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A Pluralism Worth Having: Feyerabend's Well-Ordered Science

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Abstract

The goal of this dissertation is to reconstruct, critically evaluate, and apply the pluralism of Paul Feyerabend. I conclude by suggesting future points of contact between Feyerabend's pluralism and topics of interest in contemporary philosophy of science. I begin, in Chapter 1, by reconstructing Feyerabend's critical philosophy. I show how his published works from 1948 until 1970 show a remarkably consistent argumentative strategy which becomes more refined and general as Feyerabend's thought matures. Specifically, I argue that Feyerabend develops a persuasive case against *rationalism,* or the thesis that there exist normative and exclusive rules of scientific rationality. In Chapter 2, I reconstruct Feyerabend's pluralism and detail its relationship to his humanitarianism and epistemological anarchism. I understand Feyerabend's pluralism as the combination of the principles of proliferation and tenacity. I show the evolution and justification of these principles from Feyerabend's early papers until the late 1970s. In Chapter 3, I defend Feyerabend's pluralism from its most prominent criticisms. I then clarify that Feyerabend's pluralism amounts to a conception of the logic of theory pursuit and modify his view using insights from C.S. Peirce, Pierre Duhem, and Michael Polanyi. From Peirce, I show how economic, sociological, and value-laden features of theory pursuit may be used to constrain proliferation and tenacity. From Duhem and Polanyi, I try to show the proper role of tacit knowledge within a Feyerabendian framework. Finally, I show what implications Feyerabend's pluralism has for models of distributing funds within scientific communities. I contend that it provides a more promising model that the 'well-ordered science' proposal advanced by numerous philosophers and social scientists. Specifically, I aim to understand what taking Feyerabend's pluralism seriously entails for principles of balancing funding allocation decisions and the role of peer-review in evaluating the potential success of research proposals. I conclude by suggesting future lines of research for further analyzing and applying Feyerabend's pluralism.

Keywords: Feyerabend; theoretical pluralism; methodological pluralism; theory pursuit; economics of discovery; tacit knowledge; well-ordered science; funding distribution models; peer review.

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Introduction

> "Pluralism is no longer simply an asset or a prerequisite for progress and development, it is vital to our existence" (Aga Khan IV).

> "my purpose is not to provide a scholarly account, my purpose is to tell a *fairy-tale* that might some day become a scholarly account and that is more realistic and more complete than the fairy-tale insinuated by Lakatos and his mafia" (AM_{B1}, 209-210).

This dissertation is written on the basis of two convictions. The first is quite optimistic: It is that every idea has value and that value is discovered through careful examinations of the idea, from various perspectives, and its practical implementation. The second is quite pessimistic: All ideas are inherently limited and those limitations are discovered through the interplay of distinct ideas. A great amount of damage has been done by neglecting the value of marginalized ideas or by asserting the absolute superiority of one's own preferred approach over its rivals. I hope that the content and justification of these convictions will become clearer in the following pages.

The title of this dissertation is a response to a challenge from Alan Richardson. In his contribution to *Scientific Pluralism*, he seems largely suspicious of a wide variety of pluralisms for their lack of philosophical interest. Rather, we should seek "a pluralism worth having" (Richardson 2006, 8). Richardson provides only hints at what such a pluralism may be.[1] The goal of this dissertation is to meet this challenge by reconstructing, critically evaluating, and comparatively assessing perhaps the most controversial and radical view of pluralism that has ever been proposed. This view has been completely ignored, dismissed, caricatured, or forgotten in nearly every corner of 21st century philosophy of science. This is the pluralism of Paul Feyerabend.

Feyerabend occupies a fascinating position in the history of 20th century philosophy of science. On the one hand, he is frequently mentioned as one of the forerunners of the 'historical turn' and, thereby, can be seen as one of the primary contributors to the contemporary

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[1] He suggests Rickert's pluralism which states that "there are different kinds *that can be known only in different ways"* (Richardson 2006, 7) and references Hacking's pluralism of 'styles of reasoning' (see Hacking 1996) but gives no argument to support them. Neither of these views will be defended or discussed in this dissertation.

intellectual landscape. He is often mentioned in passing in introductory textbooks and is covered in undergraduate philosophy of science classes suggesting that he was one of the more important figures in shaping the current intellectual landscape. On the other hand, Feyerabend is frequently accused of being a kind of sophist who used rhetorical techniques to provoke his opponents rather than trying to provide a positive philosophy. He is regularly marginalized by his detractors as, at best, an overreaction against the excesses of his predecessors and even his sympathizers only offer a cursory note or two on his influence. Rightly or wrongly, this has greatly contributed to the dismissal of engaging with Feyerabend's philosophy in a detailed and systematic manner. One of the aims of this dissertation is to overcome the many rumours and simplifications that cloud Feyerabend's place in history and demonstrate the historical importance and contemporary relevance of his pluralism.

Feyerabend's pluralism, as I see it, is essentially caught between two conflicting impulses in his general approach to philosophy. The first impulse is, in a sense, particularist. Considering each instance of, say, scientific reasoning as its own unique entity that can only be an instance of a general kind by being distorted. Any singular term, 'science', 'Voodoo', 'reason', 'truth', 'beauty', etc. ultimately represents a patchwork of instances or, as Wittgenstein put it, "a complicated network of similarities overlapping and criss-crossing" (Wittgenstein 1951, §66). These terms can only be truthfully discussed with exceptional, and often inordinate, amounts of care. "The one monster called SCIENCE that speaks with a single voice", Feyerabend lectures, "is a paste job constructed by propagandists, reductionists and educators" (Feyerabend 2011, 56). The second impulse is, essentially, systematic. Feyerabend remained, until the end of his career, a *general* philosopher of science in the literal sense of the word. He recognized how general principles and abstract considerations do not vanish, but reappear, in even the most benign examples. Even the most clear, obvious, or commonsensical view is contaminated implicitly or explicitly by highly abstract theorizing. How, then, can we proceed? We cannot *start* with general principles, since they will inevitably conflict with those instances they are meant to explain, and we cannot *start* with examples since those are partial products of general principles. Unlike the foundationalists, who pick a starting point one way or another, the hermeneutists, who argue that we should move back and forth between general principles and concrete cases, or the coherentists who merely want consistency between instances and general principles, Feyerabend's solution to this problem is through his pluralism, which encourages the development, maintenance, and interaction between inconsistent ways of understanding the world.

Feyerabend's pluralism is quite interesting to investigate for a number of reasons. First, as a matter of historical scholarship, it is an often misunderstood view. This is partially because Feyerabend's *magnum opus*, *Against Method*, is filled with rhetoric, exaggerated claims, *ad hominem* attacks, and is a poorly organized text. This text was representative of Feyerabend's philosophy to an emerging generation of philosophers who were unmotivated, as witnessed by the massive negative reception of AM, to seriously engage the text. It was also partially caused by the fact that Feyerabend's views were paraded around, in a quite simplistic manner, as a part of the exceptionally heated 'science wars' which framed issues in a different way than Feyerabend did. As such, Feyerabend's pluralism was never abandoned because it was found to be philosophically unsatisfactory after rigorous analysis, it was largely *forgotten* as a result of trends within 20th century philosophy of science and science studies. Even the recently budding secondary literature, as I will show, fails to recognize many of the most fascinating features of his pluralism.

Another reason to engage this position is its brute *uniqueness*. It is commonly known that Feyerabend rejects the very idea of 'scientific rationality' or 'scientific method', but it is unappreciated just how radical a claim this is. As I will show, Feyerabend does not merely dissolve 'the scientific method' into a taxonomy of methods for different domains or different instances of scientific research, he rejects the very notion that 'rational' considerations have *any* special bearing on scientific decision making. Even seemingly obvious maxims like "theories should be consistent with the evidence" or even "theories should be self-consistent" must be frequently violated and a great deal of 'arbitrariness' emerges as a necessary component of scientific progress. Even many of today's pluralists who want to maintain that there are still rational procedures, though those procedures are only rational in a given context, will conflict with Feyerabend's pluralism.

Finally, and most importantly, I think that Feyerabend's pluralism has much to offer contemporary philosophy of science. Thus, reviving this position is not just important as a point of contrast or of historical curiosity, but is crucial for the subsequent development of a number of interrelated topics that philosophers discuss nowadays. While I spell this out in concrete detail, this marks only the beginning. I have a strong suspicion that many views that depend upon assumptions about the nature of scientific rationality, the role of values in science, and the relationship between science and society will be drastically reformed after confronting Feyerabend's texts. Recent volumes on the relationship between history and philosophy of science (Mauskopf & Schmaltz 2012), demarcation (Pigliucci & Boundry 2013), the disunity of science (Galison & Stump 1996; Kellert et al. 2006), and values in science (Machamer and Wolters 2004), have systematically neglected to engage with, or even acknowledge, Feyerabend's insights. At the end of this dissertation, I will return to this and point out more concrete points of engagement that could be addressed in future research.

Before outlining the structure of this dissertation, it is worth separating Feyerabend's pluralism, as he understood it, and a Feyerabendian pluralism as I defend it by the end of Chapter 3. The first two chapters are historical in that they aim to reconstruct Feyerabend's pluralism from his texts. However, there are several features of this pluralism that are either implausible or unclear. These problems must be remedied in order to have present a pluralism worth having in its own right. At the end of the third Chapter, I will spell out the details of the differences between Feyerabend's pluralism and a Feyerabendian pluralism.

The structure of this dissertation is fairly straightforward. In Chapter 1, I reconstruct Feyerabend's critical philosophy. Specifically, I outline a family of arguments that become more general and precise as his thought progresses and culminates in Feyerabend's mature thesis against *rationalism* or the thesis that science operates according to some specifiable set of methodological rules which exclude 'irrational' approaches. I also show that the refutation of rationalism presupposes principles that Feyerabend, more or less, takes for granted. Namely, the principle of fallibilism, which states that every epistemological feature of science may be shown to be limited, and the principle of testability, which says that we should maximize the amount of ways in which we can test our theories. Since rationalists often preach some kind of fallibilism, much of Feyerabend's philosophical analysis and historical reconstructions is dedicated to showing how rationalism, if enforced, would have prevented progress and violates the principle of testability. While rationalism is never wholly defeated, since Feyerabend's pluralism requires that *every* position is of *some* value, it is downgraded to being a single approach among many.

In Chapter 2, I reconstruct Feyerabend's positive account of pluralism and contrast this reading against those of contemporary Feyerabend scholars (specifically John Preston, Robert Farrell, and Eric Oberheim). This account of pluralism relies on both the principles of fallibilism and testability, and the principles of proliferation and tenacity. The principle of proliferation was initially conceived of as a means to maximizing testability in a rather narrow sense, but becomes more unconstrained as Feyerabend's thought develops. The principle of tenacity, which Feyerabend learns from Kuhn, allows scientists to pursue a theory regardless of its faults. These principles unite to lead to Feyerabend's famous thesis that 'anything goes.' I take this chapter to contribute to the scholarship on Feyerabend's pluralism and providing a self-contained view worth of external consideration.

In Chapter 3, I critically appraise Feyerabend's pluralism. I begin by responding to some of the more influential, though superficial, criticisms and then entertain some more serious problems in Feyerabend's view. Some of these problems will lead to modifying Feyerabend's position, others will make underlying assumptions more explicit, and others will be addressed. I end the chapter by incorporating insights from a range of philosophers, specifically Duhem, Polanyi, and Peirce, with Feyerabend's pluralism to reformulate the view into a more plausible position. From Duhem and Polanyi, we recognize the importance of tacit knowledge in theory pursuit, which modifies the principle of tenacity. From Peirce, we learn that economic considerations allow us to restrict the pursuit of theories past certain points. This lesson is completely compatible with Feyerabend's view, which only states that the principles of proliferation and tenacity are *methodologically* unconstrained, but ends up bringing Feyerabend closer to Kuhn than we may expect. I take this chapter to open up new dialogues and offer novel criticisms and modifications to Feyerabend's pluralism.

Finally, in Chapter 4, I show some implications taking Feyerabend's pluralism seriously has for questions about how to organize scientific research. I begin by discussing Kitcher's depiction of a 'well-ordered science', and show how it must be revised. I go onto to show how particular models of resource allocation and diversification of research implicitly assume a Kitcherian model and show what limitations these models possess as a result. I go on to develop a positive conception of what Feyerabend's well-ordered science may look like and what implications it has for funding policy and the use of peer-review.

Before starting this dissertation, I would like to make a few minor stylistic comments. First, unless otherwise stated, italics appearing in quotes exist in the original quotations. Second, while there are certainly interesting differences between the four editions of AM,[2] I will cite the 4th edition unless otherwise stated. Finally, papers that appear in the 'Philosophical Papers' collections will be cited from those editions (with the exception of "Consolations for the Specialist"). This means I will use page numbers from those collections, but I will cite using the original dates for chronological purposes.

[2] I will cite the paper version of AM as "AM_P" and the book version as "AM_B" with additional numbers for the edition (e.g., AM_{B1}, AM_{B2}…etc.) where needed.

Chapter 1
The Revolt Against Rationalism: Feyerabend's Critical Philosophy

> "And Reason, at last, joins all those other abstract monsters such as Obligation, Duty, Morality, Truth and their more concrete predecessors, the Gods, which were once used to intimidate man and restrict his free and happy development: it withers away…" (AM_{B1}, 180).
>
> "'Das war eine graudsame Salbe! [That was a cruel ointment!]' – I suspect this, or some similar exclamation will spring to the lips of many readers of Paul Feyerabend" (Næss 1975, 183).

The goal of this chapter is to reconstruct Feyerabend's critical philosophy. Specifically, I argue that Feyerabend's papers from 1948 until the late 1960s constitute a series of arguments against *rationalism*[3] or, the view there are exclusive rules of rationality that form the basis of a normative methodology. 'Rationalism', as a term, does not appear until AM_{B1}, and even there it isn't straightforwardly defined. The term was first used by Popper, as a description of his own view in *the Open Society*, which Feyerabend co-opts as a form of mockery. In his later works, Feyerabend conceives of rationalism as the view that rationality itself is an ahistorical phenomenon that doesn't belong to any particular historical tradition (Feyerabend 1987a, 65). However, in AM, Feyerabend uses the term 'rationalism' to refer to a set of commitments regarding scientific methodology. Specifically, the commitment that 'rules of reason' provide *normative* and *exclusive*[4] criteria for some aspects of scientific decision-making. Rules of reason are epistemologically foundational and, therefore, not the kinds of things that can be tested. I contend that Feyerabend's criticisms of empiricism, critical rationalism, and his remarks on quantum mechanics are instances of a sustained and developed train of thought culminating in his criticisms of rationalism in AM. However, at its peak in the mid to late 1970s, rationalism does not merely encompass the positions of Feyerabend's interlocutors, but becomes a *novel*

[3] Rationalism is similar to, but not identical with, what Oberheim (2005) calls 'conceptual conservatism' or the view that theories that have already proven their success are inherently epistemically superior to untried alternatives. It should also be mentioned that rationalism, in Feyerabend's sense, is not equivalent to rationalism in the traditional sense (i.e., all knowledge derives from self-evident first principles).
[4] 'Exclusive' in the sense that the application of such rules excludes the desirability of applying conflicting rules.

position in its own right; a particular way of considering scientific methodology. I contend that there is a common style of thinking, present from Feyerabend's first publication as a graduate student in 1948 until the late 1970s, which became more general, radical, and sophisticated, as Feyerabend's thought matured. In this chapter, I will describe this style of thinking and what premises it relies on.

Interpreting Feyerabend is a difficult task for several reasons. His frequent jokes, occasional lexical carelessness, inconsistent self-reconstructions, numerous asides, rhetoric, and abrupt changes of topic make it exceedingly difficult to discern Feyerabend's views, even during a particular period of his career. Additionally, throughout his academic career, Feyerabend was surrounded by a wide variety of interlocutors with different perspectives which, combined with Feyerabend's intellectual openness, make Feyerabend's background motivations multifarious and, in some cases, rife with internal tension. As Oberheim and Collodel note:

> It remains unclear and it is still highly controversial to what extent, width and depth Popper's [or anyone's] interests and ideas had an impact on the development of Feyerabend's thought. This undoubtedly owes much to the wide range of intellectual stimuli from which Feyerabend could benefit, thanks to the combination of his vibrant curiosity and of the exceptional conjuncture of circumstances in which he found himself throughout his life, starting from his formative years (Oberheim and Collodel forthcoming (a), xix-xx).

Oberheim argues that the diverse and ephemeral nature of Feyerabend's views provide grounds for abandoning trying to reconstruct a self-consistent, positive position: "the inconsistencies in Feyerabend's diverse publications are not an immediate ground for criticism. This is because *Feyerabend never set out to supply a consistent philosophical position, nor did he need one*" (Oberheim 2006, 207-8). Against this, I demonstrate that Feyerabend developed remarkably coherent arguments for his own views and against various incarnations of rationalism. *Pace* Oberheim, the inconsistencies or tensions discovered within the next two chapters are essential to assessing the cogency of Feyerabend's philosophy. To demonstrate this, I must first be explicit about the historiographical method being used in the following two chapters.

Of the current interpretations of Feyerabend on the market, only Matteo Collodel declares what method he is employing (Collodel 2016, 34-5). Of course, there are many methods that could be employed. Kusch (1999) seeks to understand the structure and content of philosophical moves by situating them within a socio-political context (without reducing the arguments to that context). Members of the 'Cambridge school' seek to "downplay character and personality and focus instead on reconstructing authorial intentionally [via] understand[ing] what its author was trying to do" (Gross 2008, 6). Proponents of the 'humanist' approach seek to "weave a coherent narrative of a thinker's life and work around the notions of character and personality, to explain a thinker's ideas by situating them in the context of the life which they arose" (5). My purpose, as previously stated, is to reconstruct Feyerabend's views on pluralism. By this, I mean a view that can be reasonably attribute to him by a close reading of his published corpus. How Feyerabend understood his own views is, to me, incidental; he is but one interpreter of his own corpus.[5] Because of this, Feyerabend's *published* works will take precedence in the first two chapters. As Collodel writes, "[i]t can be agreed that quite in general, Feyerabend's early[6] published academic writings should be considered the "primary" sources for Feyerabend scholarship. This is both because they represent the main object of analysis, and because their authenticity is hardly disputable" (Collodel 2016, 33). Therefore, the following two chapters focus on Feyerabend's published work and biographical remarks and correspondences will play a supplementary role when needed.

The structure of this chapter is as follows. In section 1, I reconstruct Feyerabend's arguments for and against a family of views of methodology that Feyerabend calls 'empiricism.' Specifically, I demonstrate that his arguments against Schrödinger, Bohr, von Neumann, positivism, and instrumentalism from 1948 until the late 60s are motivated by a similar set of concerns where their views instantiate a version of rationalism. Feyerabend's arguments, in each case, shows how rationalism needlessly limits the testability of scientific theories or our ability to pursue theories. In section 2, I compare Feyerabend and Popper's conceptions of methodology and analyze the degree to which these views overlap. I argue that there is much less overlap than one may expect and, where such overlap exists, Feyerabend and Popper have distinct motivations for sharing similar views. As such, Feyerabend was barely a Popperian at any stage in his career and most of their commonalities are superficial. I also demonstrate how Feyerabend's disputes with Popper on Popper's views of quantum theory, which existed from the

[5] See Gadamer (1960, 1984) and Gadamer and Ricœur (1982) for a defense of this view. For a critical evaluation, see the papers collected in Schmidt (2016).

[6] Collodel specifies 'early papers' here since he is focusing on the relationship between Popper and Feyerabend which becomes distorted as Feyerabend removed positive reference to Popper in reprintings of his earlier papers.

late 1950s, were symptomatic of a deeper disagreement surrounding the nature of methodology. This disagreement became fully explicit in the 1970s and resulted in Feyerabend's public disavowal of falsificationism. In section 3, I detail the main arguments against rationalism in AM and clarify the content of rationalism. The historical argument, I contend, follows naturally from Feyerabend's portrayal of the history of science and the methodological argument follows from the principles of testability and fallibilism used throughout his earlier career.

1. The Acceptance and Rejection of Empiricism

After Feyerabend's brief flirtation with empiricism, he became one of its foremost critics. His criticisms appear in different contexts, but they share a common core: empiricism is unwise since it is a form of rationalism or view that "[excludes] the possibility of alternatives" (Feyerabend 1960a, 221). In this section, I reconstruct Feyerabend's arguments against different formulations of empiricism and extract their common core.

1.1: Feyerabend's Defense of Positivism

While Feyerabend was a graduate student, he founded the 'Kraft circle' (the 'third Vienna circle' (Stadler 2010)). This group "set itself the task to discuss [i]norganic sciences by studying the foundations of measurement procedures and their application in the special case of relativity theory [and] compare these methods to those in everyday life and the humanities" (quoted in Kuby 2016, 59).[7] Feyerabend retrospectively called himself a 'raving positivist' (Feyerabend 1978a, 112; AM_{B3} 275) who thought that "science is the basis of knowledge; science is empiricism; non-empirical enterprises are either logic or nonsense" (Feyerabend 1995, 68). However, there is only one published paper, "The Concept of Intelligibility in Modern Physics" (1948), where Feyerabend defends positivism.[8] Here, I summarize its structure and contextualize its conclusions.

Feyerabend defines 'positivism' as the view that science "is forced strictly to adhere to the phenomena and to remove all elements that do not have any representation in the phenomena from his mental images" (Feyerabend 1948, 1). On this definition, Feyerabend replaces the

[7] This quote comes from an overview of the reading group from May of 1948. See AM_{B2} pg. 274-5 for Feyerabend's description of the club and its activities.
[8] Feyerabend's next paper, "Physics and Ontology" (1954a), "criticiz[es] positivists and show[s] that their ontology has long been superseded in the course of scientific progress" (22).

dichotomy between 'appearance' and 'reality' with the dichotomy between 'visualizable' and 'abstract.' If a theory is visualizable (*Anschaulich*),[9] then we should be able to interpret it such that we can form mental presentations that "do not…require further explanation. They are immediately clear, evident, [and] visualizable" (2).[10] This notion is slippery since it requires that we are able to form mental images that are comprehensible; they must be *intuitive* in some sense. This is not the same as the mechanist view that all mental pictures must be presented in terms of elastic collisions; we can form a mental picture of action-at-a-distance without constant collision. For Kant, the necessary intuitions to form any mental picture were the pure intuitions of (Euclidean) space and time. What intuitions are necessary vary from thinker to thinker. What is common is that visualizability requires being able to form an intuitive mental picture of a physical process.

Feyerabend then distinguishes between the sense in which any macroscopic object or process is visualizable and those that enter lawlike regularities that science seeks to describe. This second sense involves *expectations* and is, therefore, not limited to immediate experience. Since our expectations are comprehensible by appealing to the way familiar objects behave, and what is familiar is contingent, what is intelligible changes over time; "Laplace's theory of capillary action is the best example of how much the concept of intelligibility is subject to change" (ibid). This argument, as Feyerabend was well aware, is not original. Max von Laue, for instance, argues that:

> What is deemed *anschaulich* is time-conditioned. A theory that forces ourselves to change our conventional conception of the external world seems always *unanschaulich* (unvisualizable) and necessarily so, in most cases even to its creators. That was already the case with Copernicus [and] with Faraday-Maxwell (Von Laue 1934, 441).

Feyerabend's twist is that constructing a visualizable theory requires constructing an unvisualizable theory *first*. As such, visualizability is a *result* of theory change rather than a condition for theory pursuit. Feyerabend writes "the elements of [an] earlier [theory] must be completely removed in order to let the new lawful regularities emerge. *This* is the position of contemporary positivism" (Feyerabend 1948, 3). For example, classical conceptions of the atom are easily visualizable, but problematic for empirical reasons. Every non-uniformly moving

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[9]*Anschaulich* is notoriously difficult to translate. It can be translated as 'intuitive', 'visualizable', 'picturable', 'intelligible', and 'insightful' (Kuby 2016, 60). Feyerabend equates *Anschaulichkeit* with 'visualizability.'
[10] For a history and defense of this view, see Cushing (1994, 20-22).

charge is the source point of electromagnetic radiation, and the first law of classical thermodynamics entails that radiation is accompanied by a loss of motion, entailing that continuous emission should lead to the collapse of the atom. This contradicts the stability of atoms and the sharpness of spectral lines. This model developed by saving it with the auxiliary hypotheses that electrons rotate around the nucleus without emitting continuous radiation, electrons follow particular paths (quantization), and the emission process causes electrons to leap from one orbit to another. This process of conceiving of a new atomic model *transforms* the classical model "until ultimately there is nothing of it left" (3). While initially unvisualizable, the process of developing a more empirically adequate atomic model *resulted* in one that was *anschaulich* in a novel way.

This paper appears as an antithesis to Schrödinger's "On the Peculiarity of the Scientific Worldview" (1948) where he argues that *anschaulich* must be understood as processes within space and time.[11] This requires that theories that are *anschaulich* must be amenable to a continuous space-time representation. Schrödinger makes a pragmatic and a principled argument for this. The pragmatic argument was simply that this method solved outstanding problems and was vindicated by the success of his wave mechanics.[12] The principled argument is that *anschaulich* is a *necessary condition* for physical theorizing. He writes:

> It has even been doubted whether what goes on in the atom could ever be described within the scheme of space and time. From the philosophical standpoint, I would consider a conclusive decision in this sense as equivalent to a complete surrender. For we cannot alter our manner of thinking in space and time, and what we cannot comprehend within it, we cannot understand at all (Schrödinger 1982, 26-7).

Feyerabend's argument addresses Schrödinger's principled argument, but not the pragmatic one. Demanding visualizability would prevent the pursuit of theories that are *initially* unvisualizable.

Recall that rationalism has two primary features: it must be normative and exclusive. Feyerabend interprets Schrödinger as being normative since it has practical implications and it is

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[11] This is contrasted with Heisenberg's conception of *anschaulich*, where "we can grasp the experimental consequences qualitatively and see that the theory does not lead to any contradictions" (Heisenberg 1927, 172). Schrödinger's justification of the *anschaulich* condition is transcendental whereas Heinseberg's is instrumentalist

[12] Kuby writes "[w]ave mechanics was Schrödinger's attempt to reinstate *Anchaulichkeit* in the atomic realm" (Kuby 2016, 60). However, the near universal adoption of wave mechanics was not met with a universal adoption of Schrödinger's interpretation. As Beller (1983, 491) remarks "Schrödinger witnessed a remarkable state of affairs: the universal use of his theory coupled with an almost total rejection of his interpretation."

exclusive since it places constraints on theory pursuit. If Schrödinger merely provided the practical justification, which would be *inclusive* of incompatible methodologies, Feyerabend would have no objection. In this sense, Schrödinger's position is an instance of rationalism. Finally, it is interesting to note that Feyerabend's criticism of Schrödinger is a non-sequitur. Schrödinger, like those who presented similar arguments before him, argues that *anschaulich* is a *goal* of physical theorizing: "[t]he discussion about *Anchaulichkeit* was a part of a family of interrelated disputes about the *aims of physics* and, in particular, *the requirements of a satisfactory physical theory*" (emphasis added, Kuby 2016, 60). In modern terminology, Schrödinger is concerned with *acceptance* whereas Feyerabend is concerned with *pursuit*. I bring this up now, since this argumentative twist will reappear in the future.

1.2: Feyerabend and Quantum Mechanics

Most of Feyerabend's papers in the 1950s, and many of his papers in the '60s, deal with technical issues within quantum mechanics. It is unclear whether, or to what extent, Feyerabend's work in this area is an *application* of his general philosophical views, or whether his philosophical views *emerged* from these studies. Regardless, there remains striking continuities between Feyerabend's arguments on von Neumann's no-go proof and Bohr's complementarity principle and his more general philosophical views that cannot go unnoticed. In this section, I will reconstruct these arguments and relate them back.[13]

Von Neumann claims that his 'no go' theorem[14] proves that any causal interpretation of quantum theory will contradict quantum theory which is well confirmed (Feyerabend 1954b, 104). Since many theories contradicted the previously best theories in their domains (e.g., Newton's laws contradicted Kepler's), von Neumann's proof provides neither a necessary nor a sufficient condition against constructing a causal interpretation (Feyerabend 1958a, 345). Feyerabend's primary argument runs, roughly, as follows: von Neumann follows the von Mises

[13] I am implicitly rejecting a few views on interpreting Feyerabend's remarks on the Copenhagen Interpretation. Preston (1997; 2016) argues that Feyerabend is arguing against the CI on purely Popperian grounds. I think this view is mistaken since Feyerabend disagrees with Popper's understanding of quantum theory (what he would later call "pompous declarations and sophomore mistakes" (Feyerabend 1969b, 294)) by 1957. The second interpretation, is that Feyerabend is arguing against the CI on purely *contrarian* grounds due to the CI's intellectual dominance (Oberheim 2006). I think this view is mistaken since it overlooks Feyerabend's commitment to realism during this period.

[14] Feyerabend's interpretation of von Neumann's proof is nearly identical to Bohm's. See Bub (2010) for an alternative interpretation.

interpretation of probability, which is compatible with Born's statistical interpretation and the fact that large ensembles can be easily experimented on. All ensembles, for von Neumann, are either pure ensembles (i.e., irreducible and can disperse) or combinations of pure ensembles. For this proof to show that determinism contradicts the *theorems* of quantum mechanics, von Neumann must assume what Feyerabend calls the 'completeness-assumption' that probabilities can be attributed to individual systems via relative frequencies (Feyerabend 1954b, 244).[15] Since this completeness-assumption begs the question against those who argue individual deterministic systems form statistical ensembles (Bohm), von Neumann's proof only shows that the Copenhagen interpretation is *compatible* with quantum mechanics and not that it follows from the theorems of quantum mechanics. The proof, therefore, does not rule out strategies that do not hold the completeness-assumption.[16]

Feyerabend, early in his career, argues that Bohr's complementarity principle is a form of rationalism (Feyerabend 1958b; 1960a; 1962a). The principle of complementarity entails that we must interpret microphysical theories instrumentally since all physical theories must be expressed in classical terms. Since micro-theories cannot be classical, it will be impossible to provide a realistic theory of quantum phenomena. As opposed to Heisenberg and von Weizsäcker, who thought this was merely contingent that we experience the macro world classically,[17] Bohr, on Feyerabend's reading, thought it was *impossible* to violate complementarity. Feyerabend argues that experiencing the world classically is a historical accident. As Feyerabend puts it, "if classical physics is a universal theory and has been used long enough then all our experiences will be classical and we shall therefore be unable to conceive any concepts which fall outside the classical scheme" (Feyerabend 1962a, 324). Feyerabend's argument against this, which greatly resembles his argument against Schrödinger, begins by noting that it is *possible* to construct an equally comprehensive, yet incompatible, theory.[18] He

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[15] Feyerabend rephrases this as "the statistical properties of any ensemble of quantum mechanical systems can be represented by a non-negative Hermitian operator" (Feyerabend 1958a, 344).
[16] This argument is expanded and refined in his (1957) and his (1962a).
[17] Hanson (1959) distinguishes between the 'extreme right' (Bohr), who saw complementarity as a principled position, and the 'center' (Heisenberg and von Weizsäcker) who saw complementarity as a contingent fact of experience. Feyerabend (1961a; 1962a, fn. 62 322) disputes the historical veracity of this claim and argues that all three were committed to the principled position.
[18] Feyerabend repeats this argument in his 1958c paper. Here, he writes "the invention of new conceptual schemes need not be psychologically impossible so long as there exists abstract pictures of the world (metaphysical or otherwise) which may be turned into alternative interpretations [of 'facts']" (24).

substantiates this claim with historical examples. He writes, "[s]uch a situation is by no means uncommon. The behaviour of the planets, of the sun and of the satellites as described by the Babylonians, by Plato, and by Ptolemy; it can be described by Newtonian concepts and by… general relativity" (323). He also provides the example of the appearance of the devil, which was a psychological fact in Cartesian psychology[19] and became lost during the transition to Galileo. Here we have examples of successor theories that are incompatible with previous best theories in their domain. Feyerabend summarizes his conclusions quite nicely:

> This result is exactly as it should be. Any restrictive demand with respect to the form and the properties of future theories can be justified only if an assertion is made to the effect that certain parts of knowledge we possess are absolute and irrevocable. Dogmatism, however, should be alien to the spirit of scientific research, and this is quite irrespective of whether it is now grounded upon 'experience' or upon a different and more 'aprioristic' kind of argument (325).

As we have seen, this statement is in line with Feyerabend's argument against Schrödinger in 1948.

