epistemology should have two functions: motivating the formation of questions and hypotheses, and motivating disagreement between different approaches. These functions are due to Peirce and Huw, respectively.

Second, methodological monism claims that there is no particular structure to scientific practice which pervades its entire history. Scientific practice, despite having a uniform aim, has a variable structure, varying according to how well it can fulfill its aim. We may articulate these as three stages: relativity, plurality, and unity. The first stage, relativity, places approaches on equal epistemic footing, since relativistic practices lack standards to decide whether to trust one approach over others. In the second, plurality, approaches become more ordered at a local level, adopting local standards to judge themselves and other approaches, but without a tangible method to judge their own standards. In the third, unity, there exist tangible methods to judge between approaches, so that with effort it is possible to plot a trajectory toward the truth. Although there is a hierarchy between these stages, they do not have to occur in a strictly linear order. There can be failure, regress, and stagnation, so that the ultimate trajectory of scientific practice is uncertain.

3.3.1 Two Desiderata for Truth

The internal aim of science practices, insofar as they are epistemic, is truth. Longino and Chang's epistemologies acknowledged this, even if they took it in a very different direction so that they could accommodate a pervasive plurality. Their concepts of truth were relative to social units, to communities or systems of practice and their values, aims and interests. To avoid epistemic relativism, we must first of all have a uniform concept of truth which articulates the aim which scientific practices have in common, as epistemic practices. Differences between approaches should give rise to reasonable questions, even if the approaches in question are in a state of relativity or plurality. Likewise, the differences between these approaches should cause friction, motivating disagreement. These are two functions for the concept of truth, a positive and a negative function, respectively. The positive function, due to C. S. Peirce, is enabling hypotheses: our epistemology must motivate hypothesis formation, the practice of generating serious candidate answers to a question. The negative function, due to Huw Price, is disagreement: truth must motivate criticism between two different positions on the same question.

In his 1898 Cambridge Conference lectures, C. S. Peirce argued that "the logic of retroduction directs us to adopt Monism as a provisional hypothesis of philosophy, whether we think it or not; and not to abandon it until the position is stormed and we are forced out of it."[29] The logic of retroduction is more commonly known as abduction. Abduction is a distinct type of reasoning by which we form hypotheses, which we may think of as educated guesses or serious candidate answers to a question. Forming hypotheses is the first inferential step in any inquiry. It reasons beyond the limits of our current knowledge, so that we can then analyse and test this provisional idea to learn something new. Thus, without a willingness to form new hypotheses, we have no way to make progress in scientific reasoning. To be unwilling to form hypotheses about a phenomenon is to be unwilling to explain it.

This is the positive function which truth should fulfill: to motivate hypothesis formation, not just where it is obviously fruitful, but in general. To adopt monism as a provisional hypothesis requires that we assume different approaches are connected because their phenomena are connected, even if we do not know how. This just the opposite of what the pluralist stance urges, which is to assume that different approaches are separate, until such a time that there is a clear connection between them. This difference in assumptions is crucial. If we adopt monism, then we have some reason to construct common standards and methods of comparison between approaches, even without any positive reason to believe that one must exist. The monist practices of the evolutionary synthesis would be judged rational in that light, even during the most plural episodes of that history.

However, simply motivating hypothesis formation is not enough on its own. The positive function ensures that we assume different approaches are connected, but more needs to be specified about how approaches interact. The negative function of truth is therefore about disagreement. Huw Price develops this idea while arguing against Rortian relativism, according to which communal norms of justification can replace the notion of truth. Price argues that a truth norm is necessary over and above a community's justificatory norms. "There is an important and widespread behavioral pattern that depends on the fact that speakers do take themselves to be subject to such an additional norm [to justification]," namely "conversation itself." "Conversation" is a term borrowed from Rorty, Price's main interlocutor. This sense of conversation has a much broader meaning than we need, but it can be restricted easily enough to a scientific context, where scientists communicate

[29]Peirce 1898, pp. 203-204.

about the scientific practices they are engaged in. Conversation depends on a truth norm, Price argues, because truth "gives disagreement its immediate normative character, a character on which dialogue depends, and a character which no lesser norm could provide."[30]

Neither Longino's conformation nor Chang's internal truth fulfill these functions. There is no friction between different approaches or representations just because they are different. Within a community or system of practice, there might be friction between representations which contradict each other, or contravene some other local norm. But between communities and systems, their epistemologies of science discourage friction due to differences. The aim of their epistemologies is to motivate the continual existence of different approaches without epistemic conflict.