1.3: Feyerabend on Observational Statements

Feyerabend's dissertation, *Zur Theorie der Basissätze* ("On the Theory of Basic Statements"), supervised by Viktor Kraft, was completed in November 1951. An abbreviated version of it appears in his "An Attempt at a Realistic Interpretation of Experience" (1958c)[20] and developed in "On the Interpretation of Scientific Theories" (1960b) and "The Problem of the Existence of Theoretical Entities" (1960c). Combined, these papers provide a comprehensive overview of Feyerabend's criticisms of empiricist conceptions of observational sentences.

Feyerabend distinguishes between instrumentalism, the view that "scientific theories are instruments of prediction which do not possess any descriptive meaning" (Feyerabend 1958c, 17) and positivism (attributed to Carnap)[21] that "scientific theories do possess meaning,

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[19] Feyerabend cites chapter 7 of Huxley's 1952 book *Devils of Loudun*. This chapter argues that the appearance of demons was also a 'fact of experience.'

[20] This is mentioned in the first footnote in the paper. See also appendix 2 of AM_{B2} for a description of his dissertation.

[21] It is difficult to discern whether Feyerabend's interpretation of Carnap is fair since Carnap was, in my view, exceptionally cagey as to whether he was merely explicating a notion of inductive confirmation or whether he intended his view to be normative. Popperians often pressed Carnap on this point. Much of Carnap's rhetoric appears normative (e.g., calling metaphysics 'futile' and 'sterile' (Carnap 1932) or using suggestive examples of the irrelevance of metaphysics (cf. chapter 1 of Carnap 1935)). The closest remark I can find on this topic is when

but…their meaning is due to connection with experience only" (ibid).[22] The primary target of these papers is the latter view. The argument runs as follows:

(1) Positivism should be[23] committed to a 'pragmatic view of meaning.'
(2) If (1), then positivism is committed to the stability thesis.
(3) The stability thesis conflicts with a reasonable methodology.
(4) We should be committed to a reasonable methodology
(5) ∴ We should abandon positivism.

The pragmatic view of meaning states that two conditions are necessary and jointly sufficient for a language to count as an observational language. First, for every atomic sentence,[24] there exists a physical situation where a class of observers will accept or reject that sentence. These observers must make this decision definitively, quickly, (nearly) unanimously, and the vocalization of the sentence must be causally dependent on the presence (or lack thereof) of the relevant event. Any sentence that meets these conditions is an observation sentence. Feyerabend calls this the 'characteristic' of a language, which guarantees the reliability of behaviors produced by physical events. On this view, humans are simply measuring apparatuses in that they have no special epistemological status over, say, a thermometer. The second set of conditions regards *interpretation*. For observation *sentences* to become *statements* (i.e., parts of a language with *meaning*), the sounds of observers must be understood in some way. Positivists claim that meaning is provided by experience whereas Feyerabend claims that meaning derives from a sentence's partaking in a theoretical context that has meaning independent of its

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Carnap states that "the purpose of inductive logic is precisely to improve our guesses [conjectures] and, what is of even more fundamental importance, to improve our *general* methods for making guesses, and especially for assigning numbers to our guesses according to certain rules" (Carnap 1966, 249) suggesting that induction can provide numerical values for theory choice and theory evaluation. Carnap also says that "I always [except in the *Aufbau*] emphasized, together with Neurath and the majority of the other members, that every factual statement is not certain…but may always be subject to re-examination and modification" (248-9). However, as Uebel nicely shows, Carnap actually had five distinct views of observation statements, three of which required incorrigible features (Uebel 2007, 444-5).

[22] More precisely, positivism can be subdivided into 4 distinct positions: "(a) theoretical terms are explicitly definable based on observational terms; (b) theoretical terms are extensionally reducible to observational terms; (c) theoretical terms are intensionally reducible to observational terms (d) theoretical terms are implicitly definable with the help of interpretative systems with either may, or may not, contain probability statements" (Feyerabend 1960b, 37). Feyerabend claims that these subdistinctions are irrelevant for his criticisms.

[23] Feyerabend later defends the pragmatic theory of meaning since it is compatible with his fallibilism (Feyerabend 1965a, 125). His attribution of the pragmatic view of meaning to positivists, is therefore, not exegetical but normative. Since the argument for this is in Feyerabend's dissertation, which has yet to be translated into English, I have nothing to say about the basis of this argument.

[24] The term 'sentence' is somewhat misleading here since it suggests that what is said has some logical form whereas a 'sentence', in Feyerabend's sense, is merely a dispositional behavioral pattern. As Kuby nicely puts it: "It is important to notice that within Feyerabend's account sentence uttered by an observer are simply physical facts, not interpreted linguistic statements…but they alone do not give any indication of what exists" (Kuby 2015, 119).

connection to experience. How does a theory acquire such a meaning? Feyerabend provides an empirical answer:

> [t]his is curious reasoning indeed if we consider that children learn new languages without the help of a previously known idiom [see chapter 16 of AM_{B4}].[25] Is it really asserted that what is possible for a small child will be impossible for a philosopher, a linguistic philosopher at that? (Feyerabend 1962b, fn. 74 77-8).

Simply put, we learn theoretical terms and then use them to render observation terms meaningful.

Feyerabend claims that "[a]ny philosopher who holds that scientific theories and other general assumptions are nothing but convenient means for the systematization of the data of our experience is thereby committed to the view (which I will call the stability thesis) that *interpretations…do not depend upon the status of our theoretical knowledge*" (Feyerabend 1958c, 20). Feyerabend's argument that the pragmatic view of meaning commits one to the stability thesis is quite simple. For a statement to be about the world (as opposed to a statement about one's private experiences),[26] that statement must have ontological consequences. Feyerabend defines ontological consequences as *testable* consequences of a statement, rather than logically entailed statements (20-1).[27] Even mundane activities like counting assume that the counted objects are discrete and the results of counting are independent of the order of counting. Regardless of how obvious these assumptions may be, there is no *a priori* reason for them to be true. If observational statements have ontological consequences, then we could not 'discover' the ontological consequences nor could they be shown to be incorrect since that would assume that the meaning of observation statements change as a result of theoretical discoveries. Therefore, for a language's characteristic to uniquely determine the interpretation of observational statements, changes in theory cannot lead to changes in interpretation.

[25] Here, Feyerabend uses Piaget's theory of language acquisition where theoretical terms are learned without relating them to other terms or through any observational idiom.

[26] This is because of Feyerabend's rejection of sense-data epistemologies whereby statements reflect subjective experiences. Statements understood this way cannot be about the world (i.e., it collapses into idealism) or used to communicate since subjective experiences are, by definition, inaccessible interpersonally.

[27] In a later paper, Feyerabend writes that the inability to state the ontological consequences of an observation statement makes observation statements 'metaphysical' because it is "not possible to specify an experimental result that would endanger it" (Feyerabend 1968a, 109) suggesting that having ontological consequences being logical consequences of observation statements violates the principle of testability. Therefore, we *choose* to interpret observation statements as having independent ontological assumptions.

This conclusion of positivism is clearly historically inaccurate. Still, Feyerabend thinks, this isn't enough to refute the stability thesis. Even if positivism were unanimously endorsed throughout history, it wouldn't be enough to justify its interpretation of observational sentences since scientists can be mistaken about what their methodological commitments should be. Rather, Feyerabend writes:

> What we are referring to when discussing the issue between positivism and realism [or any other thesis] are certain *ideals* concerning the form of our knowledge. In short: *the issue between positivism and realism is not a factual issue which can be decided by pointing to certain actually existing things, procedures, forms of language, etc., it is an issue between different ideals of knowledge* (33-4).

Feyerabend considers two objections to this characterization. The first objection is that "all our theoretical knowledge is (uniquely) determined by the facts and cannot be chosen at will" (ibid). Since we are always *able* to pursue new interpretations, we need additional arguments for why these alternatives should not be pursued. The second worry is that resolving the debate between positivism and realism conventionally makes the resolution arbitrary. Feyerabend denies this and asserts that we can "judge an ideal by the consequences which its realization may or may not imply" (35). Feyerabend thinks positivism has three unwanted consequences: 1) the interpretation of observation statements should be stable, 2) we lose the distinction between thought and imagination and sensation, and 3) it leads to subjectivism. For 1), since the stability thesis conserves *extant* interpretations, and those interpretations presuppose a theory, that theory becomes untestable (ibid). This conflicts with maximizing the testability of a theory. For 2), we place a "restriction on the argumentative use of our language" (ibid). In other words, we cannot use thought to change our sensations and, thereby reduce the means of exposing their limitations. Finally, some experiences depend on a *relation* between the observer and the world, rather than being a mere *property* of an object. If we restrict our knowledge to what is given in experience, discussing the 'real relation' between an observer and the observed makes no sense. Therefore, we could never discover that what we thought was a property was actually a relation. Because of these considerations, Feyerabend thinks that positivism is an unwise choice.

Feyerabend has a second closely related criticism of empiricism stemming from his analysis of the observation/theoretical distinction (O/T distinction). Feyerabend's first formulation of the O/T distinction is as follows:

> A concept is an observational concept if the truth-value of a singular statement containing either only that concept, or that concept along with other observational concepts, can be determined quickly and solely on the basis of observation, or at least if it is imaginable that a decision of this kind will be possible someday (Feyerabend 1960c, 16).

Feyerabend thinks that what is observable changes with time. Habituation and growing accustomed to features of experience eliminates or expands the conscious distinction between direct and indirect observation. If this is true, then all theoretical concepts are observational concepts at different points in history.

Feyerabend then considers the objection that this formulation of the O/T distinction is misguided since psychological facts about our observations are irrelevant to the logic of science. What is relevant is how we *justify* theoretical statements.[28] In other words, the O/T distinction is a distinction between types of justification. Feyerabend reformulates the problem as this:

> An observational concept is a concept which can be constructed so that a singular statement which contains only this concept is not only arrived at immediately, without reflecting on it at all, but it is also a statement which does not require further justification other than pointing out that a certain observation was made (21).

On this view, observation statements are certain[29] and theoretical statements are hypothetical. However, this quickly leads to the conclusion that all concepts are theoretical for three reasons. 1) we must be able to distinguish objects of perception from other elements of our visual field, 2) observational statements depend on mediating causes in the act of observation,[30] and 3) physiological factors can affect the content of observation. All three reasons require theoretical statements and, therefore, we must *justify* the veracity of the observational statement using *theories*.[31] Therefore, *all statements are hypothetical*.

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[28] Feyerabend extends this argument to statements made in ordinary language, making it applicable to Carnap's 'thing-language' (Carnap 1938) as well (cf. Feyerabend 1960c, fn. 5 41).

[29] This reading is true of Schlick's views (though his first view allocated certainty to *statements* while the second allocated certainty to the *affirmations of statements*) and two of Carnap's views (where certainty was allocated to untranslated statements) (cf. Uebel 2007, 444-5). These details should not affect Feyerabend's argument.

[30] This argument continues in Feyerabend's more mature works. In his 1970b, he writes "even the most advanced theories and the most advanced observations may deceive us by reflecting the properties of our close surroundings, measuring instruments, and senses included, rather than the laws of the world at large" (69).

[31] In his 1960b, Feyerabend elaborates on this argument and tries to show that attempts to give a fully observational account of a physical situation leads to an infinite regress (39). Since understanding the relation between an observer and what is observed requires understanding what Feyerabend calls 'mediating terms' (features of an observational environment which causally affect what is perceived without being directly perceived themselves, like the status of the retina, intensity of light radiation, etc.), we must try to understand mediating terms observationally. But these

Clearly, Feyerabend interpreting positivism as a normative thesis which is quite intentional, even if it is ambiguous as to whether the proponents of positivism intended their theses normatively. Furthermore, positivism thus construed prevents the pursuit of theories that -*re*-interpret observation statements thus making positivism *exclusive*. However, we see that Feyerabend's primary criticism of positivism is that it reduces testability. While I will discuss Feyerabend's adoption of the principle of maximum testability in future sections, it is worth briefly commenting on how testability is related to rationalism. Feyerabend often uses the term 'test' in a quite broad manner. Unlike Popper, who defines tests as homotopic basic statements that contradict the laws of a theory, Feyerabend uses 'tests' both in the conventional sense and as a synonym for 'criticism.' Criticism, for Popperians, is an activity and can thereby considered a line of *pursuit*. Limiting testability, therefore, can be seen as a limited case of pursuit. Since criticism often, as we will see throughout the next two chapters, requires an alternative theory, sometimes limiting testability requires limiting *theory* pursuit.

1.4: Feyerabend on Instrumentalism

Instrumentalism, for Feyerabend, is not necessarily committed to the O/T distinction. This is because theoretical terms, for instrumentalists, have no epistemic status at all. Instrumentalism, therefore, requires an independent criticism which is found in his "Realism and Instrumentalism" (1964a).[32] Here, Feyerabend defines instrumentalism as the view that "even a theory that is wholly correct does not describe anything but serves as an instrument for the prediction of facts" (176). Feyerabend specifies that he is interested solely in instrumentalism that "actively contribute[s] to the *development* of factual knowledge rather than make comments, in a 'preferred mode of speech',[33] about the *results* of this development" (177). Feyerabend then distinguishes between instrumentalism as a *global position*, established by philosophical

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would require a different observational situation that would require its own mediating terms and so on ad infinitum. See Preston (1997, 35-6) for a discussion and Zahar (1982) for a criticism of this argument.

[32] A paper which, interestingly enough, Lakatos calls an "excellent paper from [Feyerabend's] *almost-Popperian* period" (emphasis added, Lakatos and Zahar 1976, fn. 4 175).

[33] This phrase comes from Nagel (1961, 152). Here, Nagel discusses how realism and instrumentalism are malleable enough to "assimilate into its formulations not only the facts concerning the primary subject matter explored by experimental inquiry but also all the relevant facts concerning the logic and procedure of science" (ibid). Feyerabend writes "I do not doubt for a second that there are versions of the problem which do possess this degenerate character. At the same time, it seems to me that the instrumentalist position of Proclus, of some astronomers of the early seventeenth century, and of Niels Bohr is prompted by much more substantive motivates that the predilection for certain modes of speech" (Feyerabend 1964a, 176).

argumentation, and local instrumentalism about *specific theories*. He writes "within quantum theory [and the heliocentric hypothesis] the instrumentalist position has been forced upon the physicist by the realisation that the current theory interpreted realistically must lead to the wrong results; it is not merely a repetition of the philosophical idea that all theoretical thinking is of instrumental value only" (fn. 32, 190). He provides two case studies: Bohr's interpretation of quantum theory[34] and the Copernican revolution. Since I have touched on the former, I will briefly outline the latter.

Feyerabend begins by outlining the virtues of Aristotelian cosmology. It had solved the difficulties of monism exhibited by Parmenides and Zeno, that monism entails that no change is possible, with the distinction between the potentiality and actuality of matter and the corresponding account of causation (see Coope 2009). The resultant law is well confirmed in everyday life; physical objects do not move unless they are hit or pushed by another object. It was also comprehensive since it explains types of change including quantitative, qualitative, generative, and locomotive, and was quite detailed.[35] This provided a successful empirical theory that had immediate consequences for the motion of the earth: The earth must be at rest since all objects near the surface of the earth would be whipped along by the motion of the earth and objects in the skies would assume their natural state of motion and be 'left behind.'[36] The *initial* Copernican hypothesis was an "unsupported conjecture in the face of fact and well-supported theory" (181). It required ad hoc additions of epicycles to explain the 'loops' of planetary motion, it had well-known dynamical difficulties,[37] it conflicted with 'common sense',[38] and had "no independent evidence in [its] favor" (182).[39] Feyerabend finishes with characteristic flair: "[t]he only favorable remark that could be made was that it somewhat simplified calculations by

[34] Here is the first instance where Feyerabend departs from his earlier reading of Bohr as holding complementarity as a principled position. In this paper, complementarity arises due to paradoxes and empirical problems that arise with interpreting the uncertainty relations realistically. I will return to this point later in this chapter.
[35] Feyerabend also addresses the possible objection that Aristotle's view of motion did not give a satisfactory account of projectiles and falling bodies by showing how they can be explained by the theories of impetus and antiperistalsis (Feyerabend 1964a, fn. 4 178).
[36] Feyerabend additionally cites Ptolemy's dynamic arguments for the view that the earth is at rest (cf. 179-80).
[37] Feyerabend asserts that Copernicus was 'well aware' of the dynamical difficulties and cites Galileo's arguments in the Second Day of the *Dialogues* as a recognition of this problem. He does not mention what the difficulties are.
[38] "Copernicus was regarded as absurd; read Luther, Sir Francis Bacon and the professional astronomers of the time" (197). See also chapter 13 of AM_{B2}.
[39] This is a bit of an overstatement. But it is certainly true that it appeared less empirically adequate in its initial form (cf. chapter 2, Crombie 1959). Feyerabend backtracks on this claim slightly in chapter 10 of AM_{B2}.

a suitable coordinate transformation. [However,] the resulting mathematical success does not imply that the coordinate system chosen has any *dynamical* preference over other[s]" (183).[40]

This is what forced Osiander[41] and Bellarmine to take an instrumentalist position on the Copernican theory; the problems with the theory and the successes of Aristotelian cosmology made the latter a more obvious candidate as a true description of reality.[42] Feyerabend contrasts this with global versions of instrumentalism that "concern not only *one* theory but *any possible* theory" (185) which, he claims, has been 'decisively refuted' by Popper.[43] Following Farrell (2000, 261), we may call the view that particular theories cannot be true descriptions of the world 'local instrumentalism' and 'global instrumentalism' as the view that *all* theories are mere instruments. The motivations for adopting local instrumentalism differ. In Osiander and Bellarmine's case, it was because considering the Copernican hypothesis "as a correct account of the actual situation in the universe amounts to upholding an unsupported conjecture in the face of fact and well-supported theory" (181). At best, it was a mathematical instrument for calculating some planetary phenomenon. For Bohr, instrumentalism was needed to remedy contradictions between energy transfer and the quantum postulate (186-7).[44] Therefore, the refutation of global instrumentalism is not sufficient for refuting local instrumentalism:

> It is very important to realize the complex character of the situation, for otherwise one will be satisfied too early and too easily. Thus a thinker who is acquainted with the epistemological arguments will regard a refutation of these arguments, and the construction of an alternative epistemology…as the completion of his task….Quite

[40] Even the increase in simplicity of the Copernican view was called into question by many historians. While the Copernican system avoids commitment to equants and some eccentrics, these are replaced by new epicycles and epicyclets. See section 2 of Lakatos and Zahar (1976) and the references therein for a more detailed discussion and Kuhn (1957, 133-170; 1977, 324).

[41] In appendix 1 of AM_{B1}, Feyerabend argues that Duhem omits key passages where Osiander cites the factual inadequacies of Copernicus and he thus mislabels Osiander as a 'dogmatic instrumentalist.' Feyerabend also writes that Osiander had tactical reasons for his instrumentalist interpretation.

[42] There were also political/moral reasons for instrumentalism, which Feyerabend defends in appendix 2 of AM_{B2}.

[43] In this paper, Feyerabend cites the arguments of chapter 2 of *Conjectures and Refutations*. Popper has many interwoven arguments here, so it is difficult to discern what exactly impressed Feyerabend here. One major argument Popper gives, and I would guess is what Feyerabend is referring to, is summed up as follows: "Instruments, even theories *in so far as they are instruments*, cannot be refuted…The instrumentalist interpretation will therefore be unable to account for real tests, which are attempted refutations, and will not get beyond the assertion that *different theories have differed ranges of application*. But then it cannot possibly account for scientific progress…For it is only in searching for refutations that science can hope to learn and to advance" (Popper 1962, 151-2). See Tibbetts (1972) for a distinction between five different criticisms Popper launches at instrumentalism throughout his career.

[44] Feyerabend also, briefly, provides the examples of the kinetic theory of matter and special relativity (Feyerabend 1964a, fn. 20 185).

> obviously such an attitude will not impress the 'physicists' whose arguments have not even been touched (185).

Rather, local instrumentalism is problematic for a distinct reason: it is inherently *conservative*. Since interpreting theories realistically, in the domains discussed here, will lead to theories that contradict established theories in their domains (cf. sec. 9), local instrumentalism claims that alternatives are not legitimate candidates for pursuit as potentially true descriptions of the world.

We see how local instrumentalism is a kind of rationalism: it restricts the scope of theories that can be pursued. What is distinct about local instrumentalism from the positions discussed this far, is that its exclusivity is not grounded in any epistemic principle. Rather, the *current state of research* blockades some instances of theory pursuit. It is unclear, however, why Feyerabend rejects global instrumentalism. Clearly, any instrumentalism that takes observation terms as basic will be subject to the Feyerabend's criticisms against positivism. But strictly speaking, the notion that theories just are instruments seems compatible with pursuing other (incompatible) instruments or criticizing instruments to improve them. Without more detail, Feyerabend has not given us much reason to think that global instrumentalism is an instance of rationalism.

1.5: Concluding Remarks

Let us conclude by considering what positions have been refuted and for what reasons. The positions addressed thus far were:

(1) Schrödinger's *anschaulich* condition
(2) Bohr's Complementarity Principle (and von Weizsäcker and Heisenberg's versions)
(3) Von-Neumann's 'no-go' proof
(4) Positivism
(5) Local Instrumentalism[45]

(1) excludes theories that cannot be expressed in a continuous space-time representation, (2) excludes the description of microphysics classically and realistically, (3) excludes particular kinds of hidden variable theories, (4) excludes the possibility of 'correcting' observational facts via theoretical considerations and providing differing interpretations of a single characteristic, and (5) excludes pursuing theories with theoretical and/or empirical difficulties. Since being

[45] I have left global instrumentalism off this list since it is unclear why Feyerabend rejects it.

exclusionary is a feature of rationalism, we can see how each of these targets is a kind of rationalism in this sense. Second, the *justification* of each of these views comes from some general principle. (1) is justified by the nature of understanding, (2) is justified by the necessity of classical terms, (3) is justified by the principle that we shouldn't pursue theories that contradict established theories, (4) is justified by the definition of observation statements as being uniquely determined by a language characteristic, and (5) is justified by the view that we shouldn't pursue theories that contradict the 'evidence' of the day. Each of these principles is *general* and *inviolable*. This also presupposes that they cannot be *tested*, but only, at best, defused through philosophical reasoning. Third, while Feyerabend is never explicit, he is concerned about what implications these positions have for *pursuit*. Recall, for (1), Feyerabend argues that the *process* of conceiving an *anschaulich* theory requires violating the *anschaulich* condition. For (2) and (3), Feyerabend is ultimately concerned about the *pursuit* of deterministic (specifically Bohmian) microphysical theories. For (4), Feyerabend is concerned with showing that we *can* pursue theories with differing theorical frameworks with the same empirical evidence. For (5), Feyerabend is concerned with pursuing theories that conflict with extant evidence. Finally, and more obviously, each of these views is interpreted as a *normative* theory. I will not address whether or not this is a fair interpretation of other thinkers, but I will return to the point later about why Feyerabend thinks methodology must be normative. As seen from our definition of rationalism given in the introductory section, each of the views Feyerabend addresses at this stage in his career are instances of rationalism.

2. Feyerabend on Falsificationism

What is remarkable about the 'Popperian stage' of Feyerabend's career is how suddenly he appeared to accept falsificationism and how quickly he became its fiercest critic. Many of Feyerabend's 1960s papers are meant to complement falsificationism[46] though he never argues for this. In this section, I outline the ways in which Feyerabend and Popper's views overlap and differ. More importantly, I argue that Feyerabend never was a full-Popperian and the differences in their views culminate in his public renunciation of falsificationism.

2.1: Feyerabend's 'Fall' for Falsificationism

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[46] This is suggested by Feyerabend's numerous off-hand remarks, footnotes citing Popper as the philosophical background to his arguments, and his use of Popperian terminology.

Feyerabend retrospectively claims that he ‘fell’ for falsificationism.[47] While these remarks appear where Feyerabend would underplay his intellectual debt to Popper, it is compatible with an odd feature of Feyerabend’s corpus: he never defends the principles of falsificationism, even though he explicitly endorses it in publication. Regardless, there appear to be two key factors that led Feyerabend to adopt falsificationism: one being personal and the other was the *spirit* of falsificationism. Feyerabend writes the following about his first encounters with Popper:

> I had met Popper in Alpbach 1948. I admired his freedom of manners, his cheek, his disrespectful attitude towards the German philosophers which gave the proceedings weight…his sense of humor (yes, the relatively unknown Karl Popper of 1948 was very different from the established Sir Karl of later years) and I also admired his ability to restate ponderous problems in simple and journalistic language. He was a free mind, joyfully putting forth his ideas, unconcerned about the reaction of the ‘professionals’ (Feyerabend 1978a, 115).

Later, in his autobiography, he writes:

> The argument which finally convinced me that induction was a sham [was] presented [by Popper]…[48] Falsification now seemed a real option, and I fell for it. I occasionally felt a little uncomfortable… there seemed to be a worm somewhere in the woodwork. Still, I applied the procedure to a variety of topics and made it the centerpiece of my lectures when I started teaching (Feyerabend 1995, 89).

Feyerabend admired Popper’s personal traits and this seems to have played a substantial role in his adoption of falsificationism. Furthermore, there are many common features of Feyerabend and Popper’s philosophies at a coarse-grained level. Both see criticism as central to methodology, were classical liberals,[49] rejected foundationalism, and thought methodology is normative. While the particularities of these positions may not always line up, there is a general sense in which Popper and Feyerabend’s philosophy share much in common.

This shows that there appears to be no substantial philosophical material to address the question of why Feyerabend adopted falsificationism. It was a mixture of personal impressions

[47] In a letter to Lakatos, Feyerabend writes that “My “spiritual development” always depends on theatrical episodes, *never* –thank Behemoth—on argument” (in Motterlini 1999, 272).

[48] The argument, which Feyerabend claims was “Duhem’s –but Popper didn’t say so” was that “higher-level laws (such as Newton’s law of gravitation) often conflict with lower level laws (such as Kepler’s) and therefore cannot be derived from them” (Feyerabend 1995, 89).

[49] While Feyerabend begins following a Millian liberalism in 1968, he explicitly defends many liberal values beforehand (especially freedom of thought and expression). Popper’s political philosophy, found most prominently in *The Open Society and its Enemies*, was influenced by his friend Friedrich Hayek.

and a vaguely similar approach to the philosophy of science. To understand what kind of Popperian Feyerabend was, we must compare their published work on overlapping topics.

2.2: Comparing Feyerabend to Popper on Methodology

There are five essential features of falsificationism: 1) a rejection of induction, 2) testability, 3) a rejection of ad hoc hypotheses, 4) taking falsifications seriously, and 5) the nature of scientific methodology. In this section, I argue that Feyerabend and Popper come to the same conclusions on 1), 2), and 3) but have importantly different motivations. Furthermore, their views differ substantially on 4) and 5). By going through each of these topics, I discern precisely what kind of Popperian[50] Feyerabend was.

Feyerabend has two short papers on induction in 1964 and 1968.[51] The 1964 paper makes a few cursory arguments. He first considers the justification of induction via its common usage[52] and rejects this quickly stating that "the standards implied in common behavior are themselves open to criticism and…the task of philosophy is to provide such a criticism, and not be satisfied with such popularity" (Feyerabend 1964b, 204). He also states that Hume's problem is insurmountable and weaker versions that confer probabilities to universal statements are invalid. The only (novel) sustained argument goes like this: take a universal statement about a property P belonging to every x such that $(\forall x)(Px)$. By induction, a conjunct of observation statements *uniquely* determines the truth value of $(\forall x)(Px)$. Even if this were valid, it is *unreasonable* since it conflicts with Feyerabend's defense of pluralism, which will be discussed in the next chapter. In 1968, Feyerabend argues that solely focusing on negative instances avoids the grue paradox ("All emeralds are grue" is still falsified by a blue emerald) and Hempel's paradox ("All ravens

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50 As Lakatos points out repeatedly, there are many important ambiguities in Popper's views. Lakatos even remarks, "I should like to say that never in my life have I experienced more sharply the pains of the historian than in this analysis" (Lakatos 1974a, 143). Given the complexity of this interpretation, I will not try to justify interpreting pre-1974 Popper (before he responds to Lakatos' interpretative remarks (see Popper 1974)) as 'Popper$_1$' in Lakatos' sense.

51 Feyerabend makes many side remarks about induction. He calls Bohm's use of induction 'highly unsatisfactory' (Feyerabend 1960a, 220), Kraft's arguments against induction (see Kraft 1960, 232-53) 'very convincing' (Feyerabend 1963b, 320), and criticizes von Neumann's account of induction (Feyerabend 1958a, 345-6). He also praises the use of 'counterinduction', a term introduced in AM$_B$, as early as 1961 (see Feyerabend 1961b, 56).

52 While no citation is given, Feyerabend seems to be referring to Reichenbach who argues that "the principle of induction is unreservedly accepted by the whole of science and…no man can seriously doubt this principle in everyday life either" (Reichenbach 1930, 188).

are black" is still falsified by a white raven). This second paper is less interesting in hindsight since Feyerabend later mocks the importance of such considerations (AM_B, fn. 2 28).

Compare this stance to Popper's.[53] Popper also believes that popularity is no reason to endorse induction: "even supposing [induction's popularity] were the case -- for after all, 'the whole of science' might err -- I should contend that a principle of induction is superfluous, and that it must lead to logical inconsistencies" (Popper 1935, 5). As seen in the final clause, Popper thinks induction fails on *logical* grounds; no finite amount of singular statements can confirm a universal statement and induction is inherently circular. Additionally, Popper argues that attempts to remove Hume's problem by redefining universal statements[54] rest on the assumption that meaningful statements are, in principle, completely verifiable (which fails because of the first point).[55] Since Popper and Feyerabend are conventionalists about methodology, we can only compare their views on induction by providing the grounds on which they think induction is *undesirable*. Popper's claims are much weaker than Feyerabend's; he rejects induction because of *logical difficulties* whereas Feyerabend's argues that induction conflicts with a reasonable methodology *even if it were valid.* For Popper, induction isn't even eligible as a proposed convention, since it is incoherent; "there is no such thing as induction" (18). Feyerabend accepts induction as a coherent proposal, but thinks it conflicts with reasonable methodological demands.

Many of Feyerabend's positive views are meant to complement Popper's principle of testability, although Feyerabend never justifies it himself.[56] Popper's views on testability are quite complicated but, for now, it suffices to stick to the basics. For a theory to be testable (i.e., falsifiable), it must have a non-empty set of potential falsifiers. Testability provides a criterion for theory choice (Popper 1935, 95); we should always choose (unfalsified) theories that are more testable and make current theories as testable as possible. Popper gives a few reasons to accept falsifiability. First, it avoids the logical difficulties of induction. Second, and more acutely, even though Popper regards conventionalism, his primary opponent, as a "system which

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[53] For Popper's analysis of the probabilistic surrogate here, see section 81 of Popper (1935) and his (1938, 1955).
[54] Popper is referring to Schlick's (1931, 156) view that laws lack empirical content but should be understood as "rules for the transformation of [basic] statements."
[55] See Ayer (1936) and Schlick (1934) for a defense of this view.
[56] The most explicit statement of this is found in his 1965a: "The model which underlies my own discussion has as its aim maximum testability of our knowledge. No argument will be given for this aim here" (105) with a footnote immediately after stating "For such arguments see K. R. Popper, *Conjectures and Refutations*" (fn. 4).

is self-contained and defensible [and] attempts to detect inconsistencies in it are not like to succeed" (59), he still finds it "quite unacceptable" (ibid). He writes:

> Underlying [conventionalism] is an idea of science, of its aims and purposes, which is entirely different from mine. Whilst I do not demand any final certainty from science…the conventionalist seeks in science 'a system of knowledge based upon ultimate grounds', to use a phrase of Dingler's. This goal is attainable; for it is possible to interpret any given scientific system as a system of implicit definitions (ibid).

Here, Popper contrasts the fallibility of falsificationism with the foundationalism of conventionalism. Since we can always interpret theories conventionally, the issue boils down to making a *conscious choice* to *take refutations seriously*. This difference, Popper thinks, becomes more acute during times of crisis where "[w]henever the 'classical' system of the day is threatened by the results of new experiments which might be interpreted as falsifications according to my point of view the system will appear unshaken to the conventionalist. He will explain away the inconsistencies which may have arisen…or he will eliminate them by suggesting *ad hoc* the adoption of certain auxiliary hypotheses" (60). Falsificationists will "take the greatest interest in the falsifying experiment. We shall hail it as a success, for it has opened up new vistas into a world of new experiences" (ibid).

This gives a vague sense of the difference in *attitude* between the conventionalist and the falsificationist, for Popper. Biographical roots for this attitude can be traced back to Popper's frustrations with psychoanalysis.[57] Those with similar frustrations may be convinced, but it still seems far too subjective to ground the choice between falsificationism and conventionalism. I think a stronger argument comes from Popper's *political philosophy* found in *The Poverty of Historicism* (1957) and *The Open Society and its Enemies* (1962).[58] It is clear that Popper sees strong analogies between falsificationism and open-minded democratic discourses amongst equals, and foundationalist epistemologies with 'closed societies.' The former is compatible with democracy and classical liberalism, whereas the latter is compatible with totalitarianism, which is responsible for war (Popper 1962a, 329), racism (Popper 1962b, 9), and slavery (62-5). "[T]he

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[57] Popper writes that Adler "saw confirming instances everywhere; the world was full of verifications of the theory. Whatever happened always confirmed it...every conceivable case could be interpreted in the light of Adler's theory, or equally of Freud's" (Popper 1965, 34-5).

[58] Lakatos (1970) thinks that Popper's criterion for testability comes from its conformity with the opinion of the 'scientific élite.' However, Lakatos only provides one quote to support this and the parallels between Popper's political and epistemological views are too great and too many to be mere coincidences.

closed society is characterized by the belief in magical taboos [i.e., foundations], while the open society is one in which men have learned to be to some extent critical of taboos, and to base decisions on the authority of their own intelligence (after discussion)" (Popper 1962a, 202). The details of this view are not important for now. The important point, for now, is that Popper's decision to adopt falsificationism appears to derive from his political views.

At this stage of his career, Feyerabend had no developed political (or ethical) views to compare to Popper. There is little textual evidence that Feyerabend's views on testability have the same initial justification. All that is clear at this point is that Feyerabend endorses testability and does not give any reasons for preferring this principle. However, given that the principle of testability presupposes the principle of fallibilism, more salient question is why Feyerabend endorses the principle of *fallibilism*. I will return to this point in the next section.

Popper admits that ad hoc hypotheses are logically possible but still not allowable (Popper 1935, 20). While Popper is ambiguous as to what constitutes an ad hoc hypothesis, Lakatos (1968a, fn. 1 389) helpfully points out that Popper uses 'ad hoc' to mean hypotheses that reduce the testability of a theory by removing falsifiers without adding new testable consequences.[59] While Feyerabend endorses the 'no ad hoc hypotheses' motto, he emphasizes it less than does Popper. Feyerabend also never defines what constitutes an ad hoc hypothesis but, regardless, seems to think this request is quite trivial. He writes, for instance, that ad hoc "hypotheses are *never* framed by scientists" (in Hoyningen-Huene 1995, 369) and that the request is "very modest, and almost trivial" (in Hoyningen-Huene 2006a, 626) since "as a matter of historical fact this decision [not to use ad hoc hypotheses] has been made by nearly all great scientists" (Feyerabend 1960a, 233).[60] This is an important difference: Popper takes the use of ad hoc hypotheses seriously as a decisive *philosophical issue*; Feyerabend seems to think it is a *triviality for scientists*. This hints at Feyerabend and Popper's differing motivations. Feyerabend is motivated to "understand and, perhaps, aid scientists", whereas Popper's is to "develop a special point of view, to bring this view into logically acceptable form" (Feyerabend 1981c, 21). As we shall see, this subtle difference in motivations explodes in their public disputes.

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[59] Post-1963 Popper uses ad hoc to also mean uncorroborated excess content according to Lakatos.

[60] Feyerabend does criticize Szilard's re-interpretation of the second law of the phenomenological interpretation of thermodynamics for being ad hoc (Feyerabend 1965a, fn. 25 113).

Popper's views on falsification came under fire from Lakatos, Kuhn, and later Feyerabend because it led to the overly harsh view that a single refutation can decisively and immediately eliminate a theory. This view, 'naïve falsificationism', was one that Popper often emphasized as essential to scientific methodology; there must be *definitive* refutations.[61] However, Feyerabend never fully subscribed to this aspect of Popper's view, even though he does call it "one of the most fundamental principles of methodology" (Feyerabend 1958d, 236).[62] There are two primary reasons for this. First, Feyerabend never defines falsifications as *immediate* grounds for rejecting of a theory[63] nor does he defend the view that we shouldn't *use* falsified theories.[64] Second, as will be elaborated in detail later, Feyerabend was aware of the need to develop theories despite their apparent conflicts with test statements (see Feyerabend 1960a, 233; 1961b, 70; 1963a, 196). Later, Feyerabend calls this 'the principle of tenacity.' Indeed, Feyerabend thinks tenacity is *compatible* with falsificationism. In a letter to Kuhn (circa. 1961), he writes:

> The fact that theories are not given up the moment some difficulty arises does not at all show that scientific practice does not conform to the stereotype of falsification. Quite the contrary, immediate abandonment of a theory as soon as the first difficulty is perceived would mean that an uncritical attitude is adopted with respect to the test statement itself which a falsificationist would never allow (in Hoyningen-Huene 1995, 368).

Additionally, Feyerabend recognizes how apparently refuting instances can be transformed into confirming statements. He writes:

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[61] To be more accurate, Popper wavers on this point. In a footnote added in a later edition of *The Logic of Scientific Discovery*, Popper writes that "I have been constantly misinterpreted as upholding a criterion…based on a doctrine of 'complete' or 'conclusive' falsifiability" (Popper 1935, fn. 1 28). But in section 20, Popper aims to "formulate methodological rules which prevent the adoption of conventionalist stratagems" (62) where these stratagems are precisely what Feyerabend (and Kuhn and Lakatos) use to justify tenacity: testing observation statements, blaming experimental apparatuses, ad hoc hypotheses, promissory notes, etc. (cf. 60-1). He also writes that "If the outcome of a test shows that the theory is erroneous, then it is *eliminated*" (Popper 1940, emphasis added 404). It remains unclear where Popper stands on this issue and what is left of his falsificationism if he admits that we are always justified in denying that any refutation has taken place (cf. Andersson 1991a for an interesting discussion on this topic).

[62] In this paper, Feyerabend argues that Putnam and Reichenbach's attempt to interpret quantum mechanics in terms of three-valued logic covers up important problems (how to relativize elementary quantum mechanics) without offering anything new (i.e., new theories, new experiments, new explanations, etc.) (Feyerabend 1958d, 237). This is compatible with maximizing testability but doesn't require definitive falsifications.

[63] The closest quote on this I can find is Feyerabend stating, in passing, that "the position to be demanded here [incommensurability] demands that our theories be testable and that they be abandoned *as soon as a test does not produce the predicted result*" (emphasis added, Feyerabend 1962b, 45). First, incommensurability, as will be seen in the next chapter, merely shows that a certain kind of proliferation maximizes testability that does not require the added assumption that these tests are instant or definitive. Second, Feyerabend often remarks to the contrary (even in the same paper). Because of this, I think this statement is a mere slip of the tongue.

[64] See Bartley (1968, 51-2) for a discussion of this.

> Refutations are final only as long as ingenious and nontrivial alternative explanations of the evidence are missing. There can be no argument to the effect that the absence of such an alternative at a particular time will make the refutation final. It is very important to point out…that the fate of many theories strongly depends upon the belief of their defenders that it will be possible, at some future time, to incorporate into them all the apparently refuting instances (Feyerabend 1961d, 158).[65]

Since we can always construct 'ingenious and nontrivial alternatives', there are no final refutations. More concisely put, "as long as different points of view are encouraged it is difficult to see how any refutation could be decisive" (155). Clearly, Feyerabend was *never* a Popperian on this score.

Finally, and most importantly, it is worth discussing the nature of scientific methodology for Popper and Feyerabend. I divide this topic into three components: 1) the *justification* of methodology, 2) the *scope* of methodology, and 3) the *normativity* of methodology. For 1), both Feyerabend and Popper think that we adopt a methodology by *convention*. We can understand Popper's position by contrasting it with the positivist's conception of methodology:

> Positivists usually interpret the problem of demarcation in a *naturalistic* way; they interpret it as if it were a problem of natural science. Instead of taking it as their task to propose a suitable convention, they believe they have to discover a difference, existing in the nature of things, as it were, between empirical science…and metaphysics…They are constantly trying to prove that metaphysics by its very nature is nothing but nonsensical twaddle-'sophistry and illusion', as Hume says, which we should 'commit to the flames'. If by the words 'nonsensical' or 'meaningless' we wish to express no more, by definition, than 'not belonging to empirical science', then the characterization of metaphysics as meaningless nonsense would be trivial…But of course, the positivists believe they can say much more about metaphysics than that some of its statements are non-empirical. The words 'meaningless' or 'nonsensical' convey, and are meant to convey, a derogatory evaluation; and there is no doubt that what the positivists really want to achieve is not so much a successful demarcation as the final overthrow and the annihilation of metaphysics (Popper 1935, 12-3).[66]

Naturally, we can construct many self-consistent and mutually exclusive methodologies. Popper simply writes that "anyone who envisages a system of absolutely certain, irrevocably true statements as the end and purpose of science will certainly reject the proposals I shall make here"

[65] Later in this paper, Feyerabend argues that the irregularity of the motion of planets was strong refuting evidence against the claim that fixed stars and planets obey the same laws of circular motion and says that "[s]imilar considerations apply to Copernican astronomy (the refuting evidence here was provided by the Aristotelian theory of motion), the kinetic theory, and the like" (Feyerabend 1961d, 158).

[66] Lakatos (1968a) also argues that the conventionalist approach is distinct from the 'justificationist' approach where methodology is justified by 'foundational' epistemological arguments.

and "in arriving at my proposals I have been guided, in the last analysis, by value judgments and predilections" (15). In passing, Feyerabend makes similar remarks. He mocks the idea that methodology can be 'discovered' either in the nature of scientific practice, since this is to be corrected by methodology, or in some foundationalist epistemology. At other times, Feyerabend motivates the conventional nature of methodology for *fallibilistic* reasons. He writes,

> The reason I cannot accept Bohm's methodology of caution,[67] and why I prefer to it the methodology of falsification as it has been developed by Popper, is therefore that the methodology of caution assume the existence of things we know for certain, whereas I believe…*that this is too optimistic a view of the status of our knowledge* (emphasis added, Feyerabend 1960a, 235).

We see here, that while Popper and Feyerabend's views on 1) converge, they converge as a result of slightly different motivations.

For 2), Popper thinks methodology applies to the appraisal of ready-made theories. This is a result of his distinguishing between the context of discovery and justification. To avoid conflating questions of fact and questions of validity, Popper writes:

> I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge – in contradistinction to the psychology of knowledge – I shall proceed on the assumption that it consisted solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained (Popper 1935, 8).

The logic of science, or the philosophical task of constructing methodological rules, is done by value judgments and argumentation. However, "it is impossible to decide, by analyzing its logical form, whether a system of statements is a conventional system of irrefutable implicit definitions, or whether it is a system which is empirical in my sense; that is, a refutable system" (61).[68] Instead, the *series* of methodological moves falls within the scope of methodology. In other words, "whether someone's intellectual behaviour counts as "scientific" or not depends not upon where he gets his hypotheses from but rather upon what he does with them when he has

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[67] See sections 6-8 in Feyerabend 1960a for his interpretation of Bohm's view on methodology.

[68] Lakatos calls this "Popper's greatest innovation" though Popper's "demarcation criterion is formulated in terms of falsifiability or unfalsifiability of *propositions* rather than in terms of the progressive or degenerating character of *problemshifts*. But a careful reading of his text enables one to extricate the emerging powerful idea in spite of the old-fashioned terminology" (Lakatos 1978a, 221; cf. Giedymin 1968, section 3). Wittgenstein, interestingly enough, makes the same point (Wittgenstein 1969, §98).

them" (Bennett 1964, 35). Conventionalists, according to Popper, proceed in the following way: *first* we have a theory, *then* it is threatened, *then* the theory is saved by conventionalist stratagems.[69] Falsificationists, on the other hand, proceed differently on the third point in that they 'take the refutation seriously.' Feyerabend, on the other hand, never clearly defines his methodological scope. However, as I have already mentioned, Feyerabend has implicitly conceived of methodology as applying to *theory pursuit*. While theory pursuit sounds like the Popperian scope of methodology, Feyerabend's is interested in *under what conditions we can pursue theories*. Rationalism exclude alternatives from being pursued. Popper is also interested in this question, since falisifiability provides criteria for theory pursuit (though Popper uses the terminology of theory 'choice') and theory rejection but, additionally, he is interested in how we are to develop pursuitworthy theories. Feyerabend's methodological scope, therefore, is only a subset of Popper's.

Feyerabend provides two lines of thought for 3). The first comes from Feyerabend's countless remarks denigrating philosophy that does not attempt to *improve* science itself.[70] The intuitive basis for this appears to be that a methodology incapable of providing such means (or, at least, *attempting* to provide such means) is a mere idle pastime. Feyerabend writes,

> In about 1925 philosophers of science were bold enough to stick to their theses even in those cases where they were inconsistent with actual science. They meant to be *reformers* of science, and not *imitators*…In the meantime they have become rather tame (or beat) and are much more prepared to change their ideas in accordance with…the latest fashion of the contemporary scientific enterprise. This is very regrettable, indeed, for is considerably decreases the number of rational critics of the scientific enterprise (Feyerabend 1963a, fn. 23 89).[71]

There are a couple of elements to this. The first is the wide-open call for 'critics of the scientific enterprise', which, as will be seen in the following chapter, is a renunciation of Feyerabend's pluralism. Second, Feyerabend worries that philosophers have the surreptitious motivation of dogmatically praising scientific achievements or science as a whole. Indeed, the (largely)

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[69] Note that this order is both logical and historical. Obviously we must have a theory before we have a refutation of that theory logically speaking, but Popper also thinks that theories must be initially unfalsified and, therefore, the refutation must come after the theory.

[70] See Feyerabend 1970a for a sustained discussion of this kind. Remarks like this are also scattered throughout his early papers as well (cf. Feyerabend 1964a, 176).

[71] This sentiment can also be found in Feyerabend (1978a, 40): "It is surprising to see that philosophers who were once inventors or new world views and who taught us how to look through the status quo have now become its most obedient servants."

unspoken *political* attitudes that supported the activities of the Vienna circle (as opposed to their published works) may become reinforced by reconstructions of science.[72] Third, Feyerabend's pleads that philosophers should not consciously shape their philosophies to conform to scientific practices. Contradictions are necessary for critical discussion and should be welcomed. This is consistent with Feyerabend's reasoning behind the normative nature of scientific methodology:

> how do we know (independently of the fact that [the sciences] exist, have a certain structure, and are very influential) …that the sciences are a desirable phenomenon…and that their analysis will therefore lead to reasonable methodological demands? And did it not emerge that… [unreasonable methodological demands] *are* adopted by some scientists? Actual scientific practice, therefore, cannot be our last authority. We have to find out [what] are *desirable* conditions and this is quite independently [sic] of who accepts and praises [methodological demands] and how many Nobel prizes have been won with their help (89).[73]

Science is conducted by humans who make mistakes, even about the nature or importance of their own trade. To accept scientific practices as the *final word* on methodology is an appeal to authority.[74] Compare this to Popper's stance: "the whole of science may err" (Popper 1935, 5). The 'facts' of scientific practice are subject to criticism and, particularly, should be subjected to *philosophical criticism*.[75] This claim is stronger than Feyerabend's, in a sense. Feyerabend often *motivates* his views using historical examples and it is unclear what force his methodological arguments would have if these examples were absent. While I will return to this claim in more detail later, for now, it seems as if the notion that history can *test* methodology appears in Feyerabend's views of methodology but not Popper's.

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[72] Reisch (2005) discusses the political attitude of logical empiricists towards science extensively. See also Wartofsky (1982) for a connection between the published philosophical views of logical empiricists and their social and political aspirations.

[73] In a footnote immediate after this quote, Feyerabend cites the discovery of 'white Jews' by Nazi physicists as an instance where we can criticize scientific practices on *ethical* grounds. The passage, however, is referring to *methodological* criticisms (Feyerabend 1963a, fn. 24 89).

[74] At many points in Feyerabend's arguments where he has shown a conflict between practice and methodology, he pauses to point out that this isn't sufficient to refute that view of methodology and proceeds to give reasons why his methodology should be preferred (Feyerabend 1958c, 33; 1962b, 69).

[75] See sec. 10 of Popper (1935) for his criticism of 'naturalist' approaches to methodology and Popper (1952) for a more general discussion of the nature of 'philosophical' problems. He also defends the *philosophical* character of methodology by saying that methodologies can be compared and see whether they give rise to inconsistencies (Popper 1935, 31). Here, Popper compares the logical advantages of falsificationism over inductivism but the comparison between falsificationism and self-consistent methods, like conventionalism, requires both a philosophical analysis and a decision made on the basis of value judgments.

To sum up, while Feyerabend certainly was a kind of Popperian, their views diverge in a variety of ways. Popper criticizes induction on logical grounds, Feyerabend criticizes it since it conflicts with pluralism which, as we shall see, ultimately depends on the principle of maximal testability and the principle of fallibilism. While both seek to maximize testability, Feyerabend motivates this principle on fallibilistic grounds whereas Popper motivates it on political grounds. Popper criticizes ad hoc hypotheses to clarify his own methodology, whereas the use of ad hoc principles isn't a live issue for Feyerabend. The fact that Popper takes this issue more seriously than Feyerabend hints at their differing motivations: Popper's definition of falsifications as instant and definitive is a decisive issue between falsificationism and conventionalism, whereas Feyerabend denies this definition implicitly by defending the principle of tenacity. Finally, Popper's view on the scope of methodology is about the pursuit and pursuitworthiness of theories whereas Feyerabend is solely interested in pursuitworthiness. Popper's reasons for making methodology normative stem from the logical priority of methodology whereas Feyerabend is motivated by the brute intuition that the purpose of philosophy of science is to improve science.

2.3: Feyerabend's Realism and Fallibilism

Feyerabend defends realism from his dissertation in 1951 until 1981.[76] Specifically, he defends *conjectural* realism, or the view that we treat scientific theories *as if* they described the world without being committed to the view that theories succeed in doing so. Conjectural realism requires that theories are universal (i.e., support counterfactuals) and, therefore, transcend possible boundaries of experience and should be interpreted as if they describe genuine features of a mind-independent reality. The first formulation of realism Feyerabend provides is the following:

> In [the realist] tradition the facts of experience, whether or not they are now describable in terms of a universal theory (such as classical mechanics), are not regarded as unalterable building stones of knowledge; they are regarded as capable of analysis, of improvement, and it is even assumed that such an analysis and improvement is absolutely necessary (Feyerabend 1960a, 224).

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[76] In the paper I am referring to here, "Proliferation and Realism as Methodological Principles" (1981), Feyerabend spends the majority of the paper arguing for pluralism and dedicates only the last paragraph to realism. Feyerabend also defends a realism in his 1999, but this realism is quite distinct from the realism discussed here (see Brown 2016).

Feyerabend mentions two ways that universal theories can help in 'analyzing' (or 'correcting') the facts of experience, both of which we have already encountered. The first way we may call the psychological effect of universal theories; they "lead to an experience of reality that is very different from our own" (Feyerabend 1961b, 58). This could reasonably be equated with the 'theory-ladenness' thesis that beliefs causally affect perception.[77] The second argument is grounded in the principle of testability: realism makes observation statements testable.[78] Take, for example, the statement "This table is brown." Not only does this require theoretical assumptions to be veridical, but a realist interpretation makes this statement subject to tests, since 'table', 'brown', and the identity relation are all theoretical terms. This is opposed to both empiricism and instrumentalism that interpret this statement as something whose truth-value is *assumed* (either because it represents a statement about sense-data or because it is something that must be recovered by general laws). Interpreting theories realistically is just a consequence of the principle of testability.[79] As mentioned in the previous section, Feyerabend gives no reason for accepting the principle of testability. Among Feyerabend's discussions of realism, we can see that Feyerabend's endorsement follows from his principle of *fallibilism*. Feyerabend, at numerous points, makes this more general principle essential to methodology:

> We have to realize that no element of our knowledge, physical or otherwise, can be held to be absolutely certain and that in our search for satisfactory explanations we are at liberty to change any part of our existing known, however "fundamental" it may seem to be, to those who are either unable to imagine or to comprehend alternatives (Feyerabend 1961a, 384).
>
> Neither 'facts' nor abstract ideas can ever be used for defending certain principles come what may. Wherever facts play a role in such a dogmatic defense, we shall have to suspect foul play…of those who try to turn good science into bad, because unchangeable, metaphysics (Feyerabend 1963a, 102).
>
> Our intention [is] not to except any part of our knowledge from revision (Feyerabend 1965a, 125).

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[77] This equation must not be taken too strictly. Feyerabend's empirical evidence for theory-ladenness comes largely from the Whorff-Sapir hypothesis and Piaget's developmental psychology (see section 9 of Feyerabend 1965a). However, Feyerabend also expresses some skepticism about theory-ladenness (AM_{B1}, 133). For a more empirically updated version of theory-ladenness see Estany (2001) and the literature on cognitive penetrability (Stokes 2013).

[78] Or, choosing instrumentalism which entails that "the relations between terms which constitute this 'observational core' will forever be exempted from change. Their stability will be guaranteed not because a detailed examination has found them to be inadequate… [t]hey have been turned into veritable *idola fori*" (Feyerabend 1966a, 243).

[79] Furthermore, as we remember from the section on instrumentalism, we cannot *universally* interpret theories realistically without potentially introducing new problems. As such, interpreting theories realistically isn't a 'principle' in the sense that it cannot be used for *all theories*.

> No part of our knowledge is ever exempt from change and… it is futile to base eternal truths on conceptual considerations (Feyerabend 1966b, 415).

Feyerabend's fallibilism extends to every feature of epistemology; experience, reason, intuitions, ordinary language, etc. In essence, any grounding of certainty.[80] Realism maximizes testability which presupposes fallibilism. You can't test something that can't be false (or modified). Feyerabend recognizes this point himself: "For a critical attitude to make sense, the theories to which this attitude is applied *must be fallible*, that is, they must be accessible to criticism and they must not be built in a manner which in advance guarantees their absolute validity" (Feyerabend 1961b, 70). Therefore, we can conclude that realism follows from the principle of fallibilism.

Feyerabend also defends realism in a second manner. He writes that realism "encourages research and stimulates progress" by "remain[ing] persistent in the face of the sometimes quite formidable objections of their opponents" (Feyerabend 1964a, 196). This will later be called the 'principle of tenacity', which will be discussed in the next chapter. This view is distinct from Feyerabend's earlier conception of realism since one can accept that observation statements are testable without accepting that we should pursue theories despite their problems. Is realism *necessary* for tenacity? I don't think it is. Recall Feyerabend's earlier argument against local instrumentalism: it "is more conservative and therefore liable to lead to dogmatic petrification" (ibid). This only follows from instrumentalism with the *added premise* that one should not pursue theories that contradict previously established theories in those domains. Some instrumentalists have taken this attitude, but it does not necessarily follow. Because of this, tenacity does not presuppose realism in any important sense.

Feyerabend's personal commitment to realism notwithstanding, his deeper commitment is to the principles of testability, fallibilism, and tenacity. While realism appears prominently in Feyerabend's corpus, it is reducible to Feyerabend's more general principles.[81] As such, it isn't a central feature of Feyerabend's methodology.

2.4: Feyerabend versus Popper on Quantum Theory

[80] Reducing certainty to some notion of 'probability' would only displace Feyerabend's criticisms rather than avoid them since it leaves the question of what features of science we *can* criticize unaltered.

[81] Remember, we cannot just *automatically* interpret any theory realistically since that will lead to problems in particular cases. We may try to resolve those cases, but these actions are justified by tenacity and not realism.

Feyerabend always distanced himself from Popper's views on quantum mechanics. As Oberheim rightly notes, Feyerabend "was very critical of some aspects of Popper's work on Quantum Mechanics already in the early 1960s" (Oberheim 2006, 110). In fact, the disagreement began in April of 1957, when Popper entrusted Feyerabend to read his paper *in absentia*, where Feyerabend's disagreements with Popper's interpretation of the CI first materialized and were amplified by his conversations with Bohm (Collodel 2016, 39).[82] Feyerabend first publically criticized Popper in his paper on the CI (Feyerabend 1961b),[83] but only in passing. It isn't until 1968 where Feyerabend provides a sustained criticism of Popper on quantum theory. My contention is that these seemingly minor disputes, when combined with Feyerabend's substantive views, provide the beginnings of one of Feyerabend's major disagreements with Popper.

As we have seen, Feyerabend distinguishes between global and local instrumentalism. Popper never makes this distinction.[84] Feyerabend's criticism of Popper lies at precisely this point. Popper argues that realism is criticized in quantum mechanics from philosophical prejudice whereas Feyerabend sees it as an instance of local instrumentalism. This is apparent in their interpretations of Bohr; Feyerabend interprets Bohr as a local instrumentalist[85] whereas Popper interprets him as a global instrumentalist. Most succinctly, Popper writes "I do not believe that physicists would have accepted such an *ad hoc* principle [complementarity] had they understood…that it was *a philosophical principle*" (emphasis added, Popper 1962, 135). This is symptomatic of a deeper disagreement about methodology; Popper thinks that philosophical arguments about the relationship of experience to theories are sufficient for undermining local instrumentalism whereas Feyerabend sees them as irrelevant.

The fact that Popper thinks arguments against global instrumentalism suffice is evident from the structure of his papers. In (1956), Popper straightforwardly equates Berkeley's

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[82] Feyerabend first met Bohm at this event. However, as Collodel notes, and contrary to Feyerabend's retrospective remarks (Feyerabend 1978a, 116; 1979a, 205), Bohm's influence did not appear in Feyerabend's publications until briefly in his 1965a (pg. 53). Feyerabend also had privately expressed reservations of Popper on quantum theory in some of his correspondences (Collodel 2016, 39).

[83] Collodel states that Feyerabend's paper "O Interpretacjj Relacyj Nieokreslonosci" (1960d) also criticizes Popper on quantum theory. Since this paper has not been translated, I have no stance on this matter.

[84] Feyerabend recognizes this point in his 1964a (fn. 19 185).

[85] Feyerabend wavers on this point. In some moments, he engages in a straightforwardly Popperian criticism of Bohr qua global instrumentalist. In other moments, Feyerabend states that Bohr's view can actually be regarded as a realist view (Feyerabend 1961c, 404) or materialist (Feyerabend 1966b, 416).

arguments for instrumentalism with Bohr's complementarity principle, Heisenberg, and Mach. He writes:

> Today the view of physical science founded by Osiander, Cardinal Bellarmino, and Bishop Berkeley, has won the battle without another shot being fired. Without any further debate over the philosophical issue, without producing any new argument, the instrumentalist view (as I shall call it) has become an accepted dogma. It may well now be called the 'official view' of physical theory since it is accepted by most of our leading theorists of physics (although neither by Einstein nor by Schrödinger) (Popper 1956, 99).[86]

Here, he straightforwardly conflates local and global instrumentalism. Similarly, Popper criticizes Berkley's arguments on absolute space in Newton on purely philosophical grounds without touching on the difficulties Berkeley accuses Newton of in *De Motu* (Popper 1953).[87] More generally, everywhere he discusses instrumentalism (Popper 1935, fn. 1 37, sections 11-15, 19-26, in the postscript), Popper *constantly* lumps arguments against the Copenhagen interpretation, Mach, and Berkeley together with global instrumentalism. Feyerabend criticizes the instrumentalism of Bohr, Heisenberg, Osiander, etc., only when they try to expand their arguments into *general* epistemological positions or think realism is *impossible* in a given case. For Feyerabend, local instrumentalism can be a reasonable way of interpreting theories.

This view can also be seen in Feyerabend's criticism of Popper's interpretation of Bohr. In Feyerabend's papers on the subject,[88] he makes two arguments. First, Popper's attempt to construe quantum theory as a generalization of classical statistical mechanics via his propensity interpretation of probability[89] runs into established physical problems. Feyerabend argues that Popper's view is a repetition of the Bohr-Kramer-Slater conjecture that had been refuted[90] by the Bothe-Geiger and Compton-Simon experiments. Second, he argues that Bohr's position is not a

[86] Feyerabend takes issue with this quote later on in his career (see Feyerabend 1964a, fn. 32 190; 1969b, 279-80).

[87] In his section 3 of his1954a, Feyerabend repeats Popper's arguments nearly verbatim. In 1965, however, Feyerabend writes "Berkeley's notion of space as explained in *De Motu* was different from Newton's – but this difference appeared neither in experiment nor in the mathematical formalism accepted by either man. It consisted in an attitude influencing the *future development* of the theory of gravitation" (Feyerabend 1965c, fn. 2 98).

[88] These arguments primary appear in Feyerabend's two-piece paper on complementarity. I will be citing from the combined abridged version "Neil Bohr's World View" (1969b).

[89] This interpretation, in Popper's words, results in "the reduction of the wave packet…has nothing to do with quantum theory it is a trivial feature of probability theory" (Popper 1967, 37). See also Popper (1982, 71-3) for a response to Feyerabend's criticism and Feyerabend's reply (1984, note 1 128).

[90] Feyerabend notes that Popper could view these results with suspicion. However, some argument would be needed. Also, though Feyerabend does not mention this, Popper would be violating his maxim of taking falsifications seriously.

form of rationalism; Bohr was a practicing pluralist who came to accept complementarity as a result of research. Contrary to Popper, Feyerabend writes:

> The idea of complementarity is therefore not just the result of having pursued a mistaken programme to the bitter end as Popper would want us to believe…Bohr, after all, did consider a purely statistical theory. He did consider such a theory despite the fact that it was not in line with his own point of view…Bohr was...open minded enough…to develop and publish a purely statistical particle theory with waves acting as probability fields only. It was only after the refutation of this theory that he returned to his earlier philosophy – and this time with very good reasons. This important episode is not mentioned anywhere in Popper's paper – a very unfortunate omission that makes the idea of complementarity appear much more dogmatic than it actually is (Feyerabend 1969b, 268-9).

Feyerabend then provides numerous personal remarks from Bohr's colleagues that show that Bohr had an open-minded, pluralist attitude towards science[91] and discusses Bohr's skepticism of his own atomic model (270-3). He also cites quotes to demonstrate that Bohr understood his own ideas only as tentative starting points for further research rather than philosophical principles. In his concluding section, 'Back to Bohr!', Feyerabend states his position succinctly:

> Popper's criticism of the Copenhagen Interpretation, and especially of Bohr's ideas, is irrelevant, and his own interpretation is inadequate. The criticism is irrelevant as it neglects certain important facts, arguments, hypotheses and procedures which are necessary for a proper evaluation of complementarity…His accusation of dogmatism, too, is quite ill founded. There was hardly a physicist who was so intent on seeing all sides of a problem, and so fond of qualifying his remarks, as was Bohr…*he was prepared to test his philosophy with the help of alternatives* (293-4).

Regardless of whether Feyerabend's interpretation of Bohr is correct, we see that the objection against local instrumentalism requires the principle of tenacity. Without this, Popper cannot maintain any global argument about how to interpret theories.

2.5: Abandoning Falsificationism

Feyerabend's criticisms of Popper's conception of methodology come out most forcefully in 1970 and become more expanded and polemical in subsequent papers.[92] As suggested

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[91] See fn. 51, 269, and footnotes 54, 55-59, 62, and 64 for quotes to this effect.

[92] There are at least six biographical accounts of what led Feyerabend to distance himself from Popper. Agassi (1980, 422-4) claims that Feyerabend was a 'disciple of Popper's' until the student revolution in California in the late 60s: "the move was political, not intellectual" (422). Roy Edgley claims that Feyerabend gradually gave up falsificationism during the 50s as he independently "developed a cluster of ideas that converged with Kuhn's…[t]hese ideas decisively undermined falsificationism and empiricism in general" (Edgley 1996, 155-6).

previously, Feyerabend and Popper had distinct views about the relationship between methodology and scientific practice. However, Feyerabend's disagreement with Popper on quantum mechanics implicitly suggests that Feyerabend had always held this view and it merely remained dormant until the 1970s. After Feyerabend's break with Popper, this is repeated at multiple points:

> [Popper's] aim is no[t] to understand and, perhaps, to aid the scientists; nor is there any attempt to check the version by a comparison with scientific practice. The aim is to develop a special point of view, to bring this view into logically acceptable form (which involves a considerable amount of pointless technicalities) and then to discuss everything in its terms…Not the ever-changing demands of scientific research but the rigid requirements of an abstract rationalism decide about the form and the content of the principles accepted (Feyerabend 1981c, 21).