When their epistemologies were applied to the evolutionary synthesis, they struggled to explain the debate between Fisher and Wright, who seemed arbitrarily partisan rather than normal, rational scientific actors. The problem, from the pluralist point of view, was that Fisher and Wright acted as though their approaches conflicted, even though they really just parsed causes differently and emphasised different operations. They had no positive reason to believe that these differences caused a conflict. This is true enough; however, they did not need a positive reason. It was enough that they noted the differences and that their approaches tried to explain some of the same phenomena. It was then reasonable, according to a truth norm fulfilling both functions, to test these approaches against each other, to see which was more viable (if either was), to hypothesise about their connections and divergences.

There is an important objection. Perhaps the pluralist stance only downplays conflict in areas where, to borrow Peirce's phrase, monism's position has already been stormed. If there is no positive evidence of monism in a particular domain, despite scientist's persistent monist practices, then downplaying conflict cannot be problematic. However, there are two problems with this objection. First, whatever the trend in some area of science may be today, scientists have to make

[30]Price 2003, p. 168. Though Price also argues against Peirce's theory of truth as a "limit of inquiry" in this essay, Atkin 2015 points out that Price's convenient friction is a Peircian idea, quite compatible with Peirce's ideas about truth on a more careful reading of the primary texts.

a decision about how to proceed. And there is a practical difference between methodological monism and the pluralist stance. According to the pluralist stance, there is no reason to think that a conflict exists unless there is a clear reason to indicate that some differences are problematic. Differences between approaches are never problematic as such.[31] Well, then there is no reason to investigate these differences, except insofar as they advance the existing aims of an approach. Whereas, following methodological monism, the point is always to go beyond the limits of current approaches, even if we cannot really imagine how that will happen. Given that Longino and Chang emphasised the need for communities and systems to go beyond their limits, they should agree that demotivating conflict between approaches is a problem.[32]

Second, this objection forgets the theoretical problem with the pluralist stance, that it has not managed to avoid relativism. Methodological monism does avoid relativism, even in cases where the state of scientific practice is relativistic. For objection to have force, a pluralist solution to relativism would have to be found. To substantiate this, we should turn now to the three stages of scientific inquiry.

3.3.2 Relativity and Plurality in Inquiry

If the presumption is that differences imply a conflict, absent a positive reason otherwise, then scientific practice cannot have a uniformly pluralist structure. Science is successful, on Longino and Chang's views, because different, equally legitimate approaches can illuminate a phenomenon without conflicting with each other. If scientists assumed that a conflict existed, they would be mistaken; if they worked to resolve this nonexistent conflict, they would risk burying knowledge and causing regress. Chang argues as much about the chemical revolution, when he suggests that chemists would have discovered chemical potential energy much earlier if the phlogiston theory had continued alongside the caloric theory.[33]

Using evidence like this, pluralists argue that unifying efforts are unnecessary. The point of science is to rationally address local aims and interests. If unification

[31] Kellert, Longino, and Waters 2006, p. xxv.
[32] Longino 2002, pp. 128-131 and Chang 2012, pp. 268-284.
[33] Chang 2012, pp. 42-47, and 62-65.

does not have some concrete connection to those aims and interests, then questions about unification must come from some external standard outside these particular communities. Inquiry cannot be governed by an alien external standard, since what really drives scientific progress are those local aims and interests. In the face of an external standard, we can always ask, as Longino asked Kitcher and Hull, why we should not allow plurality rather than insist on unity.

By contrast, for an epistemology which adheres to the two desiderata, it is natural to pursue unifying efforts. If two approaches to the same or similar phenomena differ in some way, whether in their answers to the same questions or in asking different questions, this difference can be interrogated. This alone is sufficient to justify an investigation, though there might be some practical reason why it cannot or will not happen.[34] On this view, it makes sense that Fisher and Wright would be animated by their different theories of evolution, or that so many scientists would care about resolving the debate between naturalists and experimentalists. Even when such views are walled off from each other institutionally, they are still relevant to each other because of their partially shared subject matter and their different answers. And this epistemic relevance is not cancelled even if there are practical reasons why scientists cannot address it at the moment.

So far, however, there is nothing separating methodological monism from the other monist views, against which pluralists had convincing arguments. What distinguishes methodological monism is that, unlike even the empirically sophisticated monisms of Hull and Kitcher, it attributes a variable structure to scientific practice rather than a uniform one. Longino asked why we should not allow plurality rather than insist on unity. The answer is that we can, in a sense, do both at once. We can allow that plurality is a real feature of some scientific practices, while insisting that the aim of scientific practice is a uniform notion of truth.

This requires a different treatment of epistemic values. Longino and Chang described a range of epistemic values (simplicity, elegance, accuracy, consistency,

[34]Practical constraints include feasibility given available resources, urgency compared to other needs, opportunity costs, and so on. Peirce discussed such constraints in his writings on the 'economy of research'. See Bergman 2018, p. 275ff. Kitcher also accounted for such factors in Kitcher 1993, Ch. 8.

model fit, and so on) which are independent of each other, which differing, equally legitimate interpretations across scientific communities, and which pull scientific practice in different directions. But if all scientific practices share a common notion of truth, according to which differences cause some friction and motivate investigation, then scientific practice must presuppose that epistemic values are ordered toward the truth. The actual conditions pluralists describe might hold; in some given situation, for example, simplicity might pull against model fit; but insofar as scientific practice is organised toward truth, these conditions will elicit friction and questions, so that, if possible, the bifurcation values and approaches can be resolved.

The conditions of scientific practice may be of three broad kinds: relativity, plurality, and unity.[35] Let us begin by considering relativity, where scientific practice places each approach on equal epistemic footing because scientists lack epistemic standards to judge between them.

Stages in inquiry is not a new idea, of course. It occurs in the thought of many philosophers over millennia. The direct inspiration for treating relativity such a stage is somewhat newer, arriving with the advent of systematic social science around the turn of the twentieth century.[36] Alasdair MacIntyre argues for treating relativity this way in his 1984 APA Presidential address. "[R]elativism, like scepticism, is one of those doctrines that have by now been refuted a number of times too often. Nothing is perhaps a surer sign that a doctrine embodies some not to be neglected truth than that in the course of the history of philosophy it should have been refuted again and again. Genuinely refutable doctrines only need to be refuted once." He goes on to clarify: relativism is true of "a moment in the development of thought which has to be, if possible, transcended; and this even although we may as yet lack adequate grounds for believing ourselves able to

[35]Broad, since it is possible to make finer distinctions, or for these conditions to overlap by applying to some aspects of practice but not others. Dividing it into three is convenient for purposes of discussion, because doing so captures the range of uniform structures we have considered, namely relativism, pluralism, and monism.

[36]This is the principal reason why philosophers of science cite sociologists when they discuss the issue of relativism.

transcend them."[37]

There is a common thread between MacIntyre, who says we must transcend relativist conditions "even although we may as yet lack adequate grounds for believing ourselves able to transcend them" and Peirce, who says we must provisionally adopt monism, "whether we think it or not." The common thread is that epistemic practices aim at a uniform aim, the truth; and also that there is no guarantee that they will fulfill that aim.

Relativity and plurality share a core element: in Longino's words, there is no shared context between representations or practices. A shared context would consist, for example, of a common language in which both approaches may be expressed and compared in a coherent way; Longino points out that culture is often a source of plurality, due to barriers like language and geography which inhibit transmission and uniformity.[38] A shared context would also consist of common assumptions and aims. As Chang argues, the divide between phlogistonists and oxygenists in the chemical revolution was partly caused by holding different "problem fields." Each side expected scientific theories to give different kinds of explanation which prioritised solving different scientific problems; moreover, there was no clear way to address all of those problems at once.[39]

What distinguishes relativity from plurality is what kinds of contexts are lacking. Plurality is marked by there being some shared contexts: there are clusters of local epistemic communities, of coherent systems of practice. Within these contexts, there can be direct comparisons; friction between different representations comes easily and there are ways to investigate and resolve it. Relativity lacks even this kind of shared context, so that there is no epistemic friction between representations. There may be highly organised social forms and practices, but they do not support the systematic inquiry which makes up successful scientific practice, with the internal aim of seeking the truth.