More specifically, with regards to Popper's arguments on instrumentalism:

> For [Popper] the detailed physical arguments, the many attempts to escape 'instrumentalism' as he calls the final position of the Copenhagen school simply do not exist. The tendency to 'translate' cosmological assumptions into the 'formal mode of speech' and so to conceal their factual content aided this blindness and the resulting rigidity of their philosophers' approach…the breakdown [of falsificationism] will never become visible to a philosopher who hides factual assumptions behind 'logical' principles and 'methodological' standards (Feyerabend 1978b, 87-8).

And, in its most succinct and violent form:

> The ideas of great [scientific] thinkers were distorted beyond recognition by the rodents of neopositivism and the competing rodents of the church of 'critical rationalism' (AM_B, viii).

This stems from the deep-seated disagreement between Popper and Feyerabend about the nature of methodology. Historical examples, for Popper, provide mere elucidations or coincidental parallels between practice and philosophy. Feyerabend's views are much more complicated. In

Watkins claims that Feyerabend became uncomfortable with critical rationalism in 1967 and his turn to anarchism was a "rather desperate [way of] getting Popper off his back" (Watkins 2000, 49). Preston claims that "Feyerabend was, until the late 1960s, a Popperian" (Preston 1997, 11) whose anarchism "represents Feyerabend's belated recognition of the poverty of the normative approach to philosophy he learned from Popper and Kraft" (179). Farrell claims that Feyerabend "remained a Popperian at the meta-methodological level" (Farrell 2000, 264) and "was not a disappointed Popperian but, in many respects, a die-hard pluralistic Popperian" (257). Finally, Oberheim claims that "Feyerabend attempted to keep some critical distance to…Popper's critical rationalism…from the early 1950s" (Oberheim 2006, 105) due to Feyerabend's iconoclastic personality. Other than Farrell, all of these interpretations give different dates and reasons for abandoning falsificationism (cf. Collodel 2016). Since I am focusing on Feyerabend's published works, personal motives do not concern me and I will focus on his published criticisms of Popper.

his early papers, Feyerabend praises the use of history as ‘steps in the right direction’ though it tempered by a ‘methodological backbone’ (Feyerabend 1962b, 67). Indeed, in nearly every publication he ever wrote he uses historical case studies, but never defines their methodological significance. Later, he privileges methodology:

> I really hope that the occasional disparity between [methodology] and actual scientific practice will be regarded as a criticism of the latter, not the former. In the struggle between an ideal and actual reality the ideal must always be given the upper hand. After all, we do not to leave the historical development of a discipline up to chance…we want to shape it, and improve it in accordance with ideas we find reasonable (Feyerabend 1965a, 111).

This seems like a genuine tension since Feyerabend’s *use* of case studies presuppose that they can evaluate methodological views but his stated view on the relationship between history and methodology suggests that historical case studies have *no* methodological significance. He has merely “dull[ed] the scalpels of philosophy by burying them in the historical gravel” as Hanson (1962, 580) once put it.[93] However, Feyerabend’s own escapades into history, I believe, clarified this matter for Feyerabend and the results are to be found in AM_{B1}.

As we have seen, Feyerabend initially thought that forbidding ad hoc hypotheses was a trivial rule since scientists rarely (if ever) use ad hoc hypotheses anyways. As we shall see, Feyerabend’s case study of Galileo shows that ad hoc hypotheses played a crucial role in the development of his mechanics. By $AM_{B,}$ we see Feyerabend make an even more general claim: “[w]e find then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other” (23). My conjecture is that taking a closer look at history forced him to realize that any general account of methodology will have counterexamples. But a ‘counterexample’ cannot simply be an instance in which scientists violated a methodology, since the methodologist can always respond that those scientists were simply acting unreasonably. What Feyerabend *presumes* is that ‘today’s science lovers’ will intuitively assess certain episodes in the history of science as being progressive where ‘progressive’ is a normatively appraised concept according to some pre-theoretical conception of progress. To be clear, Feyerabend is not making the methodological claim that methodologies

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[93] Hanson continues: “There can be no doubt about it, within the history of philosophy illumination has been lost and scattered through clouds of irrelevant historical detail….Indeed, *the* standard “goof off” amongst professional philosophers is to serve up a tray of facts when what is really needed is the sharp scalpel of analysis” (Hanson 1962, 580).

must be compatible with pre-theoretical intuitions, since that would be rationalism. Rather, he uses history to test methodologies while 'test' means something like 'raises critical discussion.' Regardless of the details of this account, this provides a means for making methodology fallible and, if we remember Feyerabend's principle of fallibilism, knowledge about methodology must be fallible and, therefore, should be maximally testable. Allowing historical examples to challenge methodological claims is a step in this direction – a step that Popper never took.[94]

Feyerabend's most in-depth historical case study is Galileo's role in the Copernican revolution. Despite the fact that AM_B received overwhelmingly negative reviews upon its release, one feature that was praised was this case study.[95,96] In the section "The Progressive Role of Ad Hoc Hypotheses", Feyerabend analyzes two ad hoc hypotheses and one 'at least partly' ad hoc hypothesis used by Galileo.[97,98] The first is Galileo's view that *only* relative motion is operative. In Aristotelian kinematics, absolute place, direction, and velocity are all testable; the center of the universe is found by backwardly elongating the direction of two flames. This provides a frame of reference for absolute place. Similarly, distance is determined by the strength of the upward motion of the flames and direction is determined by determining the axis rotation of a stellar sphere (AM_P, 64). Since these measurements don't make sense within Galileo's original kinematics, there is no *initial* way of testing the operative character[99] of relative motion. "The new relativistic principles…are therefore metaphysical, and, because adapted to the tower experiment,[100] also ad hoc" (ibid). The second ad hoc hypothesis concerns the rotation of the earth.[101] Namely, the hypothesis that natural circular motion applies to both supralunar entities and terrestrial objects. This follows immediately from defining the earth as a

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[94] I have been told that Feyerabend read Bartley's *Retreat to Commitment*, wherein Bartley proposes a means to extend the principle of maximal testability to methodology itself, extensively and was thoroughly impressed. This may have influenced Feyerabend in this direction, though I have no textual evidence to support this.

[95] In his autobiography, Feyerabend writes "[Philipp] Frank argued that the Aristotelian objections against Copernicus agreed with empiricism, while Galileo's law of inertia did not. As in other cases, this remark lay dormant in my mind for years; then it started festering. The Galileo chapters of *Against Method* are a late result" (Feyerabend 1995, 103).

[96] Feyerabend's case study of Galileo has attracted a great deal of secondary literature, criticizing some aspects of it and confirming others (Machamer 1973; Chalmers 1985; Thomason 1994). I will assume, for the time being, that the case study is historically accurate.

[97] Previously in AM_P, Feyerabend argues that Galileo's relativity principle was defended partly by "showing how it helps Copernicus; this defense is truly ad hoc" (Feyerabend 1970c, 62).

[98] We can assume that ad hoc hypotheses are hypotheses that lower the testability of a theory.

[99] Feyerabend uses the term 'operative' in the same way Galileo does which is akin to a naive realism. 'Operative motion', therefore, is the motion present in observation.

[100] I will describe Feyerabend's analysis of the tower argument later (see Andersson 1991b for a discussion of this).

[101] The orbital motion of the earth was not ad hoc since it predicted a stellar parallax.

star since stars naturally move in circles. However, this assumption, Feyerabend claims, implied no further testable consequences at the time. It “leaves everything as it is, and it especially leaves the results of the tower experiment and the cannon experiment unchanged” (65). Finally, Galileo’s account of ‘neutral motions’ in *Dialogue*, or motion that is neither caused by natural or external forces, is ‘partly’ ad hoc; it is formulated for methodological reasons (cf. 67-8) and because of its inconsistency with the relative character of motion. The normative *appraisal* of these moves comes in a few steps.

Feyerabend argues that interpreting the tower experiment to be consistent with Aristotelian dynamics would also be ad hoc since the experiment *prima facie* contradicts the motion of the earth.[102] Therefore, we must use some ad hoc interpretation regardless of whether or not we try to refute Aristotelian dynamics. Which interpretation should we choose? Feyerabend writes:

> One wants an interpretation that turns the motion of the earth into a refuting instance of [Aristotelian] dynamics, without lending ad hoc support to the motion of the earth itself. The first step towards such an interpretation is to establish contact, however vague, with the “phenomena”… and establish it in such a manner that the motion of the earth is not *obviously* contradicted. The most primitive element of this first step is to frame an ad hoc hypothesis with respect to the rotation of the earth. The next step would then be to elaborate the hypothesis, so that additional predictions become possible. Copernicus and Galileo take the first and most primitive step. Their procedure looks contemptible only if one forgets that the aim is to *test older views* rather than to *prove new ones*, and if one also forgets that developing a good theory is a complex process that has to start modestly and that takes time (68).

Developing a theory that isn’t *entirely* ad hoc requires some *initial* ad hoc hypotheses. As Feyerabend puts it in AM_{B1}, “ad hoc hypotheses and ad hoc approximations create a tentative area of contact between ‘facts’ and those parts of a new view which seem capable of explaining them” (178). What implications does this have for methodology? Feyerabend writes:

> Why, an impatient methodologist might ask, did it take so long before additional phenomena were added? It took so long *because the domain of possible phenomena had first to be circumscribed by the further development of the Copernican hypothesis*. It is much better to remain ad hoc for a while, and in the meantime to develop heliocentrism in all its astronomical ramifications which can then be used as guidelines for a further elaboration of dynamics (AM_P, 68).

[102] This would be clearly question begging against the Aristotelian but not the contemporary methodologist who argues that Galilean revolution was rational and forbids ad hoc hypotheses.

And, in conclusion:

> Galileo *did* use ad hoc hypotheses. It was good that he used them…as one cannot help being ad hoc, it is better to be ad hoc with respect to a new theory, for a new theory, like all new things, will give a feeling of freedom, excitement, and progress. Galileo is to be applauded because he preferred protecting an interesting hypothesis to protecting a dull one (69).

It should be clear that Feyerabend, starting from at least 1968, came to deeply disagree with falsificationism. The radical nature of Feyerabend's break from Popper will become increasingly apparent in the coming sections on Feyerabend's mature philosophy.

2.6: Concluding Remarks

We can see that the only common view between Feyerabend and Popper are those on maximizing testability. It would appear that this should be sufficient for Feyerabend's adoption of falsificationism given the schema outlined at the end of the previous section. However, let us look at Feyerabend's criticisms of Popper:

(1) Popper misinterprets Bohr as a global instrumentalist. This reveals a general fault in Popper's philosophy: research can test philosophical principles.
(2) Popper disallows 'ad hoc hypotheses' which can be used profitably.
(3) Popper disallows pursuing falsified theories. Historical examples suggest that this would have eliminated many scientific achievements.

The previous section showed Feyerabend's aversion to positions that minimized testability or excluded alternatives. Falsificationism, Feyerabend argues, excludes alternatives by forbidding them the methodological tools necessary to construct successful alternatives. Finally, seeing the limits of philosophical methodology in practice suggests the more *general* view that contingent features of practice may require abandoning *any* methodological principle, no matter how obvious or 'basic' it may be. This discovery will play a crucial role in Feyerabend's more general condemnation of rationalism: his arguments for anarchism in AM.

3. Against Rationalism

Against Method is an exceptionally difficult text to reconstruct for several reasons. First, it is unclear which parts are Feyerabend's and which were the result of editorial decisions that significantly shaped the phrasing of many claims (cf. Motterlini 1999, 290). Second, as Feyerabend asserts, AM is not a book with a clear thesis but a hodgepodge of discussions with no clear attempt to synthesize them into a coherent strand: “AM is not a book, it is a collage” (Feyerabend 1995, 139). Third, each edition of AM could be considered its own book given the substantial number of changes; some chapters were dropped, new ones were added, appendices were removed or decontextualized, some citations are added and some are replaced, and so forth (see Hacking 2010, vii). Finally, Feyerabend’s personality, at times, ran counter to an effort to clarify his position:

> I like to hear that I am an Important Figure, but I also *don’t* like to hear it…Because then I think I should do this and I should do that, write and write, make things clear, correct here, and correct there, make things “perfectly clear” where I have been misunderstood, and for the like of me I do not want to be dragged down into the sewers by tendencies of this kind. I want to live a quiet, peaceful, lazy life (in Motterlini 1999, 281).

It would require an entirely new dissertation to uncover these various strands and how they interrelate. My analysis in this section must be, regrettably, much more simplistic than the text itself. Regardless, in this section I unpack a few interrelated arguments against two opponents, rationalism and dogmatism, and discuss the relationship between these two views.

While Feyerabend never defines rationalism,[103] a definition can be inferred by his usage of the term: Rationalism is the view that there exists some set of exclusive and normative standards by which scientific decision making can be judged and, thereby, provide practical imperatives for action. Consider the original German title of AM: *Wider den Methodenzwang*. The suffix ‘swang’ suggests that the title would more aptly be translated into *Against the Forced Constraint of Method* emphasizing that it is the *forced constraint* that is problematic, not method itself. Additionally, *Methodenzwang* has the connotation that it is singular and unique; there is only one true ‘Method.’ Therefore, even though the term ‘rationalism’ is never defined and sparsely used, the title of the book indicates that it is a central concern in AM. Rationalism is a general view that is central to the practical relevance of empiricism, critical rationalism, ordinary language approaches, transcendental analyses, and most kinds[104] of conventionalism, though I will leave it for future discussion as to what extent this mapping is accurate. Specifically, rationalism comprises the core two theses of each of these positions: there are methodological

[103] The term ‘rationalism’ was frequently used by Popper and, most likely, Feyerabend’s choice of the term is satirize Popper.

[104] I am excluding Le Roy who extended conventionalism to ‘facts’ and, thereby, has a similar to view to Feyerabend.

rules that can constrain theory pursuit on epistemic grounds and these rules have genuine normative content.

The primary argument of AM is historical. In response to a review of AM, Feyerabend consents to Hellman's rendition of the historical argument of AM. Feyerabend writes:

> Take any rationalist criterion of scientific progress (call it C) and any set of methodological rules (call it R) which actually rule something out; then there are non-trivial examples of actual scientific development…satisfying C which not only fail to comply with R but are such that, given real world circumstances, had R been complied with, the progress would not have been made (Hellman 1979, 193).[105]

Feyerabend claims that he does not try to 'establish' this thesis ("how could a thesis such as this possibly be "established"?" (Feyerabend 1979b, 203)), but merely tries to "make it plausible with the help of some examples" (ibid). In other words, this view of history is a *conjecture* that is motivated by historical case studies.[106.107] Given that Feyerabend's responses to reviewers are not entirely trustworthy sources of the content of AM,[108] it must be noted that passages in AM support this reading. Feyerabend writes that given any rule, there are historical instances in which it was profitable to violate the rule. "We find then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time at some time or other" (AM_{B1}, 23). Furthermore, Feyerabend is not merely speculating that there exists a counterexample or two to any theory of rationality, but that rationality is *frequently*

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[105] Nearly every superficial reading of Feyerabend's anarchism that I am aware of takes this line. For a few examples, see Laudan 1996, 17; Preston 1997, 174; Mitchell 2009, 107; Sankey 2011, 566. This view is most bluntly put by Arne Naess, who writes "Feyerabend seems to see himself as the old Nordic god Thor, using 'the hammer of history' to smash false images of science in thunder and lightning. His source of authority: *actual scientific practice*" (Naess 1972, fn. 6 133).
[106] This avoids Feigl's worry that "There can be little doubt that he wishes of service (at least) in a critical and/or advisory capacity to scientists…[b]ut if he is to full the[se] functions, what else can be do but watch…and *extrapolate*?!" (Feigl 1971, 147) since Feyerabend's use of history in AM is not inductive.
[107] Some claim that AM only contains the Galileo case study and hastily generalizes from this case study, which is blatantly false. While this case study is the most in-depth, Feyerabend references many dozens of case studies from many sciences in various degrees of depth and cites literature to substantiate these case studies. Feyerabend also quotes other historians, like Butterfield (AM_{B1}, fn. 2 17), who share the same view of history. Again, Feyerabend is only trying to motivate this conjecture, making the lack of depth excusable.
[108] In his autobiography, Feyerabend writes "[r]eading the reviews, I faced illiteracy pure and simple for the first time. I didn't realize it right away. Having forgotten the details of [AM_B] and being too lazy to check, I often took the critics at their word. So when a reviewer wrote "Feyerabend says X" and then attacked X, I assumed that I had indeed said X and tried to defend it. Yet in many cases I had not said X but its opposite" (Feyerabend 1995, 144-5).

violated. "Without a frequent dismissal of reason, no progress" (179).[109] This conjecture also supports Feyerabend's principles of fallibilism and testability since it shows how often rules of rationality have been 'falsified' and, therefore, plays the dual role of criticizing rationalism and supporting his own principles.

But here we have a tension between Hellman's reconstruction of Feyerabend's view, which is a *counterfactual* view of history, and Feyerabend's claim about the *factual* history of science. "If rationalism is followed, then progress will be maximized" is not refuted by the claim that irrationalism has been followed and was progressive. Feyerabend needs an extra argument that had Galileo, or whoever else, obeyed methodological desiderata they wouldn't have progressed as they did. This conflicts with Feyerabend's view that the history of science is *unpredictable*. Feyerabend writes:[110]

> History generally, and the history of revolutions in particular, is always richer in content, more varied, and more many-sided, more lively and subtle than even the best historian and the best methodologist can imagine. History is full of accidents and conjectures and curious juxtapositions of events and it demonstrates to us the complexity of human change and the *unpredictable character of the ultimate consequences of any given act or decision of men* (emphasis added, 17).

If this is the case, then Feyerabend has no way of saying whether rationalism would have been more or less successful than irrationalism. How can this tension be resolved? It could be the case that Feyerabend's historical conjecture requires that all decision making is *local*, such we can make predictions but only on a case-by-case basis. On this view, we can predict what research will be successful in a given case but not *globally*. On another view, the development of science is unpredictable such that we can *never* predict how research will turn out. I will leave this issue aside for now and return to it in the following chapters. For now, it is worth noting that both resolutions are antithetical with rationalism which presupposes *global* rules of rationality that guarantee (at least to some satisfactory level of probability) future successes.

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[109] "To sum up: *wherever we look, whatever examples we consider*, we see that the principles of critical rationalism…and, *a fortiori*, the principles of logical empiricism…give an inadequate account of the past development of science and are liable to hinder science in the future" (emphasis added, $AM_{B1,}$ 179).

[110] This quote includes segments from Lenin and Butterfield, whom Feyerabend is quoting with approval.

Finally, I would like to point out that rationalism is not equivalent to *dogmatism*. Dogmatism,[111] whether it is grounded in mere arrogance[112] or given a Kuhnian justification,[113] is not a *position* but an *attitude*. Feyerabend's arguments against Kuhn's dogmatism are underdeveloped. He makes the *moral* argument that pluralism is necessary for the development of flourishing individuals. "[A dogmatist] scientific education…cannot be reconciled with a humanitarian attitude. It is in conflict 'with the cultivation of individuality which alone produces, or can produce, well-developed human beings'" (20).[114] He also points to numerous examples of 'theoretical butterflies' who researched pluralistically throughout their careers. Finally, theoretical monism must be counteracted by pluralism to maximize testability. I develop these points in the next chapter, but it is worth pointing out that this kind of dogmatism is distinct from rationalism. One can deny the existence of universal rules of reasoning but still maintain that we should act *as if* such rules existed. While the two may often come hand in hand, as rationalism is often used to justify dogmatism, this is not necessarily so and, therefore, these positions must be kept separate.

I have outlined the historical conjecture of AM and how it refutes rationalism. I have argued that Feyerabend motivates a perspective of history that provides counterexamples to any theory of rationality. Furthermore, the history of science is so chaotic that it unfolds in an unpredictable manner and is filled with accidental discoveries. This also problematizes

[111] Feyerabend often uses 'dogmatism' and 'rationalism' interchangeably. This is confusing since Feyerabend also uses dogmatism in the sense described here. I have used the term 'rationalism' in places where Feyerabend uses the term 'dogmatism' for consistency's sake.
[112] See Kidd (2016) for a discussion of Feyerabend's pluralistic meta-methodology as a kind of virtue epistemology meant to combat ignorance and arrogance.
[113] One of Kuhn's central theses is that dogmatism, defined as "preconception [of puzzles] and resistance to innovation" (Kuhn 1963, 349), is a necessary condition for scientific progress. This isn't a result of individuals being overly ardent, but a *structural feature of communities*. During revolutions, where scientists act pluralistically, progress seems hard to come by. Since scientists must appeal to more heterodox audiences at conferences since there are no well-defined methods, or standards experiments are interpreted in a bewildering amount of ways, theories are constantly struggling to find their feet (e.g., during pre-paradigmatic electricity, for example, "electrical investigations tended to circle back over the same ground again and again. New effects were repeatedly discovered, but many of them were rapidly lost again" (355), and so on. During normal science, however, where paradigms set the conditions for how to interpret experimental results, what techniques and methods are acceptable, what kind of entities exist (or could exist), and so forth, "that progress both seems obvious and assured" (Kuhn 1962, 162). Because of these reasons, "it is often better to do one's best with the tools at hand than to pause for contemplation of divergent approaches" (Kuhn 1959, 225). This list of reasons is, obviously, cursory and not exhaustive. Kuhn also spends a great deal of time talking about the productive function of textbooks which *presuppose* historical accomplishments rather than having them open to students for criticism and the methods by which scientists learn how to discern 'outdated' works and not revisit them (Kuhn 1963, 359-68). See also Cohen (1952) and Barber (1961) for similar arguments.
[114] The quote within this quote is from Mill.

rationalism, which requires that theories of rationality can anticipate how knowledge will develop and, thereby, develops methods to reach such goals. Instead, we need some new view of methodology that allows us to flourish while being surrounded by such uncertainty. The view Feyerabend believes fits this bill will be developed in the next chapter.

4. Chapter Summary

In this chapter, I have reconstructed most of Feyerabend's critical philosophy. I have argued that there is a great deal of continuity from Feyerabend's earliest publication to AM_B whereby Feyerabend criticizes instantiations of rationalism. Nearly all Feyerabend's published papers that criticize various kinds of empiricism show how these positions presuppose posits that are beyond critical discussion. More poignantly, these positions assume posits that are infallible and, therefore, not testable. Feyerabend's criticisms of falsificationism, specifically Popper's falsificationism, are grounded in Feyerabend's extension of the principle of fallibilism to falsificationism itself. Feyerabend shows how Popper's method of conceiving methodology makes it such that it cannot be refuted by historical considerations. This leads to Popper's inappropriate application of his views of realism and falsificationism. I then reconstructed the historical conjecture at the center of Feyerabend's criticism of rationalism in AM. This historical conjecture represents a general argument against any instance of rationalism, whereas his previous arguments only dealt with individual instances of rationalism. As such, while the historical conjecture is a novel argument in Feyerabend's corpus, it certainly builds upon a cohesive perspective that emerged in the previous 30 years of his career. While the principles of testability and fallibilism give us the first steps towards understanding Feyerabend's positive philosophy, his pluralism, which embodies the core of his philosophy of science, will be outlined in the following chapter.

Chapter 2
Feyerabend's Pluralism: Proliferation, Tenacity, and Anarchism

> I am convinced that the unwritten knowledge scattered among men of different callings surpasses in quantity and in importance anything we find in books, and that the greater part of our wealth has yet to be recorded (Leibniz 1951, 46-7).

> I favor any *skepsis* to which I may reply 'Let us try it!' But I no longer wish to hear anything of those things and questions that do not permit any experiment. This is the limit of my 'truthfulness'; for there courage has lost its right (Nietzsche 1882, §51).[115]

In this chapter, I reconstruct Feyerabend's pluralism and contrast this reading against those in the secondary literature. As we saw, Feyerabend's rejection of rationalism occupied the majority of his critical philosophy. While some think that this exhausts Feyerabend's contribution to philosophy of science, Feyerabend also dedicated a great amount of effort into formulating his own pluralist view of methodology. In this chapter, I detail the principles of proliferation and tenacity, which jointly constitute Feyerabend's pluralism, their evolution throughout Feyerabend's career, and their relationship to Feyerabend's humanitarianism and epistemological anarchism.

The structure of this chapter is as follows. In the first section, I trace the development of Feyerabend's principle of proliferation and formulate the mature version of it found in AM_B. On my account, proliferation began with Feyerabend's argument for incommensurability where we proliferate theories to maximize the testability of other theories. In 1965, when Feyerabend first formulated the principle explicitly, 'theories' becomes redefined to be more expansive and the notion of a 'test' becomes redefined as contrastive notion. Additionally, Feyerabend 'clarifies'

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[115] There are fascinating parallels between Nietzsche's 'experimentalism' and Feyerabend's pluralism. "Again we see that science is for Nietzsche not a finished an impersonal system, but a passionate quest for knowledge, an unceasing series of courageous experiments….that we cannot dodge without betraying the scientific spirit of inquiry" (Kauffman 1950, 75). The series of experiments is pluralistic to avoid dogmatism: "For I treat deep problems as I would a cold swim - quickly into them and quickly out again. That in this way one does not get…deep enough down" (Nietzsche 1882, §381) and that the philosopher should "boldly at any time…declare himself against his previous opinion" (§296). See Bearn (1986) for a somewhat extended discussion of this comparison.

his views on incommensurability such that incommensurability is no longer a necessary or sufficient condition for proliferation. I then show how the method of proliferation becomes unconstrained, and show how the object of proliferation (i.e., the thing being proliferated) similarly becomes unconstrained. I close by highlighting the connection between proliferation and Feyerabend's humanitarianism and possible empirical constraints on proliferation. In the second section, I accomplish the same task with the principle of tenacity. I contrast Feyerabend's understanding of tenacity with Kuhn as well as Lakatos' and show how his mature view has no conditions of acceptance or termination. That is to say, we can pursue any idea we want for as long as we want. I then show how the principles of proliferation and tenacity cooperate. In the following section, I detail Feyerabend's anarchism. After showing that there are two possible interpretations of anarchism, I show how Feyerabend's anarchism emerges from his pluralism. I then try to make sense of Feyerabend's claims that anarchism is conducive for progress conceived in *any way* and suggest some a-rational constraints that prevent us from interpreting 'anything goes' literally. In the last section, I compare my interpretation of Feyerabend's pluralism with the interpretations in the secondary literature.

1. The Evolution of Proliferation

In this section, I trace the development of Feyerabend's views on proliferation and formulate his mature version from the 1970s. I argue that as Feyerabend's understanding of proliferation broadens, he gradually loosens the epistemic constraints of its earlier incarnations resulting in an epistemically unconstrained principle. Specifically, the mature version of the principle of proliferation has no constraints on the content of what is proliferated, the method by which it is proliferated, or the time at which it is proliferated. The only constraints that are given for the principle of proliferation are *empirical* and *ethical*.

1.1: Feyerabend's Initial Arguments for Incommensurability

Feyerabend's views on incommensurability have received a great deal of attention. While Feyerabend hints at incommensurability in his early papers, his first extensive discussion appears in his classic "Explanation, Reduction and Empiricism" (1962b). In this section, I show the connection between incommensurability and empiricism, Feyerabend's historical case studies, the connections between incommensurability and testability, and methods of theory comparison.

In that paper, Feyerabend targets empiricist views of reduction and explanation summarized as this: "Only such theories are admissible (for explanation and prediction) in a given domain which either contain the theories already used in the domain, or are at least [logically] consistent with them" (55).[116] In other words, the laws of the successor theory are consequences of the laws of the predecessor theory. Feyerabend never straightforwardly defines what he means by a 'theory', but merely says the following:

> Empirical generalisations are statements, such as 'All A's are B's'…which are tested by inspection of instances [of A's and B's]. Universal theories…are not tested in this manner. Roughly speaking their tests consists of two steps: (1) derivation, with the help of suitable boundary conditions, or empirical generalisations; and (2) tests, in the manner indicated above, of these generalisations (44).

Here, we see that theories must be *universal* in that they make claims about some aspects of everything in their domain. Additionally, we can safely assume that Feyerabend held a syntactic and holistic view of theories; theories are sets of propositions where the meaning of each proposition is dependent on the theoretical context. This view of theories, when combined with the empiricist rule that successor theories must be consistent with their predecessors, assumes 'the principle of meaning invariance' (47), the view that the meaning of observation terms is consistent in T and T′. This rules out the introduction of theories that are either logically inconsistent or incommensurable theories, defined as theories that lead to "a *replacement* of the ontology…of T′ by the ontology…of T, and a corresponding change of the meanings of the descriptive elements of the formalism of T′" (44-5).[117] In other words, the ontological extension

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[116] Hempel: "every logically consistent observation report [should be] logically compatible with the class of all the hypotheses which it confirms" (Hempel 1945, 45) and observation reports should "not confirm any hypotheses which contradict each other" (105). Feyerabend also cites Mach who claims that "[c]onsidering that there is, in a purely mechanical system of absolutely elastic atoms, no real analogue for the increase of entropy, one can hardly suppress the idea that a violation of the second law…should be possible if such a mechanical system, were the real basis of thermodynamic process" and, according to Feyerabend, "he insinuates, for this reason, the mechanical hypotheses must not be taken too seriously" (Feyerabend 1962b, 56). This reading conflicts with Feyerabend's later papers on Mach (cf. Feyerabend 1970a) where he interprets Mach's skepticism about atomism resulting from the problems pre-Perrin statistical mechanics, not positivism. Similarly, Feyerabend cites Born who writes that "[i]f any future [quantum] theory should be deterministic…it cannot be a modification of the present one, but must be entirely different. How this should be possible without sacrificing a whole treasure of well-established results I leave to the determinist to worry about" (Born 1949, 105). While Born is certainly hostile to determinism, he never claims that it is impossible but merely that it is difficult. Born and Mach, therefore, seem to be unfair targets of this criticism. Later on, Feyerabend argues that incommensurability is incompatible with progress understood as (linear) increases in empirical content or verisimilitude (Feyerabend 1970d, 220-9; 1978a, 67-8; 1981b, 16; 1981c, 23).

[117] Feyerabend addresses the argument that a *complete* semantic shift is impossible since it needs some idiom to translate the old language into the new making one partially semantically dependent on the other since, as mentioned in Chapter 1, theories can be learned *directly* without any idiom. This anticipates Davidson's (1973) objection.

of T is zero from the perspective of T′ (i.e., it has no referent).[118] Feyerabend thinks the principle of meaning invariance can be interpreted as, "either a description of actual scientific practice, or as a prescription which must be followed if the scientific character of the whole enterprise is to be guaranteed" (48). The remainder of this paper is dedicated to showing that this view is neither accurate nor desirable.

In this paper, Feyerabend provides three counterexamples to the principle of meaning invariance: from Galileo to Newton, from Buridan's impetus principle to Galilean momentum, and from the phenomenological to the kinetic theory of heat.[119] I will briefly outline the first two, and spend more time on the third since it reappears throughout Feyerabend's corpus (Feyerabend 1963a, 92-3; 1964b, 205; 1966a, 246-7; 1966c; 1969a, 157; 1972, 140-2; AM_{B1}, 39-40; 1981a, 144-5)[120] and has received the most attention (Laymon 1977; Couvalis 1988a). Feyerabend outlines the first example rather quickly. If 'T' denotes Galilean laws of motion of material bodies, and 'T′' represents Newton's celestial mechanics, Nagel's view of reduction and explanation demand that: T′ & d ⊢T, where 'd' expresses initial conditions valid inside T (e.g., no air resistance, no rotation of the earth, etc.). However, the predictions for T and T′ will be quantitatively distinct. Galilean laws of motion assume that vertical accelerations close to the surface of the earth are constant over a finite interval but Newton's laws dictate that accelerations will decrease.[121] To establish a deductive relationship between T and T′, d must be replaced with a false statement such as "the conditions in the close neighborhood of the earth [lead to] a vertical acceleration that is constant over a finite interval of vertical distance" (Feyerabend 1962b, 58) or by formulating a new set of inconsistent and experimentally indistinguishable laws from T′. This example, however, only shows a *logical inconsistency* between T and T′ and not *incommensurability*.