[37]MacIntyre 1985, p. 5. He goes on to summarise and critique a few of the most common arguments. Martin Kusch, in Kusch 2002, p. 269ff., likewise discusses flaws in some of those same arguments and a few others, in the context of the philosophy of science.
[38]Longino 2002, p. 175.
[39]Chang 2012, pp. 19-22.

To acknowledge relativity in scientific practice is not to be a relativist. A relativist believes that relativity is pervasive, that it is the overarching structure of scientific inquiry. But we do not have to accept its permanence; it is contingent and temporary. Likewise, we can acknowledge plurality in scientific practice without being a pluralist. Plurality belongs to a stage of inquiry where different traditions of thought engage without any neutral ground: for example, without a third language into which each tradition can accurately translate its ideas, or without a commensurable standard for measurements and mathematical models.[40] Such a situation means, in Martin Kusch's terms, that an appraisal between the two is impossible, and that switching between the two requires a sudden conversion.[41] In converting, we simply adopt new aims, according to an external reason which has nothing necessarily to do with the internal standards of scientific practice. On this picture, where following approach amounts to a personal decision, it is easy to sympathise with the pluralists' call to allow different approaches to coexist.

But it is possible to imagine the resources which would allow us to compare approaches according to internal standards. Even when we lack a common language, or commensurable measurements and models, or common standards for what a theory should explain, it is possible to imagine having them. Having imagined them, we can then commit ourselves to constructing them. Insofar as this is allowed by some local scientific practice, that practice conforms to the positive function of truth; and insofar as the gaps between approaches causes friction, they conform to the negative function.

The path to unity, where tangible methods of comparison are available, follows what Chang calls "constructive ascent" and "epistemic iteration," inspired by Peirce.[42] His most detailed example is creating temperature scales, but it is generalisable. We begin with a rough but accessible hypothesis, regardless of the fact that it is likely wrong. We then iterate upon it, testing it and also testing the standards by which we judge its success or failure, ascending until we have come

[40]The first example is MacIntyre's.

[41]Kusch 2020, pp. 3-4.

[42]Chang 2004, p. 44ff for the former, 224ff for the latter. Something very like it is also in Larry Laudan's *Progress and Its Problems*, from which Ernst Mayr had drawn the concept of research traditions.

to a more stable and reliable view. It is a solution to the "experimenter's regress," introduced by the sociologist (and relativist) H. M. Collins, where experimentalists know that they have set up their equipment correctly when it shows the correct readings, but they can only know what the correct readings should be when they have determined what the correct setup should be.[43] These constructions might fail. They might also succeed for a time, but become problematic later. So they are not guarantees of progress in any sense, and what progress there is need not be cumulative or linear. But there are ways to try at progress, and there are ways to judge the attempts by standards internal to scientific practice.

Longino and Chang's epistemologies therefore already have some resources to support creating new common standards, and to explain how they can be constructed.[44] But their assumption that plurality is a uniform structure undermines their efforts to explain how relativity can be overcome; and it prevents them from seeing that we can overcome plurality by using those same resources.

There is an important objection to consider: if we accept methodological monism, then it seems that there is no point where it is permissible, according to internal standards, to abandon a problem altogether. There are always more tests to try, more hypotheses to test. But at a certain point, should we not admit that there is no fact of the matter?

There are two equally valid answers to this challenge, when to admit that there is no fact of the matter about a question. The first is practical: we say that there is no fact of the matter because we see that, with existing resources, it seems impossible to answer. If we cannot form a concrete plan to answer a question, we are forced to set it aside. Practically, this means treat it as though there were no fact of the matter. The second is logical: we say that there is no fact of the matter because we have some positive reason that the question was badly formed. Perhaps its presuppositions are contradictory or false, so that the question must be modified or rejected.

[43] Collins 1985, pp. 83-84. The experimenter's regress is also discussed, in connection with Chang, in Kusch 2015.

[44] Longino 2002, pp. 140-143 and Chang 2012, pp. 280-283.

Ultimately, however, the premise of this objection cannot be refuted. There are clear reasons to set a question aside locally and for the moment, but not forever. The only answer left to give, if the practical and logical considerations are not satisfying, is that no problem arises from questions remaining technically unsettled. To believe that is just to be a fallibilist, to hold that we may always be mistaken about our current commitments and decisions. A question which seems impossible to answer now, may receive methods for its solution later. At one point, measuring the temperature of the sun's core was impossible. Once we developed temperature scales, it was conceivable but no known method could even approach the problem. It was only after physicists developed theories of radiation and sophisticated measuring equipment that it became a tractable open problem.[45] Likewise, the question about how to unify continuous and discontinuous accounts of variation in biology seemed logically problematic, until a framework was constructed to make it a live and fruitful question. There is no harm in admitting that fanciful, impossible questions might become ripe for research later, given that there are clear local reasons to set a question aside for the moment.

The structure of scientific practice is variable. What we take to be true will vary, of course, and so will which questions are tractable. But more importantly, the internal standards by which scientists compare representations or approaches will vary as well. Those standards will vary by which kinds of representations or approaches they can submit to scrutiny. Under relativity, there is equal epistemic footing because there are no clear internal standards at all. Under plurality, there are local pockets of comparable approaches, but the differences between approaches render them incomparable due to a lack of shared context. When those shared contexts are constructed, so that comparisons can be made between approaches and not just within one, we begin to see unity.

What remains uniform is the internal aim of scientific practice, which is to create true representations. This same concept of truth motivates us to remain open to new questions and to revisiting old one, and to investigating the differences

[45] The example comes from Reichenbach 1938, pp. 47-48.

between different approaches. As Chang says, "Science is an inherently *progressivist* enterprise, which always strives to improve things, even if it should fail."[46] The variable structure of scientific practice accounts for the fact that plurality is common in science, that debates sometimes seem intractable. But it also accounts for the monist practices which persist even in these episodes, without dismissing them as structurally unimportant. Both commitments are integral to methodological monism. If we drop either one, the variable structure of scientific practice or the internal aim of truth, then we are left with a choice between three uniform structures: relativism, which rejects an epistemic standard for success, pluralism, which suffers from both theoretical and empirical problems, and the monism criticised by the editors, which externalises the epistemology of science from scientific practice.

[46] Chang 2012, p. 258.

Conclusion

The pluralist stance has faced two problems. The first, a theoretical problem, is to find a clear distinction between allowable difference and conflict so that they could distinguish pluralism from relativism. Thus Giere and Ruphy argued that different galactic models are not viciously relativistic: for Giere, the models offer complementary rather than conflicting perspectives; for Ruphy, the models are not perspectives of the same galaxy, and so cannot conflict in the first place. To apply to the whole of science, rather than specific classes of examples, a distinction between difference and conflict needs to fit within a pluralist epistemology of science.

Longino and Chang each developed an epistemology which tried this. For Longino, science is divided into epistemic community, whose values and aims constitute a local epistemology. Her hope is to show that not all communities are equal; the successful ones are open to criticism, include diverse perspectives, seek conformation among their other aims, and so on. But these qualities are indeterminate, including conformation. How much fit is good enough depends on local interests, so that model fit can mean radically different things in different communities; likewise with approximation and similarity. Allowable differences then become a matter of whatever healthy communities agree to disagree on, and conflicts a matter of what they subject to criticism.

Much the same structure is present in Chang's systems of practice. Each system houses its own notion of truth which captures the system's aims and features of its practices. Systems interact, and those interactions are helpful insofar as they produce benefits of toleration or interaction. Because each system judges success according to its own aims, and because achieving these benefits is a sort of success, whether these benefits exist cannot be judged from a completely detached perspective. The only way another system could challenge it, with the legitimate expectation that they should get a reply, is if both systems share many commitments in common. The less they share, the less there is to motivate disagreement. This is, in essence, epistemic relativism.

The second problem for the pluralism stance, the empirical problem, was that even in situations which seems structurally pluralist, scientists still adopt monist practices. This came into focus from our study of the evolutionary synthesis. Sewall Wright and R. A. Fisher held mutually exclusive theories of evolution, positing different explanations of adaptive variation. Experiment and argument were unable to settle the debate between them at the time; it would be many years before a strong consensus emerged. But they did not tolerate each other epistemically as the pluralist stance would recommend. Instead, they saw friction between their approaches, and persisted in debate. Much the same was true of the debate between naturalists and experimentalists after Darwin, even when these methodologies began to cross and blur disciplinary boundaries; and it was true of geneticists and systematists from the turn of the century onward, who could not replace or subsume each other's work, but nevertheless understood the situation as a detente rather than a peaceful coexistence.