[118] Feyerabend is clearer about this in another paper: "we shall diagnose a change of meaning either if a new theory entails that all concepts of the preceding theory have zero extension or if it introduces rules which cannot be interpreted as attributing specific properties to objects with already existing classes, but which change the system of classes itself" (Feyerabend 1965c, 98).
[119] Feyerabend adds classical celestial mechanics and special relativity later (Feyerabend 1970c, 220-2).
[120] The argument appears nearly *verbatim* in each instance so there is no development of his view.
[121] Popper (1957, 202) similarly writes, "Newton's theory unifies Galileo's and Kepler's. But far from being a conjunction of these two theories…it corrects them while explaining them…Far from repeating its explanandum, the new theory contradicts it and corrects it." This claim was originally made by Duhem (1906, 159) which was underlined in Feyerabend's personal copy (Oberheim 2006, 176).

The next example, and first of genuine incommensurability, is of Aristotelian astronomy and Newtonian celestial mechanics. Aristotle's impetus principle can be stated thusly:

> (1) Motion is a process arising from the continuous action of a source of motion, of a 'motor', and a 'thing moving.' The source of motion or motor is a force - either internal as in natural motion or external as in non-natural motion - which during motion must be in contact with the thing moved (Clagett 1957, 424).

(1) can be characterized kinematically, as spaced transversed, or dynamically. Since the dynamic interpretation is more 'empiricist' (cf. Feyerabend 1962b, 63-4), Feyerabend rephrases (1) as this:

> (1') The impetus of a body in empty space which is not under the influence of any outer force remains constant (Feyerabend 1962b, 64).

Since this principle is well confirmed,[122] it is a candidate for reduction. The closest analog to the impetus principle in Newton's theory is the conservation of momentum. However, these principles are qualitatively distinct. Impetus causes motion; momentum is the result of motion. Momentum can occur without any causes; impetus requires an active force. Finally, in Aristotelian dynamics, a body stays at rest in its natural place without any force whereas being at rest, in classical mechanics, requires the "explicit denial of a force such as the impetus is supposed to represent" (65). Furthermore, we cannot construct any 'parallel notion' of impetus that is "explicable in terms of the theoretical primitives of [classical] science" (Nagel 1961, 302). Any parallel notion that gives non-zero values must assume that inertial movements occur in a resisting medium, which is inconsistent with the classical assumption that inertial motion happens in empty space. Therefore, "the concept of impetus, as fixed by the usage established in the impetus theory, cannot be defined in a reasonable way within Newton's theory [since] the usage involves laws, such as [(1')], which are inconsistent with Newtonian physics" (Feyerabend 1962b, 66). Notice the difference between this case study and the previous one. Impetus has zero extension in classical mechanics (though it may serve as a useful approximation). The Galilean law of free fall could be derived from Newton's laws, since the terms required for its formulation have the same meaning (e.g., 'mass', 'force'), but the stipulation that all bodies fall at a constant acceleration is empirically false. As such, Galilean physics and Newtonian celestial mechanics are *inconsistent*, but not *incommensurable*.

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[122] Feyerabend writes that this principle can "easily be supported by such common observations as a cart drawn by a horse and a chair pushed around by an angry husband" (Feyerabend 1962b, 62-3).

Feyerabend's favorite example of incommensurability comes from his case study of Brownian motion. Feyerabend argues that Brownian motion could not refute the second law of classical thermodynamics without the kinetic theory of heat.[123] We now get a distinction between simply pursuing theories that conflict with the principle of meaning invariance and pursuing incommensurable theories for the sake of maximal testability. Here, Feyerabend distinguishes between direct and indirect refutations. Direct refutations require investigating the observational consequences of thermodynamics. For Brownian motion to be a direct refutation, Brownian particles need to be a perpetual-motion machine of the second kind (71-2). Discovering this observationally requires measuring the exact motion of the particle and the temperature and heat transfer in the surrounding medium. Both of these are 'beyond experimental possibilities' since they introduce heat and energy into the system.[124] Instead,

> The *actual* refutation of the second law was brought about in a very different manner. It was brought about via the kinetic theory and Einstein's utilization of it in the calculation of the statistical properties of the Brownian motion. In the course of this procedure the phenomenological theory (T) was incorporated into the wider context of statistical physics (T′) in such a manner that [the principle of meaning invariance] was violated, and then a crucial experiment was staged (Perrin's investigations) (72).[125]

Here, Brownian motion becomes evidence of atomism only *after* the kinetic theory. However, Feyerabend's presentation is somewhat misleading. Feyerabend claims that theories never compete with facts, but with other theories (ibid). If this is true, it seems as if there are no such things as 'direct tests.' Remember, for Feyerabend, theories are necessary for the *validity* and *meaning* of observation statements. So let's say we have two theories, T and T′, that both entail an observation statement expressible in the term of their theory. So classical thermodynamics, T, and T′, the kinetic theory, agree that we see Brownian motion (O). However, T interprets O as meaning "There is some random (seemingly) perpetual movements of pollen grains" whereas T′

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[123] Feyerabend breaks this statement down into two distinct theses: the historical thesis that Brownian motion would not have been *considered* a refuting instance of classical thermodynamics and the methodological thesis that Brownian motion *cannot* be a refuting instance. For the former, Feyerabend merely conjectures that Brownian motion would be regarded as an 'oddity.' Feyerabend focuses on the latter question in this paper.

[124] Feyerabend recognizes that a large fluctuation in heat transfer could be measured. However, such events are "rare, not repeatable, and therefore *prima facie* suspicious" (Feyerabend 1962b, 72).

[125] Feyerabend extends this argument to quantum theory whereby "the refutation of the quantum-mechanical uncertainties presupposes just an incorporation of the present theory into a wider context, which is no longer in accordance with the idea of complementarity and which therefore suggests new decisive experiments. And it may also be that the insistence…upon [the principle of meaning invariance] will, if successful, forever protect these uncertainties from refutation" (72-3). Lakatos (1978b, 204-5) extends this argument to the photo-electric effect.

interprets O as "Here is evidence for the existence of atoms." T and T′, in this case, conflict since T is also committed to the continuum hypothesis.[126] Without T′, O would have simply have been an oddity for T; with T′ it is a refuting instance. This test model presupposes that O is *expressible* in both T and T′, though its *validity* will differ with T and T′.[127]

Feyerabend generalizes this procedure as the following:

(1) Assume theory T has observational consequence C, but the factual state of affairs is C′ where C and C′ are observationally indistinguishable.
(2) C′ and not C, causes macroscopic process M which is observable.
(3) M refutes T, but this can only be shown if C′ is connected to M.
(4) Connecting C′ to M requires a theory T′ which must be independently confirmed and can be a 'satisfactory substitute' for T
(5) ∴ having M refute T requires T′ (cf. Feyerabend 1965b, 176).

Or, diagrammatically put:

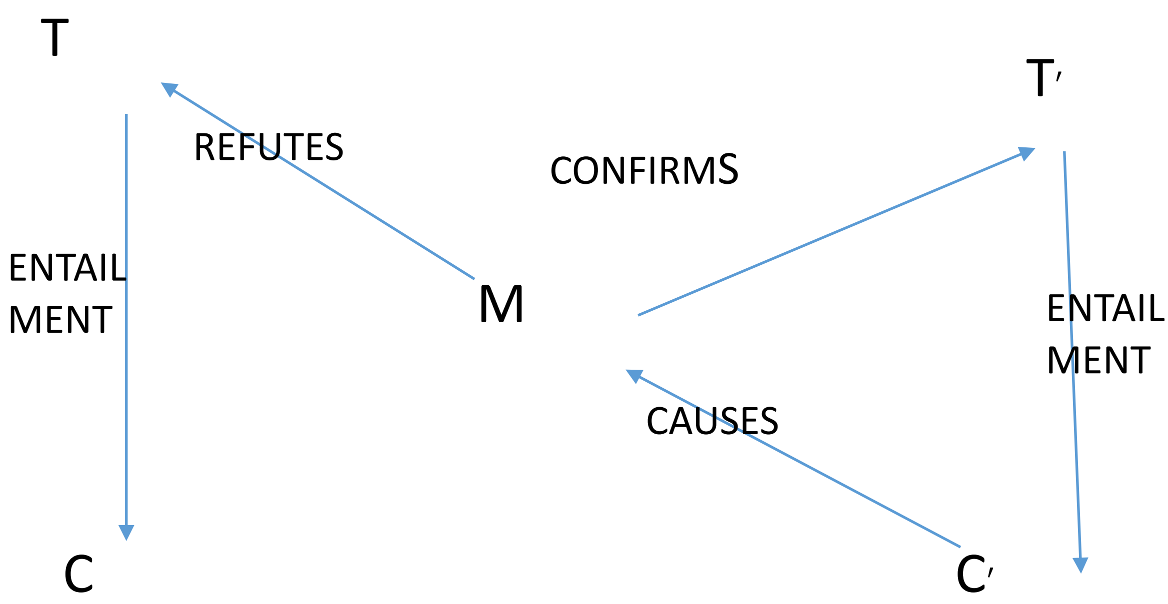

Figure 1: Feyerabend's Pluralistic Test Model

[126] However, as Poincaré (1963) points out, the atomic hypothesis (pre-Perrin) could mathematically regain classical thermodynamics and remain an 'indifferent hypothesis.' This is irrelevant for our current discussion since Feyerabend is presupposing that we interpret theories realistically.

[127] I have not covered Feyerabend's fairly brief argument concerning the use of 'subjective factors' for comparing incommensurable theories. Townsend (1970) provides an illuminating discussion on this feature of another argument Feyerabend makes in this paper argument (though he presents it as the *sole* argument of the paper). Feyerabend also briefly discusses this method briefly in his 1958c, pg. 31-2.

This is only true *if we interpret both theories realistically* since we must be able to posit that C′ is causally connected to observables. Since interpreting theories realistically is a choice, incommensurability is not a historical fact, except insofar as scientists happened to have interpreted theories realistically, but a decision. Regardless, the structure of Feyerabend's argument is subtle and often misrepresented. Couvalis, for example, writes:

> Einstein made the incredible prediction that the average distance travelled by the Brownian particle increases as the square root of the elapsed time (the displacement equation). Such a prediction would not have been (even approximately) confirmed unless the theoretical assumptions it was based on were true or almost true. That is, the fact that the Brownian particle behaves in (approximately) the way Einstein's displacement equation predicts would have to be regarded as a miraculous coincidence if the Kinetic Theory were not true or almost true (Couvalis 1988, 419).

This is blatantly anachronistic, since Feyerabend never invokes abductive reasoning, but it does point to a hole in Feyerabend's argument: why do theories need to be 'factually adequate' to test other theories? After all, the *postulation* of T′ changes the evidentiary status of O. Feyerabend never answers this in this paper but, as we shall see, it becomes addressed accidentally in the development of the principle of proliferation.

To summarize, while Feyerabend never uses the term 'proliferation' in this paper, it is clear that Feyerabend is applauding the process of proliferating theories. The maxim to proliferate is built into his pluralistic test model. We have been given two justifications for proliferating inconsistent theories: maximizing testability and providing means of theory comparison. Testability is maximized by proliferating inconsistent theories that we 'take seriously' which can prompt revisions to our accepted theories. Theory comparison comes from proliferating an incommensurable theory and comparing the relative advantages and disadvantages each has. Hopefully, these points will become clearer as we move along.

1.2: The First Formulation of the Principle of Proliferation

The first formulation of the principle of proliferation appears in "Reply to Criticism: Comments on Smart, Sellars and Putnam" which is meant to be an extension of his 1962b (Feyerabend 1965a, fn. 6 106). In this section, I outline Feyerabend's new view of testability, his 'clarification' of incommensurability, and his formulation of the principle of proliferation.

Before discussing the principle of proliferation, we must consider the objections Feyerabend addresses in this paper. Shapere phrases the basic worry as this:

> In order for two sentences to contradict one another one must be the denial of the other… and this in turn is to say that the theories must have some common meaning. On the other hand, two sentences which do not have any common meaning can neither contradict, nor not contradict, on another (quoted in Feyerabend 1965a, 115).[128]

This is the same worry as expressed I Achinstein (1964) and which forced Feigl to give "incessant arguments [for a] more detailed theory of test" (fn. 31 116). Feyerabend admits this is "entirely correct [and] indicates that the use of incommensurable theories for the purpose of criticism must be based on methods which do not depend on the comparison of statements with identical constituents" (ibid).[129] This concession is much more significant that Feyerabend appears to have realized. If this is the case, incommensurable theories cannot be used for his pluralistic test model as developed in his 1962b. Recall, I argued that Brownian motion is *semantically* common to classical thermodynamics and the kinetic theory, but the realistic *interpretations* conflict. As such, maximized testability does not come from *incommensurable* theories but from "*partly overlapping [but] mutually inconsistent*" (Feyerabend 1962b, 72) theories.

Feyerabend goes on to argue that there are tests between genuinely incommensurable theories that are 'readily available.' He provides three such means:

(1) Compare the structures of infinite sets of elements and see whether there is an isomorphism or not (cf. Feyerabend 1965c, 102-3).
(2) Compare theories via their 'local grammars' ("that part of a [statement's] rules of usage which is connected with such direct operations as looking, uttering a sentence

[128] This quote comes from Shapere's letter to Feyerabend (Feyerabend 1965a, fn. 28 115) (cf. Shapere 1964).
[129] Despite this explicit acknowledgement, and subsequent development of views of theory comparison, Feyerabend retains the testability argument throughout his career. I think that this is a mistake on Feyerabend's part. My guess is that Feyerabend has in mind the *psychological* feature alternatives have of 'suggesting', a vague term Feyerabend often uses, new tests. Further evidence for this explanation comes in a footnote in AM_{B1} where Feyerabend compares Brownian motion to Ehrenhaft's experiments that were ignored and considered a mere oddity since it did not have any alternative theoretical explanation (AM_{B1}, fn. 5 39-40; cf. chapter 4 of Oberheim (2006) for a description of Ehrenhaft's influence on Feyerabend). This suggests that alternatives *motivate* new problems for other theories, but it does not add up to the logical claim that Feyerabend is attempting to make. Lakatos claims that Feyerabend only ever stressed this psychological feature of alternates, despite Feyerabend's insistence that he is making a logical point (Feyerabend 1972, fn. 23 146). I think that Lakatos was correct on this point. Alternatively, it could just be pure sloppiness. Feyerabend writes "[w]hen writing a paper I have usually forgotten what I wrote before and application of earlier arguments is done at the applier's own risk" (AM_{B1}, 114).

in accordance with ostensively taught (*not* defined) rules") (Feyerabend 1965a, fn. 32 116).

(3) Construct a model of a theory 'T' within its incommensurable alternative 'T′' and 'consider its fate' (115).

(2) was elaborated in his 1962b. This doesn't require comparing the *meaning* of two statements, but the behavior of scientists. (1) and (3) are novel, but not developed in this paper. But this list is not exhaustive of means of testing theories. Feyerabend also provides several more means of comparison including formal criteria (simplicity, coherence, use of approximations) and informal criteria (consistent with established theories, metaphysical intuitions) (Feyerabend 1963a, 81). These criteria *are all about theory comparison*, rather than providing direct tests. But is comparison really a kind of test?[130] Feyerabend never answers this question but what he seems to have in mind is the *competition* between theories that *leads to* tests. "Knowledge….is an ever-increasing ocean of alternatives each of them forcing the others into greater articulations, all of them contributing, via this process of competition, to development of our mental faculties" (107). This is a *causal claim* about diversity and our critical faculties. It is therefore more appropriate to use the term 'criticism', a more psychologistic notion, than 'tests' in cases of theory comparison. When we proliferate incommensurable theories, our purpose is to maximize *criticism*.

We now are able to understand Feyerabend's principle of proliferation, stated thusly:

> **Principle of Proliferation:** "Invent, and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted" (105).

There are four features of proliferation that are on display in this formulation. First, proliferation requires *inventing* theories; it is an activity. Proliferation remains true even when theoretical monism is in place. Second, we must *elaborate* these theories rather than simply positing them. Third, Feyerabend says we are to proliferate *theories*. Until now, Feyerabend has been quite cagey as to what he means by a 'theory.' In this paper, Feyerabend writes the following:

> When speaking of *theories* I shall include myths [cf. Feyerabend 1961b, 57-8], political ideas, religious systems, and I shall demand that a point of view so named be applicable to at least some aspects of everything there is… I prefer this 'accordion' use of the term

[130] It should be noted that such arguments are available (see Dawid et al. (2015)), but Feyerabend never develops similar arguments.

> because it provides a single name for problems which in my presentation are intimately related (fn. 5 105).[131]

There are two remarkable features of this extremely loose definition. Theories do not have to be factually adequate; myths, religious systems, and political ideas are descriptions of the world that need not be testable and, therefore, are not the kind of entities that can be factually adequate. Second, theories must be universal within their domain of applicability (i.e., they say something about each object in their domain). After all, if this were not the case, they would not be competing with each other. Finally, as we already know, proliferated theories must be inconsistent with those previously in place.

Feyerabend then provides some additional justifications for the principle of proliferation, which are passed over in his 1962b:

(1) No theory ever agrees with the available evidence with *complete accuracy*. Proliferating alternatives which explain these quantitative inaccuracies "on the basis of new principles will lift them out of the background and deviational noise and then turn them into an *effect* that is capable of *refuting* the [alternative] scheme" (fn. 7 106).
(2) No theory ever agrees with all of the facts within its domain.
(3) Pluralism has psychological advantages; "a mind… immersed in the contemplation of a single theory may not even notice its most striking weaknesses" (ibid).

(1) and (2) involving using alternatives to *accentuate* difficulties with theories. This is a point Feyerabend makes because of Kuhn: the *significance* of facts change in light of alternative explanations. Consider Mercury's perturbations at its perihelion, a fact that was well-known since the 1840s, but became a 'crucial experiment' *after* Einstein's alternative explanation of it. What was, originally, a minor inaccuracy became a significant refutation.[132] (3) was also discussed in his 1962b and, essentially, repeats the argument for maximal criticism.

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[131] Additionally, Feyerabend says that there are 'certain similarities' between his notion of a theory and Quine's sense of 'ontology', Carnap's 'linguistic framework', Wittgenstein's 'language game', Pareto's 'theory', Whorf's 'metaphysics', and Kuhn's 'paradigm' without detailing what the similarities (and differences) are. In his 1965b, Feyerabend defines "the term 'theory' in a wide sense, including ordinary beliefs (e.g. the belief in the existence of material objects), myths (e.g. the myth of eternal recurrence), religious beliefs, etc. In short, any sufficiently general point of view concerning matters of fact will be termed a 'theory'" (Feyerabend 1965b, fn. 3 219). Later, in Feyerabend 1970d (fn. 2, 203), Feyerabend writes that he is "inclined to stick to my own and much vaguer term 'theory'…which covers both Lakatos's 'theories' and 'research programs.'"
[132] It is unclear to me why Feyerabend thinks this is the case, logically speaking.

Thus far, we have talked about proliferation as a means to maximal testability and criticism. However, we have also struggled with Feyerabend's insistence that proliferated theories must be 'factually adequate.' I think that this issue becomes implicitly resolved in Feyerabend's discussion of proliferating 'strong alternatives', which are "especially well suited for the purpose of criticism" (110), which have five features:

(1) A new theory, T′, must contain assertions over and above the prediction which lead to the contradiction between T and T′.
(2) This 'excess content' must be "more intimately connected than by mere conjunction" (109).
(3) There need to be some *possible* independent reasons to accept T′…
(4) T′ should be able to explain the success of T.
(5) T′ must be coherent (ibid).

Strong alternatives, on this picture, *succeed* their predecessors. But Feyerabend is invoking criteria for *accepting* theories, not conditions for *pursuing* them. This relocates the 'factual adequacy' condition of the 1962b to *special cases* of proliferation where we intend to *supplant* theories rather than merely using them for testability or criticism. In other words, the factual adequacy condition is (logically) irrelevant testability or criticism, but pertinent for accepting a new theory. Indeed, in his 1962b, only Feyerabend's case study on Brownian motion illustrates the argument for maximal testability. However, his case studies on momentum and Galileo's law of free fall at least imply, if not outright assume, that pursuing incommensurable theories is a perfectly reasonable activity. Thus proliferation serves three independent purposes: maximizing criticism, maximizing testability, and loosening the constraints on theory pursuit.[133]

In this section, I have largely interpreted this paper as clarifying some of the confusing arguments Feyerabend makes in his 1962b. The first, and perhaps most important, is that incommensurability plays no role in the argument for maximal testability; in fact, it *cannot* play any role in this argument. Additionally, while we may pursue incommensurable theories for the sake of maximal criticism or because we think they may be successful in their own right, incommensurability is a mere accidental property of these theories. In other words, a theory need not be incommensurable to be pursuitworthy or for maximizing criticism. Despite incommensurability being one of Feyerabend's most notorious and thoroughly discussed notions,

[133] As I argued in the previous chapter, Feyerabend implicitly conceived of methodology as applying to theory *pursuit* not theory *acceptance* and never straightforwardly separates the two. See Barseghyan (2015, 30-43) for discussion.

it plays surprisingly little role in his argument for the principle of proliferation. Another clarification is the role 'factual adequacy' plays in proliferation. Only strong alternatives, or candidates for *acceptance* must be factually adequate. Other proliferated theories must only be formally inconsistent, and comply with Feyerabend's rather loose definition of a 'theory.' Finally, we have a first explicit formulation of the principle of proliferation.

1.3: Counterinduction and Natural Interpretations

In AM$_{P}$,[134] Feyerabend repeats the principle of proliferation *verbatim* from his 1965a. Additionally, the justification remains the same as it was 1965 except that he now extends proliferation to being an "essential part of a humanitarian outlook" (26-7). What is developed in AM$_{P}$ is the *method* by which we proliferate. Another way of putting this is the ways by which we *construct* theories. In this section, I outline Feyerabend's methodological pluralism which complements the principle of proliferation.

For the falsificationist, we construct theories by devising conjectures that are compatible with established facts. A falsificationist principle of proliferation would 'invent' theories by dreaming up structures the world might have and it would be 'elaborated' by bringing that structure into a self-consistent theory that is consistent with established facts. For the empiricist, we gather facts and construct generalisations. We do not, strictly speaking, 'invent' theories but derive them from facts. They are 'elaborated' in the process of collecting facts and establishing their relationships towards a theory. In both cases, we have a 'logic' of how to proliferate. Feyerabend, on the other hand, provides no such rule. In AM$_{P}$, however, he formulates the notion of 'counter rules' "which oppose some familiar rules of the scientific enterprise" (AM$_{B1}$, 29). This is to "increase the inventory of rules" (Feyerabend 1979b, 203-4). He does not, thereby, reject these methods of proliferation but merely wants to show the function of their opposite. While Feyerabend never details what he means by 'counter rules', it clearly derives from a pluralistic understanding of methodology. Feyerabend suggests that for *any* rule, we can devise a 'counter rule', which entails that there is *no* limit on the method by which we can proliferate. Why? Because all methods, Feyerabend argues, are *inherently limited*. He writes:

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[134] The argument given here is repeated, nearly *verbatim*, in Feyerabend (1970f).

> My intention is not to replace one set of general rules by another such set: intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits*. The best way to show this is to demonstrate the limits and even the irrationality of some rules which she, or he, is likely to regard as basic. In the case of induction (including induction by falsification) this means demonstrating how well the counterinductive procedure can be supported by argument (AM_{B1}, 32).

Feyerabend's unfortunately brief discussion leaves us with little reason to accept that all methods are inherently limited. However, as we proceed into Feyerabend's defense of counterinduction, some clarity will be shed on this issue.

In AM_P and AM_B, Feyerabend focuses on counterinduction which is, obviously, the counter rule to induction. Feyerabend defines this as:

> The rule that "experience," or "the facts," or "experimental results," or whatever words are being used to describe the "hard" elements of our testing procedures, measure the success of a theory, so that agreement between the theory and "the data" is regarded as favoring the theory…while disagreement endangers or perhaps even eliminates it…Taking the opposite view, I suggest introducing, elaborating, and propagating hypotheses which are inconsistent either with well-established theories or with well-established facts (ibid).

The defense of counterinduction should be familiar: "Evidence that is relevant for the test of a theory T can often be unearthed only with the help of an incompatible alternative theory T′." (ibid). A major change in AM_P is Feyerabend's new understanding of evidence. He repeats the claim, from his 1965a, that in any historical situation, "no single theory ever agrees with all of the known facts in its domain" (36). He distinguishes between quantitative and qualitative failures and provides several examples.[135] However, most scientific theories are pursued by

[135] Feyerabend's provides specific examples of quantitative failures such as D.C. Miller's 'decisive refutation' of special relativity, general relativity's inability to explain about 10'' in the movement of the nodes of Venus and more than 5'' in the movement of the perihelion of Mars, and more vague examples such as the 'considerable difficulties' of Newton's theory of gravity, the Copernican view at the time of Galileo was "inconsistent with facts so plain and obvious that Galileo had to call is "surely false"", and Bohr's atomic model was "introduced and retained in the face of very precise and unshakable contrary evidence" (AM_P, 37). For quantitative failures, he cites Aristotle's notion of a continuum that was beset by the paradoxes raised by Zeno and Parmenides until the early 20th century, the contradiction between Newton's ray theory of light and mirror images, the implication that the motion of a free particle is self-accelerated in classical electrodynamics, and Ehrenfest's theorem that Lorentz' electron theory and the equipartition principle excludes induced magnetism (40). See ($AM_{B1,}$ 58-63, fn. 5 183) for further discussion.

ignoring, forgetting, or using patchwork ad hoc hypotheses to cover these difficulties.[136] On this, Feyerabend writes:

> The material which a scientist *actually* has at his disposal, his laws, his experimental results, his mathematical techniques, his epistemological prejudices, his attitude towards the absurd consequences of the theories which he accepts, is indeterminate in many ways, it is ambiguous, and *never fully separated from the historical background*. This material is always contaminated by principles which one does not know (43).

More specifically, "observational languages may become tied to older layers of speculation" (ibid). This is the 'historico-physiologic character of evidence' (44). This means that taking observational 'facts' for granted implies that a theory is being rejected simply because it conflicts with older theories that have 'contaminated' the observational language of the day. Notice that Feyerabend gives no defense of counterinduction by a formal analysis of theory testing. Indeed, it's difficult to imagine what such a defense would look like. Rather, counterinduction is necessary for *discovering* theoretical assumptions that we are not previously aware of. Since all evidence requires theories, and we can never be sure that we have all the relevant theoretical assumptions at hand, counterinduction is "always reasonable and it has always a chance of success" (AM_{B1}, 32).

Counterinduction, according to Feyerabend, allows us to make these contaminations explicit so they can be tested. Specifically, counterinduction reveals 'natural interpretations' defined as "mental operations which follows so closely upon the senses" (48) that act as assumptions about which sensations are veridical. Over time, these assumptions become *unconscious psychological mechanisms* whereby we obtain "(1) a clear and unambiguous *sensation* and (2) a clear and unambiguous *connection* between this sensation and parts of a language" (49). These mechanisms are jointly necessary for any fact. We *can* distinguish between appearance and the statement and psychologically separate the two mechanisms. These features only come together under 'normal circumstances' where "describing a familiar situation is, for the speaker, an event which statement and phenomenon are firmly glued together" (ibid).

[136] Feyerabend gives the example of Newton's ad hoc hypothesis that the reflection of a ray is effected by some power of the body which is evenly diffused over its surface and Barrow's acknowledgement of the problem but the statement that "neither this nor any other difficulty shall have so great an influence on me, as to make me renounce that which I know to be manifestly agreeable to reason" (39). Feyerabend then claims that "[t]his is not the usual procedure. The usual procedure is to forget about the difficulties, never to talk about them, and to proceed as if the theory were without fault" (ibid). This argument is repeated in chapter 5 of AM_{B1}.

Feyerabend argues that natural interpretations are *necessary for* observational knowledge in the first place.[137] "Eliminate all natural interpretations, and you eliminate the ability to think and to perceive" (50). Why? Because "it should be clear that a person who faces a perceptual field without a single natural interpretation at his disposal would be *completely disoriented*" (51).[138] To be clear, natural interpretations do not change the phenomenal character of experience; Feyerabend explicitly omits this assumption from his analysis (47).[139] Rather, it is the view that observational 'facts' (i.e., observation *statements*) require natural interpretations to be considered true.

Feyerabend argues that we need a measure external to the "natural discourse which…constitutes a form of life" (ibid) as a 'detecting device' for natural interpretations. Here, Feyerabend assumes that a conscious comparison between hidden natural interpretations and newly formulated natural interpretations is necessary for revealing the content of the hidden elements. Since the content of both precepts and concepts are dependent on background knowledge, which are "difficult to nail down", we will always be using those background assumptions when determining the content of observation statements. More generally put:

> Perceptions must be identified, and the identifying mechanism will contain some of the very same elements which govern the use of the concept to be investigated. We never penetrate this concept completely, for we always use part of it in the attempt to find its constituents. There is only one way to get out of this circle, and it consists in using an *external measure of comparison*, including new ways of relating concepts and precepts (ibid).

[137] Feyerabend contrasts Galileo, who wants to critically discuss natural interpretations, with Kant who wants to retain them and Bacon who wants to eliminate them (AM_{B1}, 73).

[138] It is difficult to assess how contemporary empirical research confirms or disconfirms this claim. On one hand, eye movement analyses suggest that infants under 3 months can track non-oscillating patterns of movement with simple shapes (Slater & Johnson 1997) suggesting that agents with limited theoretical beliefs can perceive some kinds of processes. On the other hand, language acquisition appears to be the result of different mechanisms of recognizing statistical trends in language use (Kuhl 2000) suggesting that verbal statements are often not learned ostensibly. This supports the natural interpretations thesis since making statements in response to observational scenarios is a matter of *habit*, which contain assumptions that are rarely explicitly cognized. This is a massively complicated topic, however, so I will assume that Feyerabend is correct on this point for the time being.

[139] In AM_{B1}, Feyerabend writes that "Having been influenced by Wittgenstein, Hanson, and others, I was for some time inclined towards [the view that theories alter perception] but it now seems to me that it is ruled out both by physiology (psychology) and by historical evidence" (133).

Analyzing the content of observational concepts within a single form of life will be circular.[140] We *must replace* the natural interpretations under investigation with new ones and compare the results. This comparison cannot retreat to intuitions, 'common sense', etc. since they privilege the older natural interpretations. What comparative standard may we use? Feyerabend simply remarks that we can "actively apply various rules of thumb, and while we may in this way arrive at a satisfactory judgment, it is not at all wise to go further and to turn these rules of thumb into necessary conditions of science" (ibid). Now that we have the abstract characterization of this method, let's look at the case study of Galileo to see it in practice.[141]

The tower argument is one of the most pivotal arguments in Galileo's corpus. Originally, it appears as an "irrefutable argument for the earth being motionless" (45).[142] If the earth were in motion, we would expect a stone dropped from a tower to fall miles away from its base. Yet this contradicts our everyday observations. Galileo accepts this, and writes the following:

> I…have never seen nor ever expect to see the rock fall any way but perpendicularly, just so do I believe that it appears to the eyes of everyone else. It is therefore better to put aside the appearance, on which we all agree, and to *use the power of reason either to confirm its reality or to reveal its fallacy* (emphasis added, Galileo 1632, 256).

What does it mean to have fallacious observations? Galileo answers this in the next paragraph:

> One may learn how easily anyone may be deceived by simple appearances, or let us say by the impression of one's senses. This event is the appearance to those who travel along a street by night of being followed by the moon, with steps equal to theirs, when they see it go gliding along the eaves of the roofs (ibid).

Feyerabend immediately notes that this is not a distinction between reason and appearance, but a distinction between reason and "appearance plus statement. There are not two acts, one, noticing a phenomenon, the other, expressing it with the help of the appropriate statement, *but only one, viz. saying*, in a certain observational situation, "the moon is following me"" (AM_P 47).

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[140] The argument from circularity appears briefly in many of Feyerabend's earlier papers (Feyerabend 1963c, 172; 1965b, 151) suggesting that this motivation had been dormant since at least the early 60s. "[P]hilosophical arguments", Feyerabend writes, are "invariably circular….We must choose a point *outside* the system…to be able to get an idea of what a criticism would look like" (ibid). Cf. also Feyerabend (1976, fn. 15 212).

[141] Here, I am using the historical exegesis as found in AM_P. This is criticized by Machamer (1973) and responded to by Feyerabend (1974) and the exegesis is reformulated in AM_B. The major addition in AM_{B1} is Feyerabend's argument that Galileo did not provide sufficient theoretical justification (nor could he) for believing in the veridicality of the images produced by his telescope and Feyerabend's slight alterations to the arguments considered in this section are negligible and can, therefore, be ignored for the time being.

[142] The argument convinced Tycho Brahe and can be found in Galileo's earlier work *Trattato della Sfera*. el

Feyerabend conjectures that Galileo replaces one set of natural interpretations with another. However, to introduce the (then) absurd claim that the earth moves and get an 'attentive hearing' Galileo uses *propaganda* or 'psychological tricks' (55). Specifically, Galileo uses *anamnēsis* to show that relative motion is present in *other* situations (e.g., on boats) and, therefore, is a part of Aristotelian *common sense*. However, physical situations with few moving parts and stable surroundings, such as dropping a stone from a tower, only contain non-relative motion within Aristotelian common sense. Galileo makes it seem as if relative motion should be present in the tower experiment on Aristotelian grounds even though it isn't. Now, instead of the tower experiment *confirming* that the earth is at rest, it merely shows that there is no relative motion between the starting point of the drop and the earth. Now Galileo needs to show that our senses are only in tune with relative motion. Galileo does this with his 'principle of circular inertia' (Feyerabend's title): "An object that moves with a given angular velocity on a frictionless sphere around the center of the earth will continue moving with the same angular velocity forever" (ibid). He defends this principle by "showing how it helps Copernicus; this defense is truly ad hoc", reemploying anamnēsis, and "surreptitiously generalizing that function" (ibid). We now see how Galileo replaces the natural interpretation that all motion is operative in perception with the natural interpretation that only relative motion is operative in perception.

The introduction of the notion of 'counterinduction' is meant to complement and elaborate upon the principle of proliferation discussed in previous sections. However, we are left with a puzzle of this more general notion of a 'counter rule', which Feyerabend appears to justify by arguing that all methods are inherently limited, in some sense. While this argument is unclear, for the moment, it should become clearer as we move along.

1.4: Feyerabend's Mature View of Proliferation: AM and Beyond

In AM_B and subsequent works, Feyerabend's mature and unconstrained view of proliferation emerges. The thesis of chapter 4 of AM_B is that "[t]here is no idea, however ancient [or] absurd, that is not capable of improving our knowledge" (27) including 'non-scientific' cosmologies (the Bible, the Iliad, etc. (fn. 1 27)). More generally put, "[alternatives] may be taken from wherever one is able to find them – from ancient myths and modern prejudices; from the lubrications of experts and from the fantasies of cranks" (ibid). Later, Feyerabend extends proliferations to "ideas, methods, forms of life" (Feyerabend 1978a, 148). The content of proliferation is unconstrained. What justifies this rapid expansion of the principle of proliferation? Feyerabend does not say. At some points, he appears to believe this as a conjecture: “There is not a single idea, however absurd and repulsive that has not a sensible aspect” (Feyerabend 1979, 63). But the argument given in the previous section gives us a more systematic hint: because all ideas are inherently limited. We must violate them before we know their limits and what value their opposites have. Feyerabend writes:

> Some of the most important formal properties of a theory are found by contrast, and not by analysis. A scientist who wishes to maximize the empirical content of the views he holds and who wants to understand them as clear as he possibly can must therefore introduce other views; that is, he must adopt a *pluralistic methodology* (AM_{B1}, 30).[143]

This contrastive notion should be familiar since it is inherent in Feyerabend’s arguments for maximal testability and criticism. What is made clearer, here, is that testability and criticism do not emerge from ideas with particular content but from the dynamics of the contrast itself. This will be clearer when we learn of Feyerabend’s adoption of Mill.

Similarly, proliferation can also include ‘dead’ ideas. Feyerabend criticizes Hesse and Skinner’s claims[144] that we shouldn’t revive Aristotelianism when even though it was revived in 17th and 18th-century electricity and “resurfaced in biology, in the thermodynamics of open systems and even in mathematics” (fn. 3 27).[145] He also cites the revival of Pythagorean arguments for the motion of the earth in Copernicus that Ptolemy dismissed as ‘entirely ridiculous.’ Considering this, Feyerabend writes “[s]uch developments are not surprising. No idea is ever examined in all its ramifications and no view is ever given all the chances it deserves. Theories are abandoned and superseded by more fashionable accounts long before they have had an opportunity to show their virtues” (29).[146] William Bartley put this point quite well in *Unfathomed Knowledge*:

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[143] It is unclear, in this chapter, what undergirds the assertion that contrast, rather than analysis, improves understanding. His uptake of Hegel (cf. Feyerabend 1981d (73-9)) may be one source of inspiration, where to say what something is requires, partially, to say what it isn’t, or his ruminations on anthropology, linguistics, and perceptual psychology where conventions become revealed when they are separated (cf. chapter 16 of AM_{B4}).

[144] See Hesse (1967, 93): “the question arises why we do not *go back* and exploit the objective criticism of modern science available in Aristotelianism, or indeed in Voodoo?” and Skinner (1971, 5): “No modern physicist would turn to Aristotle for help.”

[145] Feyerabend cites Heilbronn (1979) for the former claim and chapter 8 of *Farewell to Reason* for the latter.

[146] This point is also made in an earlier paper: abandoned theories “had only a limited time at their disposal and were never completely exploited” (Feyerabend 1972, 143).

> Ideas are not fully known to their inventors or to the communities that first sponsor them; they are autonomous and may turn out to have implications and unintended consequences contrary to the interests of their inventors or sponsoring communities. Ideas not only express the interests of the communities; they often contradict and sometimes transform the interests of the communities in which they originate (Bartley 1993, 74).

This point will make more sense once we have studied the principle of tenacity. Until then, it is enough to show that Feyerabend thinks that the content of proliferation cannot be constrained on the basis of its content.

1.5: Empirical Considerations for Proliferation[147]

As alluded to previously, proliferation has empirical dimensions. Feyerabend was wary of this since early on in his career. He began by making quasi-empirical, quasi-intuitive assertions that pluralism "will ... improve our understanding of each of its members by making it very clear what is denied by whichever theory happens to be accepted in the end" (Feyerabend 1962b, 73) and pluralism "encourage[s] the building of a great variety of measuring instruments. There will be no one way of interpreting the results, and the theoretician will be trained to switch quickly from one interpretation to another. Intuitive appeal will lose its paralyzing effect, transcendental deduction which, after all, presupposes uniformity of usage will be impossible" (75).

Later in his career, Feyerabend assesses proliferation in a more empirically grounded manner. First, there is the question about what features of our perceptual world are invariant. Feyerabend asserts that "[t]he answer to this ... question must of course remain hypothetical" (Feyerabend 1965a, 130). For affirmative evidence, Feyerabend cites the fact what "until now only two or three per cent of the inbuilt circuits of the brain have been utilised. A large variety of [change] is therefore possible" (ibid). He also cites the success of Brecht's 'V-effect' (the 'distancing effect') as a way of changing people's worldviews[148] and Nietzsche's philological findings about changes in perception from classical to Hellenistic Greece (130-1). On the other hand, Feyerabend cites Young's study (130) about the ill effects of pluralistic education on

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[147] See Preston (2005) for an empirically updated assessment of the neurological and psychological underpinnings of Feyerabend's arguments for proliferation.

[148] Feyerabend has numerous papers where he defends the use of the arts as a means of maximizing criticism. Some of these means require proliferation, whereas others are mere instances of proliferating how scientists should understand theories (in a psychological sense) (see Feyerabend 1967a; 1967b) and Couvalis (1987) for discussion.

learning and Waddington and Gautt's studies in evolutionary psychology that corroborate this view (131). This suggests that Feyerabend recognizes the empirical limits on proliferation. Later, Feyerabend becomes more confident in proliferation. In an extensive footnote in AM_P (fn. 42 107), he cites psychoanalysts (Freud, Róheim, Huxley), anthropologists (Mead), and sociologists (Merton) that support various different dimensions of proliferation.[149] Whether or not these studies really support proliferation is not my concern here. What is important, for our current purposes, is that there are empirical criteria by which we can judge proliferation:

> One should remember that the debate is about methodological rules only and that 'freedom' now means freedom *vis-à-vis* such rules. The scientist is still restricted by the properties of his instruments, the amount of money available, the intelligence of his assistants, the attitude of his colleagues, his playmates – he or she is restricted by innumerable physical, physiological, sociological, historical constraints. The…epistemological anarchism I advocate…removes only the methodological constraints (AM_{B1}, fn. 15 187).

Whether proliferation actually leads to the desired methodological effects is partially dependent on the content of the proliferated idea itself. We need ideas that are inconsistent with previous ideas, and partially dependent on empirical criteria. Whether or not proliferation will maximize *criticism* is an empirical question.

Once again, however, Feyerabend has left us with a puzzle. The 'critical' justification of proliferation is contingent on empirical considerations whereas the methodological justification, maximizing testability, is true by virtue of the logic of theory testing. Feyerabend assumes, but never justifies, that these two justifications will *always coincide*. This assumption may be oftentimes true, but what about instances in which proliferation minimizes our critical powers while enhancing testability? I think that this question provides some clarification as to the relationship between these two principles. Clearly, it must be empirically possible for the principle of maximal testability to be carried out making it subject to empirical evaluation. However, *criticism* can be left out of a reconstruction whereas testability cannot. It is this reason that leads Feyerabend to believe that pluralism is not merely contingently valuable, but *logically*

[149] Feyerabend cites the relationship between multiple personality disorder and 'intraindividual proliferation' where the ambiguity of the ego ideal allows for its parts to compete with each other which "contributes to the development of both [parts of the ego] and creates the dynamics of the individual" (AM_P, fn. 42 107) in Freud's *Das Iche und das Es*,. However, 'vengeful' competition can lead to 'disastrous consequences' and so both intra and inter individual proliferation requires a "system of institutionalized vigilance, involving competitive cooperation" (Merton 1969, 220).

necessary. But tests are merely a special kind of criticism, meaning that the principle of maximal criticism is broader. Therefore, we can contingently have conflicts between maximizing tests and maximizing other kinds of criticism, but the principles themselves remain consistent.

1.6: Mill, Proliferation, and Humanitarianism

The majority of this chapter has focused on methodology. However, ethics always played an important role in Feyerabend's epistemology. However, Feyerabend never detailed any ethical stance nor was it even is it clear that he would. As Tibbetts writes,

> [Feyerabend] understandably shies away from a detailed account of what constitutes the good life and what social/political structure would most guarantee and promote human happiness. Feyerabend is too much of a libertarian to venture into the dark waters of creeds and manifestos binding on other men (Tibbetts 1976, 368).

However, surprisingly, this is not completely true. In this section, I canvass Feyerabend's remarks on ethics and the closest thing he finds to a moral framework: Mill's pluralism.

Early in his career, Feyerabend argues that ethics is *foundational* to methodology:

> Which attitude shall we adopt and which kind of life shall we lead?... is the most fundamental problem of all epistemology…we are confronted with a real *decision*, that is, a real choice with a situation which has to be resolved on the basis of our demands and preferences, and which cannot be resolved by proof. It is easy to see that these demands these preferences concern the welfare of human beings are therefore ethical demands: epistemology, or the structure of knowledge we accept, is grounded upon an ethical decision (Feyerabend 1961b, 55-6).[150]

He repeats this sentiment, and even extends it to the whole of philosophy four years later:

> The fact that almost any philosophical doctrine may find realization either in a *cosmology*…and/or in a *theory of man*…makes it very clear that the procedure leading to the adoption of a philosophical position cannot be *proof*…but must be a *decision* on the basis of preferences…Philosophers have habitually judged the situation in a very different manner. For them, only *one* of the many existing position was true and, therefore, possible. This attitude, of course, considerably restricts the domain of responsible choice…The problem of responsible choice enters even the most abstract

[150] This quote appears in the original transcript of Feyerabend's lectures. In the 1981 collection, the following quote appears in its place: "It is easy to see that [methodological] demands and ... preferences have quite a lot to do with the welfare of human beings and are therefore ethical demands: epistemology, or the structure of the knowledge we accept, is grounded upon an ethical decision…it is usually assumed that the foundations of our knowledge are things which exist independently of human beings…This is quite correct *provided* we have already accepted a dogmatic point of view that works with certainties" (Feyerabend 1961b, 71).

> philosophical matters and…ethics is, therefore, the basis of everything else (Feyerabend 1965b, fn. 5 219).

This raises the question: does Feyerabend believe in moral principles that guide what lives we should live? While one would expect a resounding ‘No!’, there is some textual evidence that Feyerabend at least flirted with this belief. In a letter to Lakatos, Feyerabend writes “[t]he only theoretical restriction…of science which I am prepared to tolerate is what follows from a principle of general hedonism: all those elements of science which are inconsistent with hedonism must go” (quoted in Motterlini 1999, 121) where “happiness and the full development of an individual human being are now as ever the highest possible value” (Feyerabend 1970d, 21). He repeatedly discusses the value of happiness, in an undefined sense, throughout his corpus (cf. Feyerabend 1968b, 134). However, it would be naïve to suggest Feyerabend is a run-of-the-mill hedonist since Feyerabend entertains pluralism in the moral sphere as well. He writes that “[p]rogress has always been achieved by probing well-entrenched and well-founded forms of life with unpopular and unfounded values. This is how man gradually freed himself from fear and from the tyranny of unexamined system” (209-10). Similarly, Feyerabend frequently discusses the value of liberation[151] suggesting that happiness is an *achievement*. “Any ideology that breaks the hold a comprehensive system of thought has on the minds of men contributes to the liberation of man” (Feyerabend 1975b, 181). This is compatible with Feyerabend’s numerous remarks endorsing the development of ‘pluralistic’ minds that entertain many forms of life to achieve ‘higher pleasures’, and are necessary for the “production of well developed human beings” (Feyerabend 1981d, 65). He also writes that anarchists should “invent... compelling reasons for unreasonable doctrine…There is no view, however ‘absurd’ *or ‘immoral’,* he refuses to consider or act upon” (emphasis added, AM_{B1}, 189).[152] These seemingly scattered claims come together in his endorsement of Mill.

Similar to his rapid uptake of Popper’s philosophy, Feyerabend appears largely uncritical of Mill’s moral philosophy. His adoption of it comes quickly, and without qualification.

[151] Liberation is valuable for the sake of happiness. Feyerabend writes, “Why would anyone want to liberate anyone else? Surely not because of some *abstract* advantage of liberty but because liberty is the best way to free development and *thus to happiness*. We want to liberate people *so that they can smile*” (Feyerabend 1975b, 191). He also writes that “the most important question of all [is] the question of what extend the happiness of individual human beings, and to what extent their freedom, has been increased” (Feyerabend 1970d, 209).

[152] Feyerabend also writes “it should be possible, in a free society...to make propaganda for any subject, however absurd and however immoral” (Feyerabend 1970a, 127).

Feyerabend asserts that the “idea that a pluralistic methodology is necessary *both for* the advancement of knowledge *and* for the development of our individuality has been discussed by J.S. Mill” (Feyerabend 1981d, emphasis added 65). And, again, “For Mill the (material and spiritual) welfare of the individual, the full development of his capabilities, is the primary aim. The fact that the methods used for achieving this aim also yield a scientific philosophy, a book of rules concerning the ‘search for the truth’, is a side effect, though a pleasant one” (fn. 6 68) and “methodological and humanitarian arguments are intermixed in every part of Mill’s essay, and it is on *both* grounds that a pluralistic epistemology is defended” (70-1).[153] Why does proliferation have such a moral function? Feyerabend writes, quoting Mill along the way, that:

> Choice presuppose[s] alternatives to choose from; it presupposes a society which contains and encourages ‘different opinions’ (249), a ‘negative’ logic (236f), ‘antagonistic modes of thought,’[154] as well as ‘different experiments of living’ (249), so that the ‘worth of different modes of life is proved not just in the imagination, but practically’ (250). [U]nity of opinion,’ however, ‘unless resulting from the fullest and freest comparison of opposite opinions, is not desirable, and diversity not an evil, but a good’ (249) (66).

Proliferation, therefore, promotes free choice. Contrary to other readings of Mill, Feyerabend insists that free choice is not valued for the sake of happiness, “maximum happiness plays no role in *On Liberty*” (fn. 10 70), but for “the free and unrestricted development of the individual” (ibid). This development includes the development of *morals*; so Mill and, thereby, Feyerabend think that morality *itself* evolves according to this pluralistic schema. Without wading too deep into Millian waters, let’s note some interesting features of this defense of proliferation.

For Mill, we learn about the good life through personal experimentation.[155] However, a great deal of harm can come from such experimentation. The same is true of many *experiments*; experimentation on humans and non-human animals, sending experimenters to space or deep

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[153] Feyerabend makes this claim himself, without reference to Mill as well (AM_P, 26-7; AM_B, 12).
[154] The following quote is inserted in a footnote at this point: “I had to learn that I would recognize the value of health even in sickness, the value of rest through exertion, the spiritual through deprivation of material things…through evil the value of good” (Cohen 1961, 62). It should be clear from the endorsement of this quote (and other similar quotes) that Feyerabend is not a simple-minded utilitarian who seeks to maximize happiness since understanding something requires experiencing its opposite.
[155] See Anderson (1991) for a classic exposition of Mill’s ‘experiments in living.’ Anderson’s interpretation of Mill is somewhat different than Feyerabend’s in that she thinks that Mill’s principle of proliferation aims to test psychological theories of what is necessary for happiness. As such, ‘happiness’ still plays a crucial role in *On Liberty* but the content of both the mental states associated with happiness may be reformed in light of new experiments of living.

mine shafts, and so forth. We cannot even resort to a utilitarian calculus, even in principle, since the appraisal of the products of such activities depend on those activities having transpired. Feyerabend gives us no concrete advice here. My conjecture is that we can point to a disanalogy between moral decision-making and scientific decision-making, in a Feyerabendian framework. Some decisions cause harms that cannot be undone or 'outgrown.' As such, they have *permanent* drawbacks.[156] Thus, some instances of proliferation may counteract the overall goal of proliferation to achieve a "real understanding of moral and human subjects" (69). I cannot speak much more to this topic, as it would require a new dissertation to address. For now, let us rest content with the fact that pluralism extends into the moral sphere, for Feyerabend, as well.

1.7: Concluding Remarks

After following the long and winding road of the principle of proliferation, we arrive at an exceptionally general principle. We have seen four independent justifications for proliferation:

(1) It maximizes testability.
(2) It maximizes criticism.
(3) It maximizes our ability to pursue theories.
(4) It facilitates our moral growth as 'well developed individuals'

We also have a set of constrained and unconstrained features of proliferation:

Constraints	**Unconstrained**
Empirical (broadly construed)	The content of proliferation
Ethical (pursuing ideas that cause irreversible harms)	The method of proliferating
	The (epistemic) situations in which we may proliferate.

Table 1: Constraints on Proliferation

Feyerabend recognizes that these justifications are distinct, but assumes that they will always coincide. While there may be many cases in which this is true, Feyerabend has given us no reason to think this is necessarily the case. Indeed, it would be surprising for a thinker like Feyerabend would think that a general principle would be so all encompassing. One way of managing possible conflicts would be hierarchical, which seems possible given that morality is

[156] Perhaps Feyerabend could use Nietzsche's off-quoted dictum: What doesn't kill me makes me stronger.

fundamental to methodology. However, given that our moral growth is spurred on by proliferation, including proliferation of theories, there is no 'foundation' to Feyerabend's principle of proliferation. We only have four reasons to proliferate, and we have no principled way to choose which gains prominence in a given case.

2. The Evolution of Tenacity

The principle of proliferation, on its own, is bankrupt. For proliferation to be of any use, the ideas proliferated must be developed. While this seems obvious,[157] the mature version of Feyerabend's principle of tenacity is exceptionally radical. In this section, I reconstruct Feyerabend's evolving views on this topic and compare it to the views of Kuhn and Lakatos.

2.1: The First Formulation of Tenacity

Feyerabend was aware of the principle of tenacity extremely early on in his career:

> Is it not well known that refuting instances can with some ingenuity always be turned into confirming instances and that there exist elaborate theories which perform this transformation nearly automatically?... From all this we have to conclude that *nature can never force us to admit that we have been mistaken* (Feyerabend 1960a, 233).
>
> Strong beliefs are admitted and even encouraged; enthusiasm for a particular theory is most welcome and so is tenacity in the face of difficulties (after all, theory *might* be capable of solving them) (Feyerabend 1961b, 70).

However, the first explicit statement of the principle of tenacity comes in 1968:

> It would be imprudent to give up a theory that either is inconsistent with observational results or suffers from internal difficulties. Theories can be developed and improved, and their relation to observation is also capable of modification…Moreover, it would be a complete surprise if it turned out that all the available experimental results support a certain theory, even if the theory were true. Different observers using different experimental equipment and different methods of interpretation introduce idiosyncrasies and errors of their own, and it takes a long time until all these differences are brought to a common denominator. Considerations like these make us accept a *principle of tenacity*, which suggests, first, that we select from a number of theories the one that has the most attractive features and that promises to lead to the most fruitful results; and, second, that we stick to this theory despite considerable difficulties (Feyerabend 1968a, 107).[158]

[157] "[A] triviality that seems to have been forgotten" (Feyerabend 1963a, 80).

[158] Feyerabend never specifies whether tenacity is meant to apply to *individuals* or *communities*. I think we should charitably construe tenacity as being relevant to the latter for two main reasons. First, Feyerabend's ethics require that individuals are able to think pluralistically making tenacity in tension with Feyerabend's ethics. Second, even

Here, tenacity applies to testing purported falsifications, settling debates about interpretation, use of instruments, etc. making it general in its scope. Additionally, tenacity requires *modifying* theories in light of the problems it faces. Tenacity is therefore not equivalent to holding a theory come what may, but retaining a theory by suitably modifying it in light of criticisms. But this point is tricky. As Kuhn has pointed out, scientists often ignore problems that their theories accidentally solve later on. Feyerabend accepts this point, though with a different justification:

> This need to *wait*, and to *ignore* large masses of critical observations and measurements, is hardly ever discussed in our methodologies. Disregarding the possibility that a new physics or a new astronomy might have to be judged by a new theory of knowledge and might require entirely new tests, empirically inclined scientists at once confront it with the status quo and announce triumphantly that 'it is not in agreement with facts and received principles.' They are of course right, and even trivially so, but not in the sense intended by them. For at an early stage of development the contradiction only indicates that the old and the new are *different* and *out of phase*. It does not show which view is the *better* one. A judgement of this kind presupposes that the competitors confront each other on equal terms (AM_{B3}, 113).

In other words, we can re-implement tenacity at a 'micro level' with any perceived 'problem' with a theory. This leaves us in a tricky position since, if this option is always available, we can constantly avoid developing a theory in light of its problems which conflicts with the justification of tenacity in the first place. Feyerabend provides us with no firm resolution of this problem, so I will manage this issue in the next chapter. Finally, the final clause, which provides the conditions for tenacious theory pursuit, is remarkably curious; what does it mean for a theory to show promise? Feyerabend even problematizes the prospects for providing an answer in an earlier paper:

> Does such an admission [that we should tenaciously develop theories with low factual support] open the door to all sorts of wild speculations such as the hollow earth theory, Wilhelm Reich's Orgonomy, Dianectics, astrology, and other crazy ideas? Is this not making illegitimate use of the success of Copernicus (and, so one might add, of Boltzmann and Schrödinger?)…I agree: Copernicus has been very successful, astrology has not been quite so successful. But what I am talking about now is the attitude to be adopted *before* a theory has proved its fruitfulness. The objection assumes that the final success of Copernican *could somehow have been foreseen* and that we know *in advance* that Orgonomy is completely fanciful…But how could we possibly possess such knowledge? Because the existence of Orgon is *inconsistent with contemporary physics*?

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though Feyerabend's historiography is often quite individualistic, contemporary research seldom advances in this manner making the tenacity of individuals a somewhat antiquated notion. Farrell (2003, 212) briefly touches on this point and agrees with this interpretation.

> Copernicus was inconsistent with the physics of his time… Or shall we reject the Orgon idea…because it is *absurd*? Copernicus was regarded as absurd….From all of this it seem to emerge from the point of view of their status *before* their success (or failure) there is not much to choose between the hypothesis of the moving earth…on the one hand, and the hollow earth theory…on the other (Feyerabend 1964a, 197).[159]

Feyerabend never answers this question. However, Feyerabend makes an interesting note in an earlier paper. He writes:

> The… clash of meanings, initial absurdity *are desirable*; presence of…intuitive plausibility, agreement with customary modes of speech, far from being philosophical virtues, indicate that not much progress has or will be made. Such features are a sign that we are still moving safely within the boundaries of knowledge set by our ancestorism, and that we have not even started examining whether the boundaries are correctly drawn or what goes on outside them (Feyerabend 1965b, 185).[160]

It seems like we have a criteria for theory pursuit: theories that *conflicts with* previous theories since they will likely reveal lacunae in previous ways of thinking. We saw this in Feyerabend's mature arguments for proliferation where conflicting theories provide the deepest criticisms possible. This does not mean that contradiction with previous theories is a *necessary* condition for theory pursuit, but may be a *ceteris paribus* criterion for pursuitworthiness.[161]

Additionally, Feyerabend discusses the conditions of the termination of tenacity. He writes, "[o]ne might now be inclined to specify a limit of disagreement [between experiment and theory] beyond which one is not prepared to do. But it is not easy to see how such a limit can be fixed in a non-arbitrary fashion" (Feyerabend 1968a, 108). This is because, as Feyerabend has already recognized, the status of evidence changes over time; some recalcitrant facts becoming confirming instances, some minor disagreements become major and vice versa, and some disagreements become *dreckeffekt*. However, Feyerabend claims, "it *is* rational to withdraw T if there exists another theory T′ that accentuates the difficulties of T…while at the same time promising means for their removal and opening up new avenues of research. In this case the

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[159] Interestingly enough, Orgonomy has been making a comeback within the past 15 years and has seen applications in psychiatry and medicine (see Klee 2005).
[160] This insight is reiterated in Feyerabend (1965b, fn. 150 254).
[161] In another paper, Feyerabend argues that these conflicts are most conducive to criticism when it is between two *developed* theories or else we are merely "arranging a boxing match between a toddler and an adult athlete, together with a triumphant announcement that the adult will surely win" (Feyerabend 1972, 172). See chapter 12 of AM_{B1} for more discussion.

principle of tenacity itself urges us to remove T" (ibid).[162] This is the same view as the one expressed in Feyerabend (1965a); we stop pursuing a theory once a strong alternative is able to accomplish the same feats as its predecessors plus more. Therefore, we pursue T regardless of its problems *alongside* T′ until T′ meets the aforementioned criteria, *then* we pursue T and T′' simultaneously, and so on.

To summarize, I have reconstructed three key features of tenacity: the conditions of pursuitworthiness, the conditions of termination, and the process of developing a theory. Feyerabend provides no (necessary) criteria for pursuitworthiness. He provides straightforward criteria for theory termination: when a new theory has supplanted an older theory. Finally, he has implied that there is some kind of regulative ideal during the process of pursuit. Since the principle of tenacity is general in its scope, it applies not just to theories but to any 'idea', broadly conceived. I will now go on to clarify this principle via Feyerabend's debates with Kuhn and Lakatos.

2.2: Feyerabend versus Kuhn on Tenacity

Feyerabend devotes many papers to Kuhn where he discusses, amongst other things, the principle of tenacity (Feyerabend 1963d; 1964a; 1970d). Before detailing this, note that Feyerabend complains that Kuhn is presenting a methodological argument under the guises of pure history. Kuhn is thus 'dishonest' about whether he means to be descriptive or normative (though Feyerabend thinks he is wrong on both counts anyways).[163] More importantly, for our present purposes, is how Kuhn and Feyerabend diverge on tenacity.

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[162] Feyerabend adopts the same view in another paper. Feyerabend writes "[a]ssume that you have decided to articulate and elaborate a theory which conflicts with observed facts hoping to cope with them someday. When do you give this theory up? One possible way out of this difficulty is this: introduce alternative theories and try to show that certain parts of the conflicting evidence can be explained with their help" (Feyerabend 1968b, 131).

[163] Kuhn's response to Feyerabend is that "[I] should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish. The structure of my argument is simple and, I think, unexceptionable: scientists behave in the following ways; those modes of behavior have…the following essential functions; in the absence of an alternate mode *that would serve similar functions,* scientists should behave essential as they do if their concern is to improve scientific knowledge" (Kuhn 1970, 237). This response, however, neglects Feyerabend's argument that Kuhn's view of paradigms doesn't even serve the purpose Kuhn thinks it serves (see section 5 of Feyerabend 1970d and Bschir 2015) and misses the point since even if it were the case that science-as-practiced had some function, it is a *normative* question of whether that function is to be valued.

Paradigms become pursuitworthy with the resolution of a revolution. In *Structure*, Kuhn claims that while revolutions are solved largely by methods of persuasion exploiting "idiosyncrasies of autobiography and personality", "the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis" (Kuhn 1962/2012, 152). This is, partially, a *promissory note*; it is a statement about the new paradigms pursuitworthiness, a point Kuhn reaffirms later in his career (cf. Kuhn 1977, 322).[164] The validity of these promises, for Kuhn, depend on some established successes of the new paradigm (Kuhn 1962/2012, 153-5). The other 'irrational' features of revolutions, persuasion and conversions, are only effective because of these successes. This is why Kuhn is correct to reaffirm that revolutions are both *rational* and *do not uniquely determine theory pursuit*. The unequivocal pursuit of *one* paradigm requires a certain psychological disposition to have faith in the future successes of a new paradigm.

While paradigms narrow the scope of scientific research, they also have 'built in mechanisms' for producing anomalies (60). Sometimes anomalies are minor enough that they are ignored. However, sometimes they are major enough to cause a sense of 'uneasiness' within a scientific community. The "awareness of anomaly opens a period in which conceptual categories are adjusted until the initially anomalous has become the anticipated" (64). This is how Feyerabend understood tenacity in his letters to Kuhn: 'apparent' refutations only become refutations until they have been sufficiently scrutinized.[165] If this scrutiny does not refute the refutation, we have a genuine anomaly. These anomalies are deep and persistent and can last for

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[164] Kuhn is clearer on this point in the 3rd edition of *Structure*. He writes:

> if a new candidate for paradigm had to be judged from the start by hard-headed people who examined only relative problem-solving ability, the sciences would experience very few major revolutions . . . But paradigm debates are not really about relative problem- solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be *made on faith* (Kuhn 1962/1996, emphasis added 157-8).

Here, it is clearer that it is *partial success* that determines pursuitworthiness when combined with a certain *psychological disposition*.

[165] Kuhn gives a few historical examples to suggest that 'aware anomalies' persist for long periods of time before 'revolutionary' work is done to them in support of newer paradigms (Kuhn 1962/2012, 67).

centuries. More generally, in the cases that Kuhn analyzed (see 66-68), *pace* Newton,[166] "the awareness of anomaly had lasted so long and penetrated so deep that one can appropriately describe the fields affected by it as in a state of growing crisis" (68).[167] As a result, theories become proliferated, a "usual symptom of crisis" (71), to explain the anomaly and, finally, once there is enough faith lost in the paradigm, it crumbles into a state of revolution.