Pluralists have noticed this discrepancy, whether in biology and other fields. But they have explained it away by dismissing the attitudes of the scientists themselves and insisting on a pluralist rational reconstruction of the science. In such cases, the monist practices were not central to scientific progress, while the pluralist setting was. This response puts their pluralism in tension with their empiricism.

Methodological monism addresses both problems. For the first problem, the lack of justificatory standards across communities, it introduces two desiderata for an epistemology of science. These take the form of two functions which the concept of truth must serve. The positive function, derived from Peirce, is to legitimate the formation of hypotheses as answers to scientific questions. The negative function, derived from Huw Price, is to motivate friction between different approaches and representations. Whereas the pluralist stance lessens the motivation for disagreement the farther apart approaches get, methodological monism motivates investigation and disagreement even when it seems that there is no clear way to get at the problem.

As for the second problem, the coexistence of pluralist and monist practices, methodological monism offers an interpretation on which there is no discrepancy at all. Plurality is a not a default state of affairs, but a stage in a larger process. This process is ordered toward a uniform internal norm, creating true representations. Plurality exists when scientific problems persist without a clear solution. This can happen for many reasons: lack of a common language or ontology, incommensurable measurements and models, and so on. In the divide between naturalists and experimentalists, there were many factors which contributed to keeping their approaches separate. Whatever the cause, the positive function of truth motivates investigations about the division, of the kind Morgan, Dobzhansky and Wright engaged in. And the negative function creates friction between different approaches, so that their differences seem to be conflicts until there is some positive reason to believe they cohere.

Scientific monism has been subject to serious criticisms. Methodological monism learns the lessons from those debates: it is not precommitted to any strong metaphysical picture of the world, or to an a priori rational basis for epistemology. These were issues in the views of David Hull and Philip Kitcher, despite their care with the history of science. Rather than imposing an external standard on scientific practice, the point of methodological monism is to articulate an internal standard. Thus the view fulfills the pluralist stance's first two core commitments: it is empirically sensitive and open to the many possible outcomes of scientific research.

By rejecting the third core commitment, that the pluralist structure of science is constitutive of success, methodological monism rejects an assumption which the pluralist stance and its opponents had in common. This assumption was that the epistemic structure of science was uniform. Rejecting this assumption allowed us to consider that scientific practice has a variable structure, with stages of relativity, plurality and unity. These stages may have drastic differences between them, but they share one thing in common. Because they are stages of scientific practice, they are concerned with creating scientific representations. And the practice of creating representations has, as an internal aim, a clearer understanding of the targets of those representations. Therefore scientific practice has a uniform internal aim, the truth. It is by this aim that we properly judge the success of scientific practice as such, regardless of the tangible resources we have to appraise and compare representations and approaches.

This has largely been a critical project, to show firstly that there are problems at the heart of the pluralist stance and thereby scientific pluralism more broadly, and secondly that there is a plausible, positive alternative to the pluralist stance. The second step is entirely necessarily to make the criticisms serious. A critical project cannot be solely negative, since even if a view has problems, pointing this out is of little use if all of the alternatives face worse problems. And this is just the situation we would find ourselves in here, given the strong criticisms the pluralist stance has levied toward metaphysical monism and epistemic relativism, as well as various rival forms of pluralism. There must, then, be some indication of where to move forward. This is the point of introducing methodological monism, a view which draws inspiration from pragmatist philosophy (Peirce, Huw), and which is as prepared to draw on empirical studies of science as pluralism has been.

It is one thing to specify desiderata which an epistemology of science should fulfill, and another thing entirely to develop such an epistemology. We have not done this here. To move still further forward, one must be developed. Among the question left open here is what kind of mechanisms are actually available to move between stages. Further historical and sociological case studies are necessary to give evidence of the variable structure of scientific practice, including examples of regress and nonlinearity. The case study of the evolutionary synthesis, and the comparative sketch given afterward, may make this plausible, but it does not cover the range of scientific practices and institutions necessary to compel anyone who remains sceptical. However, it should be a virtue of a philosophy of science that it raises interest and gives concrete direction to further empirical and historical questions.

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