Despite the ambiguity of Kuhn's appraisal of this situation, we can decipher his version of tenacity. First, we have conditions for *pursuitworthiness*. Paradigms must have at least *some* successes and those successes must be significant enough to persuade scientists to have faith in its future performance. Second, we have conditions for the termination of a paradigm: when anomalies become too significant and can be explained by a competitor that emerges in revolutionary science. Unlike Feyerabend's view, proliferation is the *result* of a crisis rather than the *cause* of it. Kuhn writes that because "[paradigm] exploration will ultimately isolate severe trouble spots, [practitioners of normal science] can be confident that the pursuit of normal science will inform them when and where they can most usefully become Popperian critics" (Kuhn 1970, 247). In other words, the timing of proliferation is determined by the state of pursuit of a paradigm. On Kuhn's view, the historical pattern is:

Tenacity for T → Proliferation of Alternatives [Revolution] → Tenacity for T′

Whereas for Feyerabend, in 1968, the picture looks like this:

Tenacity for T *and* Proliferation → Tenacity for T′ *and* Proliferation

Additionally, Kuhnian tenacity is justified insofar as a paradigm continues to make *other discoveries*. A stagnant paradigm *plus* crisis results in a revolution whereas a 'progressive' paradigm *plus* crisis can be retained.[168] Finally, tenacity becomes unjustified *after* T has been replaced.[169] This is Kuhn's view on tenacity.[170]

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[166] The Newton example, Kuhn is referring to the "anomalies in the relation of diffraction and polarization effects" of Newton's optics (66; cf. Kuhn 1958). Since these anomies appeared during pre-paradigmatic science, it is not quite analogous to the other examples.
[167] 'External' (i.e., social, political, economic) factors can influence the timing of the crisis (see Kuhn 1957, 135-43).
[168] Normally, for Kuhn, anomalies on their own do not lead to revolutions though they're the *main* contributors. For instance, the increasing complexity of epicycles in Ptolemaic astronomy partly contributed to the Copernican revolution (Kuhn 1962/2012, 69), the "increasing vagueness and decreasing utility of the phlogiston theory" (71) and the weight-gain paradox contributed to Lavoisier's revolution, and paradoxes in absolute space in Maxwell (73-

Given that Feyerabend appreciates tenacity because of Kuhn ($AM_{B2,}$ 283), Feyerabend owes a debt of gratitude to him. That being said, however, their views importantly diverge in that Kuhn's view of normal science explicitly eschews proliferation whereas, for Feyerabend, it is a necessary condition of objective knowledge. More specifically, Feyerabend writes:

> This is really very often the case that an accumulation of "puzzles", and under some conditions a single "puzzle", *assumes the character of a fundamental problem only when seen from a new viewpoint*…Whether a problem counts as fundamental or not depends, therefore, on the existence of alternatives to the existing viewpoint: *it depends on the fact that we think in a pluralistic, and not in a monistic, way* (Feyerabend 1963d, 288-9).

I have highlighted this point before. This argument is effective given Kuhn's assumption that it is the psychological hold paradigms have on scientists that change 'testing' into 'puzzle solving.'[171]Additionally, Feyerabend is *morally* opposed to Kuhn's defense of dogmatism in normal science. For Feyerabend, tenacity is done for advancing theory pursuit; not inhibiting freethinking. As Popper writes, "we are prisoners [of a paradigm] in a Pickwickian sense: if we try, we can break out of our framework at any time" (Popper 1970, 56). Finally, remember his case study of Galileo; in that instance, we had to move *backwards* (i.e., decrease in empirical content) before moving *forwards* (AM_{B1}, 99-100). There were no, initially, *independent* reasons for accepting Galilean dynamics: "Independent evidence is as yet entirely lacking, but this is no drawback as it is to be expected that independent support will take a long time appearing" (99).[172] These are the three major fault lines between Feyerabend's early view of tenacity and Kuhn's.

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5) aided the Einsteinian revolution. These conditions become more explored in Kuhn's 1977 paper where weighted considerations of simplicity, fruitfulness, accuracy, consistency, and explanatory scope dominate theory choice.

[169] See chapter 12 of Kuhn (1962) for a discussion of those scientists who 'cling' to the losing side of a revolution.

[170] In an earlier paper, Kuhn argues that the conflict between pluralism ('divergent thinking') and monism ('convergent thinking') constitutes the 'essential tension' of all scientific inquiry. He also appears to make the more moderate claim that he is often interpreted as holding: "I shall therefore suggest below that something like "convergent thinking" is *just as essential* to scientific advance as is divergent…the ability to support a tension that can occasionally become almost unbearable us one of the prime prerequisites for the very best sort of scientific research" (Kuhn 1959, emphasis added 226). Here, he makes it clear, that he is merely *emphasizing* the importance of convergent thinking that has been neglected by others (namely, Selye and Getzels and Jackson). This is distinct from the function that is allotted to pluralism in *Structure*. I am unsure as to whether this represents an abrupt shift in Kuhn's views or an artifact of the context *Structure* was written in.

[171] See section 2 of Watkins (1970) for a discussion of this distinction.

[172] "We see again that Galileo's view of the origin of Copernicanism differs markedly from the more familiar historical accounts. He neither points to *new facts* which offer inductive *support* to the idea of the moving earth, nor does he mention any observations that would *refute* the geocentric point of view but be accounted for by Copernicanism" (AM_{B1}, 102). While Feyerabend does concede that there are other arguments Galileo provides for

2.3: Feyerabend versus Lakatos on Tenacity

Just before Lakatos died, he and Feyerabend were working on a dialogue about whether normative methodology could provide universal criteria for theory appraisal. Lakatos would argue that criteria were, in principle, possible and Feyerabend would argue that they are not. In their correspondence, the crux of their disagreement surrounded their answers to the following question: Can we give rational criteria for abandoning theory pursuit? Feyerabend's criticisms of Lakatos in section 8 of his 1970d, continue in his 1972 (160-5), AM_P (77-81), AM_{B1} (chapter 16), 1975c, and his 1976. Since Lakatos reformulates his position several times, I will only provide Lakatos' mature views on tenacity and Feyerabend's criticisms thereof. This dialogue results in Feyerabend's mature understanding of tenacity.

To understand Lakatos' view on tenacity, we must first understand his views on the role of history in the philosophy of science and his methodology of research programmes. The history and philosophy of science, for Lakatos, have a symbiotic relationship; "[p]hilosophy of science without history of science is empty; history of science without philosophy of science is blind" (Lakatos 1971, 91).[173] Philosophy of science provides rules "governing the *acceptance* and *rejection* of theories or research programs" (Lakatos 1970a, 92), by which we can reconstruct an 'internal history' of the rational growth of knowledge which must be supplemented by an 'external' socio-psychological history. History, normatively interpreted, arbitrates between methodologies. Methodology thus provides 'postdictions' about the history of science. Inductivists search for facts and generalizations thereof (e.g., Kepler's generalizations of Brahe's observations, Newton's generalization of Kepler's observations of planetary motion, and Ampère's law of electrodynamics from his observations of electric currents), conventionalists look for increases in simplicity (e.g., Copernican, Einsteinian, and Lavoisierian revolutions), and naïve falsificationists search for crucial experiments and the construction of more general theories (e.g., highly falsifiable theories include Newton and Maxwell's theories, the radiation formula of Rayleigh, Jeans and Wien, e.g., of crucial experiments include Michelson-Morley, Eddington's eclipse, and Lummer-Pringsheim).

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heliocentrism, which he argues are not sound (see appendix 2 of AM_{B1}), this is not nearly the same as Kuhn's view of 'partial support.'

[173] See Kvasz (2002) for a discussion of Lakatos' dialectics and Lakatos (1976a) for an exemplar.

Since Lakatos thinks the ‘justificationist’ attempt to ground normative methodology (i.e., a theory of rationality is justified by its truth content or degree of probability) has “crumbled under the weight of epistemological and logical criticism” (108) (cf. Lakatos 1968a), he thinks choosing between methodologies is a convention. Conventions are “criticized by criticizing the rational historical reconstruction to which they lead” (109). Specifically, a methodology is to be abandoned if it conflicts with the basic value judgments of the ‘scientific élite.’ This dispenses with the task of finding a universal set of rational rules since “[w]hile there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement in the last two centuries concerning *single* achievements” (Lakatos 1971, 111). This dialectic allows for progress in theories of rationality. It also allows for progress in historiography insofar as it can predict novel historical facts:

> Thus progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational…I need not say that such historiographical research programme can or should explain *all* history of science as rational…because of this rational reconstructions remain for ever submerged in an ocean of anomalies. These anomalies will eventually have to be explained either by some better rational reconstructions or by some ‘external’ empirical theory (118).

We now see how history can evaluate theories of rationality for Lakatos.

Lakatos formulates his ‘methodology of research programs’ (MRP) to reconcile falsificationism with tenacity. He distinguishes his view, ‘sophisticated falsificationism’, from Popper’s in three primary ways:

(1) The rules of acceptance (pursuit) for naïve falsificationism entail accepting any theory that can be interpreted as falsifiable. For MRP, to be acceptable is to have corroborated excesses empirical content over its rival. (Lakatos 1970, 116).
(2) Naïve falsificationism entails eliminating a theory if a basic statement conflicts with a it. For sophisticated falsificationism, a theory is falsified when another theory has excess corroborated content and can explain the success of its predecessor (ibid).
(3) Naïve falsificationism renounces all ad hoc maneuvers. MRP distinguishes between three kinds and only condemns those that make no new predictions.

Here, Lakatos provides conditions for pursuitworthiness; research programs must have excess content and explain the success of previous theories within the domain (1). We also have rules of termination; we should eliminate theories that have been ‘degenerating’ (i.e., becoming more ad hoc) and superseded by their competitors (2 and 3). Finally, MRP is justified by reconstructing history successfully (see section 3(c) of Lakatos 1970). Let us now see Feyerabend's response.

Feyerabend's remarks on Lakatos refine his own views of tenacity. He provides three distinct kinds of criticism: (1) criticism of the *methodology* of rational reconstructions, (2) *historical* criticisms, and (3) criticism *of MRP*. For (1), since rational reconstructions differ from the actual historical situations, "nobody can say whether a reconstruction, *when inserted into the historical surroundings that have given birth to actual science,* will produce comparable results" (Feyerabend 1976, 204). In other words, without understanding the material conditions under which theories of rationality were able to be implemented, we cannot know whether they will be similarly successful when applied in different sets of material conditions. As such, there is no such thing as an internal/external distinction since both are jointly necessary for a theory of rationality. Feyerabend thinks this requires anthropological investigations into the conditions necessary for rationality to thrive. For (2), Feyerabend criticizes Lakatos' assumption that judgments of the scientific élite are more valid than those of witches, alchemists, or astrologists. Without arguments to support the view that (modern) science is superior to other forms of life, this analysis is inconsequential. If a religious zealot denounced the Darwinian or Copernican revolution, Lakatos has no basis to retort from and is thus left with relativism. To be clear, Feyerabend is not defending relativism here. Rather, Lakatos is doing precisely what he accuses others of doing:

> Whatever specific form they take, psychologism and sociologism both seem to me to open to the following fundamental objection. Everyone…is bound to use normative third-world criteria, whether explicit or hidden, in establishing criteria for a scientific community... If one must have *some* idea of what constitutes science before one knows which communities ought to count as scientific, then one must first decide what constitutes [science] (Lakatos 1978c, 114).

Lakatos has begged the question by assuming what counts as the scientific élite in the process of determining what counts as science. Finally, for (3), Lakatos assumes that scientists during particular historical episodes are unified. Feyerabend writes:

> 'Science' is split into numerous disciplines, each of which may adopt a different attitude towards a given theory, and single disciplines are further split into schools, heresies, and so forth. The basic judgments of an experimentalist will differ from those of a theoretician (cf. Rutherford, or Michelson, or Ehrenhaft or Einstein); a biologist will look at a theory differently from a cosmologist; the faithful Bohrian will regard modifications

> of the quantum theory with a different eye than will the faithful Einsteinian (Feyerabend 1976, 208).

This leads to Feyerabend's historical criticism: many of the basic value judgments of the scientific élite are based on ignorance. Feyerabend claims that most scientists who hailed Newtonian gravity were "unaware of its difficulties and some of who believed [sic] that it could be derived from Kepler's laws" (209). Or, take Rosenfeld's support of complementarity; despite many "quantitative and qualitative disagreements with the evidence and [it's] being quite clumsy in places" (ibid), he claims that complementarity should be supported because "all evidence points with merciless definiteness in the…direction… [that] all process involving…unknown interactions conform to the fundamental quantum law" (Rosenfeld 1957, 44). Finally, "[a]dd to this the fact that most scientists accept basic value judgments on trust, they do not examine them, they simply bow to the authority of their specialist colleagues, and one will see that *common scientific wisdom is not very common and it certainly is not very wise*" (Feyerabend 1976, 209).

Finally, we can examine Feyerabend's criticisms of MRP. Feyerabend flips-flops on how he interprets Lakatos on what concrete decisions would be enforced by MRP.[174] At some points, Feyerabend calls Lakatos an "anarchist in disguise" (AM_P, 77)[175] because he thinks Lakatos has no answer. Either way, Feyerabend has two distinct arguments against the idea of an 'expiry date.' First, the same considerations that led to Lakatos giving theories 'breathing space' still apply when the theory is degenerating or overtaken by its rival since theories can make comebacks.[176] Therefore, "if not now, why not wait a bit longer?" (ibid; Feyerabend 1970d, 215). Secondly, discerning whether a theory will continue to degenerate (or progress) requires the *metaphysical* assumption that "[n]ature only rarely permits research programs to behave like caterpillars" (AM_{B1}, fn. 12 185).[177] Since we have no reasons for holding such a view, this cannot provide solid grounds for theory termination. Feyerabend thereby concludes that Lakatos has not justified his termination criteria. Without one, we may rationally pursue theories

[174] See Motterlini (1999, 3-4) for a dialectical reconstruction of the Feyerabend-Lakatos debate on this point.
[175] Musgrave (1975, 478) agrees with this interpretation. See Motterlini (1995) for a discussion.
[176] Feyerabend's favorite example of a comeback is Boltzmann's defense of atomism, in the face of the Zermelo-Poincaré recurrence objection and Loschmidt's reversibility objection, which was vindicated with Einstein's kinetic theory. Furthermore, while Feyerabend never, to my knowledge, makes this connection, comebacks can include theories that were abandoned at one point and resurfaced later on (see chapter 4 of AM and his 1970b (especially fn. 20, 66) for his defense of the revival of classical physics in the 1960s).
[177] "[T]he butterfly emerges when the caterpillar has reached its lowest stage of degeneration" (AM_{B1}, 185). A similar view is defended by Wigner (1995).

indefinitely insofar as they are complimented by alternatives. Finally, since a theory can make a comeback in *any way* (e.g., complex theories can become simpler, ad hoc theories can become more 'empirical', etc.), Feyerabend's earlier constraint that theories should be abandoned once new theories can explain more than their predecessors vanishes. The relative advantage of T′ over T may merely reflect a temporary stage of research. This also has implications for Feyerabend's account of pursuitworthiness. If any theory can make any comeback desirable, how can we provide markers for future successes? On this note, Feyerabend writes:

> there exists hardly any idea that is totally without merit and that might not also become the starting point of concentrated effort.... No invention is ever made in isolation, and no idea is, therefore, completely without abstract or empirical support. Now if partial support and partial plausibility suffice to start a new trend (and I have suggested that they do), if starting a new trend means taking a step back from the evidence, if any idea can become plausible and can receive partial support— then the step back is, as a matter of fact, a step forward (Feyerabend 1970f, 301).

The second part of the quote, which comes from Feyerabend's case study of Galileo, suggests that there are *no* markers for which ideas should be tenaciously developed. But there is a paradox lurking in the background here: even for a theory to become 'not totally without merit', it must be *pursued* first. This leads to a regress of *when* something becomes pursuitworthy. For any criteria for pursuitworthiness, *x*, we need an additional criteria for what can be pursued that may fulfill *x* and so on. While this paradox is implicit in Feyerabend's discussion, he never draws it out though it seems to support his arguments against providing criteria of pursuitworthiness. Since Feyerabend provides no resolution, this problem will be addressed in the following chapter.

To summarize, Feyerabend gives no necessary conditions for theory pursuit or theory abandonment. He provides conflict with previous theories as a sufficient condition for theory pursuit and sociological conditions for theory pursuit. Presumably, the sociological conditions extend to theory abandonment; we abandon a theory when no capable community chooses (or is able) to pursue it further. As such, tenacity is *epistemically unconstrained*.

2.4: A Tale of Two Principles: The Interplay of Proliferation and Tenacity

Feyerabend's pluralism, as I understand it, is the cooperation between the principles of proliferation and tenacity. As Feyerabend writes, "[t]he interplay between tenacity and proliferation which we described in our little methodological fairytale is also an essential feature of the actual development of science" (Feyerabend 1970d, 209). My thesis, to borrow a famous slogan, is that "Tenacity without Proliferation is empty and Proliferation without Tenacity is blind." Knowing that "[s]cience as we know it is not a temporal succession of normal periods and periods of proliferation; it is their juxtaposition" (212), tenacity and proliferation must be balanced somehow (cf. Andersson 1994, 55-6). Tenacity requires proliferation since without alternatives, we have limited the number of tests, criticisms, or potentially true theories. This is the meaning of "Tenacity without Proliferation is empty." Next, consider what proliferation would be *without* tenacity. Even conceiving a theory, let alone applying it to new domains, designing experiments to test it, and so forth requires getting over some initial confusions and difficulties. While this extreme case makes tenacity look trivial, remember that the principle of tenacity extends to any theory indefinitely. This is how proliferation of any sort requires some degree of tenacity to achieve its results. This is the meaning of "Proliferation without Tenacity is blind."[178]

So we know there must be *a* balance, but what will the balance *be*? Since tenacity and proliferation are unconstrained, we cannot delimit them by *epistemic* criteria. We can always tenaciously develop some old idea or proliferate new ideas. This makes the choice between the two, in any given case, arbitrary. These two principles must be balanced, but this balance does not come from methodological considerations.

2.5: Concluding Remarks

The principle of tenacity, like the principle of proliferation, becomes increasingly radical and unconstrained as Feyerabend's thought matures. He begins with the general acknowledgement that all theories are born refuted and require some persistence and ends up with the view that tenacity allows for the indefinite pursuit of *any* theory. I also argued that the

[178] Despite this complementary nature, tenacity and proliferation often, though not necessarily, make conflicting *practical demands*. Tenacity requires developing old ideas while proliferation requires inventing new ideas. Tenacity and proliferation can, and often are, complementary at the *social* level (or temporally by the same scientist(s)). This is a second way of resolving the apparent tension between tenacity and proliferation through the 'cognitive division of labour' (cf. Kitcher 1990) which addresses Stegmüller's (1976, 259) worry that Feyerabend is demanding that the scientific community must be filled with Einsteins and, therefore, has set an unrealistic standard for scientific practice. It is unclear, within Feyerabend's view, about whether tenacity and proliferation go together at the individual or social level (Farrell 2003, 212).

process of tenacity requires a commitment to improving a theory in light of criticisms that it faces, but such a commitment must be understood loosely to allow room for instances where theories naturally outgrow their problems.

3. Anarchism and Pluralism

'Anything goes', the central slogan of Paul Feyerabend's epistemological anarchism, is perhaps the most notorious phrase in 20th-century philosophy of science. Since its first appearance in 1970, it has provoked a largely critical response. Influential commentators have called this view 'inapplicable' (Agassi 2014), 'nonsensical' (Nagel 1977), 'difficult to take seriously' (Worrall 1978), and even 'completely absurd' (Bernstein 2011). The goal of this section is to understand epistemological anarchism and its relationship to Feyerabend's pluralism.

3.1: Two Interpretations of Anarchism

Feyerabend never held a single consistent understanding of anarchism. In AM_P, he calls anarchism a 'theory of error.' However, this view never reappears in Feyerabend's career or plays a dominant role in that paper and, therefore, won't be considered here.[179] However, there are two anarchisms that Feyerabend endorses throughout his career that are not obviously compatible. I call these readings the 'methodological opportunism' reading and the 'pluralist' reading. In this section, I outline both, though the pluralist interpretation will be of greater interest to me here.

The phrase 'anything goes' first appears in "Experts in a Free Society" (1970b).[180] Here, Feyerabend argues that scientists do not follow unique methods that are preferable to those in other professions. To do this, Feyerabend engages in the now familiar argument that even individual scientists, let alone science as a whole, do not follow any particular identifiable method. He writes:

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[179] Describing this view, Feyerabend writes "[w]e can only speak of what does, or does not, seem appropriate when viewed from a particular and restricted point of view, different views, temperaments, attitudes, giving rise to different judgments and different methods of approach. Such an *anarchistic epistemology* –for this is what our theory of error now turns out to be…" (21).

[180] However, AM_P was written first in 1968 (Feyerabend 1995, 139) and likely contained the phrase 'anything goes.'

> Neither Galileo, nor Kepler, nor Newton[181] use specific and well-defined methods. They are eclectics, methodological opportunists…looking at the actual historical situation we see that science was advanced in many different ways and that scientific problems were attacked by many different methods. In practice the only principle that is constantly adhered to seems to be *anything goes* (Feyerabend 1970b, 122-3).

Later, he writes that "great scientists, while intuitively adopting a methodological opportunism, *or anarchism*…" (emphasis added, ibid) straightforwardly equating 'anything goes' with methodological opportunism. Not only does Feyerabend think it's a *fact* of history that most great scientists have been opportunists (AM_B, 13), but he thinks it's *reasonable* that they are opportunists (14-17). Scientists "find [themselves] in a complex historical situation" and "no two individuals (no two scientists; no two pieces of apparatus; no two situations) are ever exactly alike and that procedures [i.e., methods] should therefore be able to vary also" (Feyerabend 1970b, 123). Additionally, Feyerabend equates anarchism with opportunism in every edition of AM_B.[182] He repeatedly and approvingly quotes Lenin and Einstein's remarks about the need to be an 'unscrupulous opportunist' (AM_B, 10)[183] and that "is it not clear that successful participation in a process of this kind is possible only for a ruthless opportunist who is not tied to any particular philosophy and who adopts whatever procedure seems to fit the occasion?" (ibid). As such, opportunism is consistent with the more recent metaphor of pluralism as a toolbox: for any given context there is a tool (or tools) that could be used (Waters 2011). Opportunism, for Feyerabend, is "the only type of behavior that has a chance of succeeding" (AM_B, 10).

On this view, anarchism is only true at the global level and not at the local level. In other words, 'anything goes' is true if try to formulate *general* rules of science but there are definitive rules for each context. Every historical situation is filled with unique constraints so not *all* options are available. These local constraints do not add up to the view that there is one correct method for a given situation. Pluralism, even at the local level, is often preferable. However, this plurality will not be an *unlimited* plurality (i.e., anything goes), but a tentatively constrained pluralism. Additionally, there can be degrees of correctness associated with a particular instance

[181] In other parts of his corpus, Feyerabend adds Mach (Feyerabend 1970a, 131), Einstein (AM_B, 10), Bohr (Feyerabend 1969b, 293), and Born (Feyerabend 1978a, 40 fn. 25) to this list.

[182] Since Feyerabend made many edits and retractions to AM, even removing and replacing entire chapters, the fact that these views are present in each edition is evidence that Feyerabend had some ongoing adherence to this view.

[183] Einstein's opportunism is a severely overlooked topic. Of the few who discuss it, it tends to be downplayed as a playful self-deprecating remark (Howard 1998, 3). Similarly, few Feyerabend scholars pay attention to these remarks. An exception is Oberheim (2016) who equates Einstein's views with Feyerabend's.

of opportunism that can be determined *ahead of time*. Since the opportunist needs at least some *foreknowledge* to flourish (e.g., what arguments will appear convincing, what will become accepted in the future, etc.), we do not need the *results* of opportunism to judge its validity. I will stop here, for now, but this view appears to contrast with the 'pluralist' reading of Feyerabend's anarchism.

The second reading of anarchism is that it is entailed by his formulation of pluralism. Recall the discussion of the interplay of proliferation and tenacity: if we accept these two epistemically unconstrained principles with no rules for balancing them, then 'anything goes.' 'Anything goes' was originally outlined as a *global* view of science. He famously writes:

> It is clear, then, that the idea of a fixed method, or of a fixed theory of rationality, rests on too naïve a view of man and his social surroundings. To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, 'objectivity', 'truth,' it will become clear that there is only one principle that can be defended under *all* circumstances and in all stages of human development. It is the principle: *anything goes* (AM_B, 11-2).

It is worth making a few remarks on the phrase 'anything goes.' It is clearly rhetorical, but there is a sense in which it accurately represents Feyerabend's mature view on methodology. As I have argued, 'any' can be properly applied to the content of proliferation, the method of proliferation and the conditions of pursuit. The *process* of pursuit follows definite rules, even if they are quite minimal: there must be multiple tenaciously pursued theories that critically interact. However, there is an ambiguity, here, that Feyerabend never explicitly resolves: Is there anarchism at the level of local decisions? The principles of proliferation and tenacity are *general* principles; can the balance between them be struck on a case-by-case basis for Feyerabend? The answer to this becomes clearer in Feyerabend's response to his critics (AM_{B2}, Chapter 18; Feyerabend 1977a, fn. 1 368; section 2 of 1977b; 1979b, 202-5).[184] He distinguishes between four views:

1) Old-fashioned rationalism (Descartes, Kant, Popper, Lakatos; ancestor the philosophy behind the apodictic laws of *Exodus*);

[184] Remember, Feyerabend's responses to his reviews must be taken with a grain of salt since he did not actually check AM to see if these arguments appear in AM. Given this, it may be the case that Feyerabend is espousing a new post-AM view here. However, some of the responses to the reviews make sense of particular passages from AM. As such, we can reasonably locate this view of anarchism in AM_{B1}.

2) A form of relativism that works towards…rank[ing] research programmes relative to stated goals of inquiry, background theory, and data; ancestor: the philosophy behind the case laws of *Exodus*… (Feyerabend 1979b, 203).
3) [B]oth absolute and conditional rules have their limits so that even a relativized rationality, when followed to the letter, may occasionally lead us astray and infers…that all methodological rules are therefore worthless.
4) My position agrees with [the first clause of 3, but not the second]. It argues for a contextual account, but the contextual rules are not supposed to *replace* the absolute rules, they are to *supplement* them (Feyerabend 1977a, fn. 1 368).

Chapter 1 elaborated on the arguments against 1). 2), ironically, looks like opportunism, which Feyerabend is now *contrasting* his real view. Indeed, Feyerabend's opportunism, when taken as a view of what projects to *pursue*, seems at odds with many of Feyerabend's other commitments. Recall from Chapter 1, Feyerabend thinks that knowledge grows in *unpredictable* ways. Opportunism, on the other hand, presupposes that we can make local level predictions that add up to long term decision making. Additionally, and more importantly, opportunism presupposes that we are able to recognize the method needed in a given context and adjust accordingly. Not only does this conflict with the principle of proliferation, since we are to proliferate methods that conflict with existing ones, but it conflicts with Feyerabend's conception of pursuit: what the 'right method' is in any given context is determined *after* pursuit, not *during* pursuit. It is because of this that I think that Feyerabend's opportunism is inconsistent with his pluralism. On a charitable reading, Feyerabend engaged in two mutually inconsistent lines of thought; he was being a practicing pluralist! However, we cannot see opportunism as an aspect of the position I have been attributing to Feyerabend throughout these two chapters.

4), and its rejection of 3), gives us some insight into the status of contextual rules of rationality. Feyerabend clarifies:

> I regard each piece of research [as] both as a potential instance of application for a rule *and as a test case of the rule*: we may permit the rule to guide our research i.e., to exclude some actions and to mould others, *but we may also permit our research to suspend the rule*, or to regard it as inapplicable even though all the known conditions demand its application. In making the latter decision we are not guided by any clear insight into the limitations of the rule… [w]e are guided, rather, by the vague hope that working without the rule, or on the basis of a contrary rule we shall eventually find a *new form of rationality*…This is also what is meant by the slogan 'anything goes': there is no guarantee that the known forms of rationality will succeed and that the known forms of irrationality will fail. *Any* procedure, however ridiculous, may lead to progress, *any*

> procedure, however sound and rational, may get us stuck in the mud (Feyerabend 1977a, fn. 1 368).

There is also evidence that Feyerabend held this view in AM. In the 1st edition, Feyerabend writes,

> [T]he cosmologists of the 16th and 17th centuries did not have the knowledge we have today, they did not know that Copernicanism was capable of giving rise to a scientific system that is acceptable from the point of view of 'scientific method'. They did not know which of the many views that existed at their time would lead to future reason when defended in an 'irrational' way. Being without such guidance they had to make a guess and in making this guess they could only follow their inclinations, as we have seen. Hence it is advisable to let one's inclinations go against reason *in any circumstance*, for science may profit from it (155-6).

Again, in AM_{B2}, "[s]cientists are like architects who build buildings of different sizes… who can only be judged *after* the event, i.e., only after they have finished their structure. It may stand up, it may fall down – nobody knows" (2, cf. Feyerabend 1978a, 39). If history is predictable, then the principle of tenacity would be constrained; only theories that we will be successful should be pursued. Similarly, Feyerabend's principle of fallibilism, which undergirds his views on proliferation, would become idle. Of course there is a *chance* that any pursuit will lead to success, but if we can make predictions within a reasonably high degree of accuracy, this 'chance' dwindles in practical force since we have minimal resources. It seems as if, then, that the radical nature of Feyerabend's pluralism depends crucially on his view that history is unpredictable.

I have outlined two readings of anarchism and defended the pluralist reading as more consistent with Feyerabend's corpus. Anarchism follows from unconstrained nature of proliferation and tenacity and clarifies that there is no context sensitive way of balancing them either. This shows just how closely linked Feyerabend's anarchism and his pluralism really are.

3.2: Pluralism and Progress

One of the most curious remarks Feyerabend makes relates to his views on progress:

> It should be pointed out that my frequent use of such words as 'progress,' 'advance', 'improvement', etc., does not mean that I claim to possess special knowledge about what is good and what is bad in the sciences and that I want to impose this knowledge upon my readers. *Everyone can read the terms in his own way* and in accordance with the tradition

> to which he belongs…*my thesis is that anarchism helps to achieve progress in any of the senses one cares to choose* (AM_B, 11).[185]

Even in Feyerabend's earlier career, he often speaks of progress but never defines it. Similarly, he speaks of the value of pluralism in terms of facilitating 'successful' or 'true' theories but never defines these terms. Now, he claims that rationalists must be anarchists making anarchism a candidate as an *objective* methodology for 'any' view of progress. It is worth pausing to figure out how this claim works.

Before this quote, Feyerabend writes, "theories become clear and 'reasonable' only *after* incoherent parts of them have been used for a long time. Such unreasonable, nonsensical, unmethodical foreplay turns out to be an unavoidable precondition of clarity and empirical success" (ibid). Feyerabend repeats this argument later on:

> what appears as 'sloppiness', 'chaos' or 'opportunism' when compared with such laws [of reason] has a most important function in the development of those very theories which we today regard as essential parts of our knowledge of nature. *These 'deviations', these 'errors', are preconditions of progress.* They permit knowledge to survive in the complex and difficult world which we inhabit…Without 'chaos', no knowledge" (AM_{B1}, 179).

Feyerabend's case studies are meant to show how today's rationality originated from violating yesterday's rationality. Feyerabend writes:

> The ideas survived and they can *now* be said to be in agreement with reason. They survived because…all the elements that characterize the content of discovery, *opposed* the dictates of reason *and because these irrational elements were permitted to have their way*. To express it different: *Copernicanism and other 'rational' views exist today only because reason was overruled at some time in their past*. (The opposite is also true: witchcraft and other 'irrational' views have *ceased* to be influential only because reason was overruled at some time in *their* past) (155).

Here, we see that it isn't rational rules that brought about successful theories but *their violations*. These violations are brought about by proliferation. The principle of tenacity prevents us from making claims about how proliferation *will turn out*. Progress will ultimately be achieved by

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[185] Feyerabend makes the same claim in earlier papers (Feyerabend 1965b, 150-1; 1972, 177).

practicing anarchism first.[186] Strangely enough, Feyerabend had the basic structure of this argument available in a letter to J.J.C. Smart (circa 1963):

> This methodological consideration shows that there exists an interesting asymmetry regarding the views of Nagel, Sellars, and myself. The asymmetry consists in the methodological necessity to start with my view. Nagel and Sellars can never find out whether they are not unduly restricting research. But I can find out whether I am not unduly expanding it. Hence, if I am right, and if there exists no limit to human reason, then obviously my view should be adopted. However if I am wrong, that is if there do exist inborn limits to human reason, then my view and my procedure must be adopted too, for this is the only way to discover the limits. Now there either exist limits, or there don't. Hence, my view must in any case be adopted. And considering the success of those who have adopted it in the past I have all confidence that my view is not only the necessary first step, but is also right (in Oberheim and Collodel forthcoming (b), 175).

The 'methodological consideration' Feyerabend is referring to here is the following:

> let us now assume, contrary to what emerged as plausible, that Kant is right and that we are incapable of leaving the boundaries of a certain world view. How do we find out that this incapability exists? And how do we discover what it is we cannot shake off?... The only way to discover whether there are indeed restrictions to our way of seeing the word is to resolutely try many different approaches... Only if such a procedure fails again and again, only then may we try to explain these failures by the hypothesis of an unalterable inborn world view (but even here we must be careful. What has been impossible for 3000 years may become possible tomorrow. Even within the rather restricted world view of science it took about 2000 years until the atomic theory could be finally regarded as satisfactory) (174-5).

We have already seen why Feyerabend thinks we *must* use alternatives: any attempt to analyze the content of a theory will use other concepts from that theory and therefore will be circular. This, at best, establishes the *consistency* of a theory and does not show its *limitations*. Limitations are discovered by *contrast*. As such, if there are inherent limitations to *any* theory, they can only be discovered by proliferating *first*. Anarchism, therefore, is a *precondition* for objective knowledge.

But still, we may wonder, mustn't Feyerabend provide some definition of the goal of his pluralism? What is 'success' that is achieved by pluralism? One answer is that Feyerabend is presupposing exemplars of what his opponents take to be successful (e.g., quantum mechanics,

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[186] Tambolo argues that Feyerabend defends a view of progress as "steady increase of competing alternatives" (Tambolo 2015, 33). This reading is incompatible with mine since I argue that Feyerabend has no view of progress, but merely argues that anarchism is conducive to any kind of non-linear progress (including unification).

general relativity, etc.). But this isn't enough, since Feyerabend wants to defend pluralism as his *own* position which requires that *he* defend a notion of a success. It seems as though success, for Feyerabend, would be ultimately an *value-laden* concept since it is a term of appraisal and values, as we saw, provides the foundation of appraisals. As we also saw, what constitutes values also change throughout time as a result of pluralism. Therefore, what constitutes 'success' changes as a result of the practice of pluralism. This means that there is no steady aim of anarchism, the goals of anarchism are constantly switching. Whatever the goal is at t_1 can be undermined by practicing anarchism and changing the goal at t_2. However, no matter what the goal is, and regardless of whether it's held by rationalists or not, anarchism will *always* be needed to discover what our goals truly are. In this sense, anarchism is suitable for progress in any way one chooses to characterize the goals of research.

It is quite satisfying to see how nicely this hangs together with Feyerabend's earlier arguments regarding the unexpected sources of success. But there are two pictures that we should not confuse when acknowledging the role anarchism plays in the development of knowledge. The first picture is that first we proliferate many ideas, sort through them tenaciously, and a successful theory emerges. This is reminiscent of van Fraassen's evolutionary tale of scientific success where we expect success from theories that survive from rigorous experimentation and intense criticism:

> I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature (van Fraassen 1980, 40).

On a Feyerabendian picture, this particular usage of the evolutionary metaphor is misleading in a few ways. First, theories 'survive' mostly because of tenacity that is applied consciously, not just because they had some natural capacity which was selected by its methodological environment. As such, it is historically contingent that one theory outgrew other competing theories. Since all theories that are pursued will be inherently limited, and such limits are discovered by the pursuit of inconsistent theories, we must pursue multiple competing theories *simultaneously*. Success is a by-product of this ongoing interaction between theories. As such, it isn't: anarchism first, success later. Rather, the picture Feyerabend is presenting us with is: anarchism always and success emerges.

3.3: Qualifications of Anarchism

Despite the fact that I have portrayed anarchism as the joint adherence to the principles of proliferation and tenacity understood as unconstrained, it is important to make a few remarks about what constraints could be made on Feyerabendian grounds. This section will consider two: ethical constraints and constraining 'the cranks.'

Feyerabend's anarchism is an *epistemological* anarchism. Feyerabend is silent on what implications anarchism has for ethics. I have already dealt with this issue, partially; there are some instances where instances in which proliferation or tenacity will conflict with moral considerations. Therefore, at the very least, we *can* place ethical constraints on anarchism in certain cases. In the final chapter, I shall consider whether the fact that particular communities value certain knowledge more than others (e.g., cure for cancer over increasing the accuracy of the escape velocity from Titan) or the urgency of particular issues (e.g., climate change, cures for disease epidemics) can constrain scientific practices. For now, I want to make the more general point that ethical considerations *can* constrain theory pursuit, at least in some cases.

The second restraint is also quite tricky. Some have argued that Feyerabend actually holds some unsystematic 'minimal rationality' or the deflated rationality used in ordinary affairs (Lugg 1977).[187] There is certainly textual support for this view. Most notably, throughout Feyerabend's texts he makes numerous disparaging remarks about 'the cranks.' In 1964, he writes:

> The distinction between the crank and the respectable thinker lies in the research that is done once a certain point of view is adopted. The crank usually is content with defending the point of view in its original, undeveloped, metaphysical form, and he is not prepared to test its usefulness in all those cases which seem to favor the opponent, or even admit that there exists a problem. It is this further investigation, the details of it, the knowledge of the difficulties, of the general state of knowledge, the recognition of objections, which

[187] Lugg argues that in AM, Feyerabend argues merely that science has no *distinctive* rationality. Science merely uses the rationality of common sense (though what constitutes 'common sense rationality' is left unspecified). I think this particular view is mistaken, specifically because Feyerabend repeatedly claims that 'common sense' contains older influential views of rationality and must be criticized. For now, I merely want to focus on the kernel of truth that Feyerabend has some quasi-principled resistance to accepting 'anything goes' full-stop.

> distinguishes the 'respectable thinker' from the crank. The original content of his theory does not (Feyerabend 1964b, 305).

Additionally, Feyerabend complains that much of the practice of current astrology makes "no attempt to proceed into new domains and to enlarge our knowledge of extra-terrestrial influences; they simply serve as a reservoir of naïve rules and phrases suited to impress the ignorant" (Feyerabend 1978a, 96). Moreover, one can infer what kind of proliferation Feyerabend is interested in based on whom he cites. For instance, in his defense of Voodoo, he doesn't defend con-artists on Bourbon street but the sophisticated and extensive work by C.R. Richter and W.H. Cannon (AM_B, ft. 7 30) which is scientific by any reasonable standard![188] Similarly, Feyerabend complains about 'intellectual pollution' alongside of rationalists where "illiterate and incompetent books flood the market, empty verbiage full of strange and esoteric terms claims to express profound insights, 'experts' without brains, without character, and without even a modicum of intellectual, stylistic, emotional temperament tell us about our 'condition' and the means of improving it" (219). Feyerabend's lack of defense of the 'cranks'[189] lends further support to the view that Feyerabend's anarchism is not literally unconstrained. We, therefore, get a partial restriction on scientific activity. I will return to the issue of the cranks in the following chapter as Feyerabend never tackles this question himself.

3.4: Concluding Remarks

While many have dismissed Feyerabend's anarchism as an exaggerated reaction to rationalism, it should be clear that anarchism was not simply a rhetorical device but a well thought through position. In this section, I have shown how anarchism follows from Feyerabend's principles of proliferation and tenacity. I have also argued that anarchism represents a *permanent* feature of science and is not limited to revolutionary or immature science. Finally, I remarked about constraining science on ethical grounds and to avoid the

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[188] The case is more difficult with witchcraft and ancient Chinese medicine since his references are more oblique and sporadic. For witchcraft, Feyerabend writes "every witch trial, every confession of presence at a witches' Sabbath, every otherwise unexplained occurrence of pestilence, floods disease of cattle, impotence, nightmares, unfaithfulness of wives…malformation at birth, schizophrenia – all these things would give direct proof of the existence of incubi, succubi, and other evil spirits, so that finally denying them would seem to be as nonsensical, arbitrary, and whimsical as the denial of the existence of tables and chairs appears to some "philosophers" of today" (Feyerabend 1965b, fn. 8 225). See chapter 4 of *Against Method* for a somewhat sustained discussion of ancient Chinese medicine and "The Strange Case of Astrology" in *Science in a Free Society* for his defense of astrology.

[189] Remember, Feyerabend defends the use of the cranks' *ideas* (AM_B, 26). This can also be seen in the 1964b quote where he writes that the *content* does not distinguish the respectable thinker from the crank.

cranks. I will assess the plausibility of this view in the following chapter. For now, I will try to further clarify my interpretation of Feyerabend by contrasting it with the most prominent readings in the secondary literature.

4. Contrast with the Secondary Literature

Within the last 20 years, there has been a renewed interest in Feyerabend. However, much of this analysis, especially on his pluralism and anarchism, remains superficial. In this section, I outline three interpretations of Feyerabend's pluralism and compare them with my own. This will situate my position within the secondary literature and clarify my stance.

4.1 Preston on Feyerabend's Pluralism

The first synoptic treatment of Feyerabend's philosophy came from John Preston's *Feyerabend: Philosophy, Science and Society* (1997)[190] where he devotes one chapter on Feyerabend's theoretical pluralism and another on his anarchism. My interpretation diverges from Preston's at three junctures: Feyerabend's views on meaning and testability, the principles of Feyerabend's pluralism, and the nature of anarchism.

Preston distinguishes among three features of Feyerabend's pluralism: theoretical pluralism, 'pluralist methodology' (or 'pluralistic test model'), and the principle of proliferation. The argument for theoretical pluralism is essentially the argument in Feyerabend's 1962b: Because there are only theory-dependent facts, theories compete with other theories. Maximizing the number of theories maximizes the empirical content of a theory. There are two problems with his interpretation. First, nowhere does Preston recognize that Feyerabend's notion of a 'test' has evolved. Tests do not necessarily require a comparison of *meanings*; there are many means of theory comparison. This is the opposite of what Preston suggests. Preston argues that theoretical pluralism requires a commitment to the view that the meaning of a term requires understanding its use in a theory (134). Theoretical pluralism, on Preston's view, stands or falls with Feyerabend's semantics. Secondly, Preston misidentifies Feyerabend's mature formulation of a theory. Preston *first* cites Feyerabend's extremely loose conception of a theory from his

[190] The first book devoted to Feyerabend is George Couvalis' *Feyerabend's Critique of Foundationalism* (1988b). However, this book is focuses on Feyerabend's early work on meaning and observation and not with pluralism directly.

1965a/1965b, which he calls "intolerably vague" (26), and *then* cites the more stringent definition from 1962b. This notion of a theory grounds Preston's interpretation of theoretical pluralism where all theories must be factually adequate. However, as I have shown, Feyerabend is intentionally loosening his view of what constitutes a theory. As a result, Preston wrongfully claims that Feyerabend's mature conception of theoretical pluralism requires having multiple factually adequate theories. The interpretation I give simply requires that two theories, broadly understood, 'clash.' This sufficient for revealing natural interpretations, formulating new criticisms, and providing psychological and humanitarian benefits. Preston, therefore, attributes to Feyerabend an overly narrow view of pluralism that both underplays its centrality and makes its plausibility depends on his early views on semantics.[191]

Preston then articulates Feyerabend's 'pluralistic methodology', which is meant to "decid[e] between existing theories [and] for regulating the construction of new theories" (Preston 1997, 137).[192] Preston defines this methodology as a conjunction[193] of the following principles:

(1) Principle of Falsification: Take refutations seriously.
(2) Principle of Revision: Admit no unrevisable statement into the body of our knowledge. In any conceptual change, regard no statement…as incorrigible, irrefutable, unalterable, or *a priori*.
(3) Principle of empiricism: Maximize the empirical content of existing theories.
(4) Principle of testability: Use only theories which are testable. Put theories in a form in which they are maximally testable, and test them…relentlessly.
(5) Principle of realism: Develop theories in their strongest possible form, i.e., as attempts at universally quantified descriptions of reality rather than mere instruments of successful prediction.

[191] As a side note, Preston remarks "[t]heoretical pluralism…should not be confused with *methodological* pluralism. Commentators sometimes take Feyerabend to be advocating the latter even in his pre-1970 work, but this would be a serious mistake. The pluralistic test model is intended to be a single methodology for all scientific inquiry. It sponsors the proliferation of theories, but not of methods for evaluating theories…When Feyerabend became a methodological pluralist, he had officially forsworn the resources which originally allowed him to argue for theoretical pluralism" (Preston 1997, 139). This statement is misleading in two ways. First, the pluralistic test model *cannot* be implemented for 'all scientific inquiry' but only with fully developed theories. Second, the pluralist test model presupposes the use of theories that are independently confirmed and therefore presupposes *some* other method of evaluation.
[192] I have argued that Feyerabend's pluralism applies to theory pursuit, not 'deciding between existing theories.'
[193] Preston makes no attempt to describe how these principles hang together. He simply claims, correctly, that "[t]here is no indication that Feyerabend ever put these rules together in his mind as a single pluralistic methodology" (138).

(6) Principle of proliferation: Invent, and elaborate, theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted.
(7) Principle of tenacity: From a number of theories, select the one that has the most attractive features and that promises to lead to the most fruitful results. Stick to it even if it is inconsistent with evidence, or encounters considerable difficulties (137-8)

Despite the vagueness of this view, there are still several problems with it. Preston cites papers from 1958-1970 in support of these principles with no discussion of their evolutions.[194] Depending on how (1) is interpreted,[195] Feyerabend never subscribes to (1). Preston is right to suggest that Feyerabend held (2) throughout his pre-1970 career; but (2) must be qualified to state that we should admit no *permanently* unrevisable statement into our methodology since Feyerabend recognizes that we must tentatively work with uncriticised assumptions. Preston is right that pre-1970 Feyerabend consistently held (3) but, as I mentioned before, he misses the nuance in the evolution of Feyerabend's view on testability. While it is strange to separate (4) from (3), since the testability of a theory is its empirical content, the clause 'use only theories which are testable' is false since we can use myths as critical instruments. (5), I have argued, follows from either (4) or (7) and shouldn't be considered an independent principle. For (6), Preston recognizes the evolution of proliferation. He claims that:

> Although the principle [of proliferation] occurs over a significant period in Feyerabend's work, his understanding of it changes. Initially, he understands it to require that newly introduced theories should have various virtues…But he slides from this more modest principle to….an unrestricted principle of proliferation which puts no restriction whatsoever on the calibre of the alternative theories (139).

However, Preston follows this by asserting that the theoretical pluralism is not wedded to the principle of proliferation. This is because, for Preston, only theories past a certain 'threshold point' (140) of factual adequacy can be used for increasing the empirical content of other theories:

> The pluralistic test model works with 'a whole set of partly overlapping, *factually adequate*, but mutually inconsistent theories', not any old bunch of half-baked ideas. This 'condition of factual adequacy' cannot, *pace* Feyerabend, be 'removed'…for it is only an acceptable theory which has a chance of truly superseding…and thereby replacing an older theory (ibid).

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[194] Preston also claims that Feyerabend accepted these principles "before the onset of 'epistemological anarchism'" (137) and yet cites AM_P as textual support for these principles alongside Feyerabend's papers during the 60s.
[195] If (1) is interpreted as the weak claim that scientists should consider purported falsifications, then he is correct.

Remember, however, that Feyerabend draws a distinction in his 1965a between theories qua critical instruments and theories to *supplant* other theories (‘strong alternatives’). The factual adequacy condition is necessary for the latter but not the former. Since the post-1965 principle of proliferation incorporates *all* these features, it is perfectly legitimate, in some cases, to loosen the factual adequacy constraint.

Finally, let’s consider Preston’s interpretation of anarchism. Preston sees pluralism as a phase *before* anarchism. I have argued that Feyerabend abandons Preston’s understanding of theoretical pluralism in 1965 and that Feyerabend’s understanding of pluralism evolves into anarchism. Preston most likely missed this connection because he has not fully appreciated the complementary nature of proliferation and tenacity. Preston barely discusses tenacity. Yet Feyerabend, at numerous points, makes it clear that tenacity is a necessary condition for proliferation. Additionally, Preston misreads anarchism leading him to form an unfair criticism of it. Preston summarizes the argument for ‘anything goes’ in a very simplistic fashion: there are historical counterexamples to any given theory of rationality. This argument, Preston claims, only works against Popperians who believe that methodological rules are good only insofar as they are exceptionless. Therefore,

> Having criticized ‘critical rationalism,’ for failing to realize that there are no such [methodological] rules, Feyerabend announces that there are no methodological rules at all. This strategy is obviously bankrupt… Inductive rules are perfectly good rules and can be followed, can guide action towards a goal (174).

In other words, “[i]t is because Feyerabend is a Popperian deductivist *manqué* that he sees methodological anarchism as the only (other) option” (ibid). Preston thinks rules of induction are ‘perfectly good’[196] because they are fallible. He quotes the following passage from Newton-Smith:

> We should expect our rules to have a high risk factor. If our rules are too safe…they may cushion us from error at the cost of minimizing the number of contexts in which we actually end up adopting a theory. Thus, to have evidence of a number of occasions in which some rule has led us astray is not necessarily to have an adequate reason for doubting the acceptability of the rule. It may be, these exceptions notwithstanding, that

[196] Preston appears to have forgotten Feyerabend’s arguments against induction.

> our chance of progress in the long run is greater if we employ the rule (Newton-Smith 1981, 129).

However, even the fallibilist use of induction isn't sufficient to get around Feyerabend's criticisms. First, as his Galileo case study demonstrates, counterinduction is necessary for revealing natural interpretations. Since all observational evidence is *always* and *necessarily* blanketed with natural interpretations, violating empiricism is a *necessary condition* for maximizing testability. Second, for Feyerabend, every piece of research is potentially a test case of a rule or an attempt to discover new rules of rationality. We need not be conservative and cling to those rules in use. We can always experiment with new methodologies. This is not entirely dependent on the historical record; it follows from Feyerabend's views on *fallibilism* and *testability*. We can be wrong in any number of ways; the best way to discover how is by experimenting with a wide variety of methods to reveal the faults of existing ideas. Third, as outlined in the previous chapter, Feyerabend thinks these norms are *frequently* violated, not just occasionally. Preston, therefore, provides an inaccurate presentation of Feyerabend's views on anarchism and their relationship to pluralism and so his criticisms miss their mark.

4.2: Farrell on Feyerabend's Pluralism

Farrell begins his section on pluralism by correctly stressing its importance within Feyerabend's philosophy:

> The most long-lived, ubiquitous and deepest theme of Feyerabend's philosophy is *pluralism*. The changes in Feyerabend's philosophy, over the decades is best interpreted as the gradual drawing out of the consequences of a pluralistic philosophy: pluralism is the hard-core of the Feyerabendian philosophical program and it came to permeate all aspects of his thought (Farrell 2003, 135).

While I could not agree with this statement more, I still take issue with his interpretation of Feyerabend's views on pluralism. Specifically, I take issue with Farrell's views on the cause of the split between Popper and Feyerabend and his view of the nature of context-dependent rules for Feyerabend.

Contra Preston, Farrell argues that Feyerabend remained a committed Popperian throughout his career at the 'meta-methodological level' (Farrell 2000, 264).[197] The meta-methodological level is what he calls Popper's 'axiological normativity' or, to use more standard jargon, conventionalism of methodology. This conventionalism, Farrell argues, is grounded on a *value* decision. The methodological level is simply Popper's falsificationism, which Feyerabend comes to "indict as dogmatic" (260). Farrell gives three primary motivators for this indictment, though I will focus on just one. Farrell argues that Feyerabend's "growing awareness of the rational viability of instrumentalist stratagems" (262) played a pivotal role in his rejection of falsificationism. Farrell interprets Feyerabend's 1964b as arguing that,

> Local instrumentalism is an unobjectionable move in science. For example, Feyerabend argues that the proposal by Osiander to treat the Copernican hypothesis instrumentalistically [sic] enabled the Copernican hypothesis to survive. It the Copernican hypothesis had been treated realistically, then…it should have been rejected as utterly falsified (261).

Similarly, Farrell reads Feyerabend's 1969b as defending Bohr's position as an instance of local instrumentalism. Against this, I read Feyerabend's 1964b as rejecting both local[198] and global instrumentalism. Local instrumentalism conflicts with *tenacity* whereas global instrumentalism conflicts with *testability*. His disagreement with Popper is about whether methodology alone can refute *both* local and global instrumentalism. Feyerabend's defense of Bohr is that his local instrumentalism was arrived at via pluralism and is not defended under rationalist pretences.

Secondly, I think that Farrell's interpretation of anarchism is somewhat confused. Farrell describes Feyerabend's anarchism as "the context-dependent supplication of specific rules in specific situations" (Farrell 2003, 157). He also writes that "Popper's system, for example, may have strengths in certain areas; empiricism may also have advantages in certain contexts. Consequently, the 'Rationalist' may pick and choose individual rational rules which have been shown to be independently efficacious" (45) and compares Feyerabend's new view of rationality to Aristotelian contextual reasoning where we determine the correct thing to do in a given

[197] Farrell (2000, 263-4) provides an alternative way of reading this claim where Feyerabend remained a 'comprehensive critical rationalist' where "the rationalist identity might be characterized as that of one who holds *all* his beliefs, including his standards and his basic philosophical position itself open to criticism" (Bartley 1962, 30).

[198] In chapter 13 of AM_{B2}, Feyerabend defends Osiander and Bellarmine's *ethical* reasoning for treating the Copernican hypothesis instrumentally (cf. also Feyerabend 1981c, xi). This, however, is distinct from Feyerabend's methodological accusation.

context given the rationally salient features of the situation (210). This, in a nutshell, is Farrell's interpretation of Feyerabend's pluralism: globally we are pluralists, but locally we are constrained pluralists.[199] This seems compatible with the methodological opportunist reading, though Farrell never cites Feyerabend's remarks on this subject.[200] This is certainly incompatible with my interpretation of anarchism as an outcome of his pluralism. On my interpretation, Feyerabend is committed to an epistemically unrestricted pluralism in any situation; "it is advisable to let one's inclinations go against reason *in any circumstance*" (AM_B, 116). We, therefore, disagree about how to interpret Feyerabend's mature pluralism.

4.3: Oberheim on Feyerabend's Pluralism

Oberheim's interpretation of Feyerabend's pluralism is the most detailed and influential within the secondary literature. There are three sources of contention between Oberheim's interpretation and my own: his aversion to 'unificationist' renditions of Feyerabend's pluralism, the role he allots to incommensurability in Feyerabend's pluralism, and his claim that Feyerabend's pluralism is grounded on empiricist principles.[201]

Oberheim takes issue with 'unificationist' readings of Feyerabend's pluralism. That is "presenting Feyerabend as having attempted to set *out a single, new, normative methodology*" (Oberheim 2006, 211). My strategy throughout this chapter has been to find common themes in Feyerabend's publications and show their continuity. The development of the principles of

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[199] He writes "[r]easoning, for Feyerabend, is something done within a context; consequently, in identifying a context, it may be apparent that there was *only a limited number* of rational options available to the people within that context" (emphasis added, Farrell 2003, 67). This shows that Farrell isn't interpreting Feyerabend as a contextual monist.

[200] The views aren't entirely identical. Farrell, in chapter 8, argues that Feyerabend does maintain some kind of 'minimal rationality' whereby we can rank methods into some kind of hierarchy ("Feyerabend believes that some ideas are more rational than others" (208)) and that we can *completely* reject certain ideas ("[we] can *unequivocally* reject astrology, *as it is now practiced*, as having any rational merit in *comparison with current scientific alternatives*" (208-9)). My methodological opportunist reading is not committed to this claim.

[201] It should be mentioned that Oberheim at some points claims that Feyerabend is not advancing any positive views but launching a series of 'immanent criticisms' (Oberheim 2006, 12). In other words, every apparently positive position Feyerabend appears to have held, including pluralism, is actually just a manner of showing some flaw in other philosophical views (Feyerabend "was an undercover agent playing the game of empiricism [and falsificationism], in order to undercut the authority of experience" (226)). However, he also claims "almost all of [Feyerabend's] major publications, and even in most the minor ones, contain some form of a methodological argument for pluralism" (fn. 338 246). I'm not sure what to make of this tension in Oberheim's view nor do I think he has provided sufficient textual evidence to support his view that Feyerabend had no positive philosophy.

proliferation and tenacity, Feyerabend's gradual rejection of falsificationism, and his turn towards history have clear trajectories that culminate in AM. Now, Oberheim argues that Preston's reading of Feyerabend's pluralism (given above) is internally inconsistent. This is not an accidental feature of Preston's interpretation, but an inevitable feature of interpreting Feyerabend. Oberheim writes:

> How are these inconsistencies in Feyerabend's alleged methodological position to be reconciled? Are we really to believe that these conflicting recommendations were supposed to be *universally valid* methodological rules[202] within a *single* coherent methodology applicable to all of science? If this were the case, then Preston's general complaint that Feyerabend's ideas are inconsistent would indeed be justified. But perhaps Feyerabend was suggesting that *sometimes*, scientists should retain their theories even in light of apparently falsifying facts, whereas *at other times*, scientists should take falsifying facts seriously, and decide to pursue alternative views?...The obvious discrepancies among these principles suggest that *they were not supposed to be universally valid prescriptions in the first place*, but rather context dependent prescriptions (212-3).

The unificationist strategy is bound to fail since the principles Feyerabend supports make differing demands. However, this depends on what Oberheim means by 'universal.' If he means "we *must* always be tenacious" then he is correct, but Feyerabend never defends this view. If he means "we *can* always be tenacious (or proliferate)" then Oberheim is wrong since Feyerabend defends tenacity and proliferation as *completely unconstrained* and, therefore, can *always* be implemented.

Second, Oberheim contends that incommensurability plays a vital role in Feyerabend's mature pluralism. While Oberheim acknowledges that Feyerabend's incommensurability changes throughout his career, he does not accurately highlight what those changes are. He writes:

> In 1962[b], Feyerabend talks about the impossibility of inter-defining the concepts of incommensurable theories. In 1965[a], he characterizes his notion of incommensurability by claiming that two theories are incommensurable when the meanings of their main descriptive terms depend on mutually inconsistent principles. Later, he claimed that

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[202] In fairness to Preston, he never claims that the *individual* principles Feyerabend adheres to are universal; he claims they are *jointly* universal.

> “[w]hen using the term “incommensurability” [he] always meant deductive disjointedness, and nothing else (Feyerabend 1977[a], 365)” (Oberheim 2006, 133).[203]

There are a few problems with this passage. First, the third quote is a retrospective remark about Feyerabend’s earlier views on incommensurability, not the formulation of a new view. Second, the impossibility of inter-defining concepts is a *result of* having theories with different inconsistent principles (see Feyerabend 1965c, 98-9). Regardless, Oberheim claims that, “these changes do not represent a development in a positive sense… [But] reflect his changing interests” (Oberheim 2006, 134). This is true, but not in the way Oberheim characterizes it. Feyerabend 1962b defends the *use* of incommensurable theories. However, once Feyerabend realizes that many entities have the same function, the strictness of incommensurability qua deductive disjointedness becomes superfluous. Theories must merely be *incompatible* in a looser sense. Incommensurability remains a problem for empiricist views of reduction and explanation and comparison via empirical content or verisimilitude, but it is no longer a necessary condition for proliferation or tenacity. So when Feyerabend claims that, for example, “[i]t *is* possible to use incommensurable theories for the purpose of mutual criticism” (Feyerabend 1965a, 117), he is simply stating that proliferation *can* include incommensurable theories but doesn’t *need to*. Again, in AM_{B1}, Feyerabend writes that progress is an “ever increasing ocean mutually incompatible *(and perhaps even incommensurable)* alternatives” (emphasis added, 30).[204] Even Oberheim’s claim that incommensurability provides “*superior* or ‘sharper’ means for testing our beliefs” (Oberheim 2006, 246) is inaccurate. While Feyerabend does say that “[a]lternatives will be more efficient the more radically they differ from the point of view to be investigated” (Feyerabend 1965b, 214), Oberheim is wrong to infer that “[he] argue[s here] that a plurality of competing *incommensurable theories* is best suited to promote progress” (Oberheim 2006, 252).

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[203] He goes onto to say “In the mid-1970s, Feyerabend presented a very different conception of incommensurability, which he applied to the transition from the Greek archaic, aggregate worldview of Homer to the substance world-view of Aristotle” (Oberheim 2006, 133). The ‘very different conception of incommensurability’ Oberheim is referring to here is Feyerabend’s *anthropological* understanding of incommensurability which differs substantially from ‘logical incommensurability.’ I will not detail these differences here, but simply state that this view plays no substantial role in Feyerabend’s pluralism. The goal of anthropological investigations of incommensurability may be an aid to certain features necessary for pluralism in certain cases (facilitating cross-disciplinary communication), but it plays no justificatory role for the principles of proliferation or tenacity.

[204] Again, Feyerabend writes “I never said…that *any two* rival theories are incommensurable…What I *did* say was that *certain* rival theories, so-called ‘universal theories’…. *If interpreted in a certain way*, could not be compared easily. More specifically, I never assumed that Ptolemy and Copernicus are incommensurable. They are not” (AM_{B1}, 114).

Being 'different' is not the same as being incommensurable. Pluralism, therefore, does not require incommensurability since (at least) 1965. Feyerabend puts this point quite nicely:

> I think that incommensurability turns up when we sharpen our concepts in a manner demanded by the logical positivists and their offspring and that it undermines their ideas on explanation, reduction and progress. Incommensurability disappears when we use concepts as scientists use them, in an open, ambiguous and often counterintuitive manner. Incommensurability is a problem for philosophers not for scientists (AM_B, 211).

Insofar as pluralism aims to aid science, incommensurability plays no substantial role.

Oberheim also claims that accepting Feyerabend's pluralism is conditional on accepting empiricism. Remember Preston's seven principles of Feyerabend's pluralistic methodology:

(1) Principle of Falsification
(2) Principle of Revision
(3) Principle of Empiricism
(4) Principle of Testability
(5) Principle of Realism
(6) Principle of Proliferation
(7) Principle of Tenacity

Of these principles, Oberheim writes, "Feyerabend argued for the last three principles *on the basis of* the first four principles [which constitute empiricism]" (214). Oberheim gives no reason for this interpretation, but refuting it will help to clarify my own thesis. These principles are meant to encapsulate Feyerabend's *mature* pluralism. Recall, I argue that Feyerabend never subscribed to (1), principles (3) and (4), once suitably modified, are identical, and (5) is better understood as the broader principle of fallibilism which also encompasses (2). However, remember that pluralism is also justified on *humanitarian grounds* as well as the principles of fallibilism and testability. The final picture of Feyerabend's pluralism becomes:

(1) Principle of Humanitarianism
(2) Principle of Fallibilism
(3) Principle of Testability
(4) Principle of Proliferation
(5) Principle of Tenacity

For Feyerabend, (1) is both co-extensive with and justifies (2)-(5). Remember, (2)-(5) are adopted conventionally and conventions, for Feyerabend, are grounded on ethical (i.e., humanitarian) choices. (3) presupposes (2) since there is no point in testing a claim that can't be falsified. (4) is directly justified by (1) and presupposes (2) and (3); when proliferation is meant for the sake of criticism, this presupposes that what it criticizes can be revised. (5) and (4) are mutually dependent since proliferation without tenacity is blind and tenacity without proliferation is empty. Finally, each of these principles are *universal*. (2) and (3) are *always true*, and we can act on (4) and (5) at *any* time. Therefore, it isn't *empiricism*, in Oberheim's sense, that justifies pluralism, but *humanitarianism* and the *rejection of rationalism*.

5. Chapter Summary

While Feyerabend never systematically combined these principles into a single package, I have attempted to accomplish this in a manner that is consistent with Feyerabend's written work. The principles of fallibilism and testability provide the motivation for the principles of proliferation and tenacity. This comprises Feyerabend's conception of scientific methodology which rests on his humanitarianism. Since Feyerabend's later work during the early 80s drastically changes its focus, these principles were not further developed. The following chapter will fill this gap by providing a critical appraisal of Feyerabend's pluralism.

Chapter 3
There and Back Again: An Analysis of Feyerabend's Pluralism

> "We are all agreed that your theory is crazy. The question that divides us is whether it is crazy enough to have a chance of being correct" (Bohr 1998, 84).

In the previous chapter, I have reconstructed Feyerabend's pluralism as the balance between proliferation and tenacity. Proliferation is justified in a fourfold way: by maximizing testability, criticism, humanitarianism, and increasing our chances of discovering true theories. Tenacity is justified by the need to develop theories despite their empirical and theoretical problems. However, Feyerabend provides us with no way to balance the two principles nor is it clear precisely what they entail, practically speaking. These more fine-grained points are never answered in Feyerabend's corpus, but he does provide hints as to how they may be answered. In this chapter, I will attempt to clarify the nature of Feyerabend's pluralism by providing answers to these questions. While this will, ultimately, require a reformulation of some of Feyerabend's conclusions, his more profound insights into the nature of scientific methodology will be retained.

Given how quickly many philosophers have dismissed Feyerabend's views, the position I outlined in the previous two chapters has gone largely unscrutinised. What criticisms there have been are grounded in largely erroneous readings of Feyerabend's pluralism. Regardless, these criticisms are worth addressing to make it exceptionally clear what assumptions Feyerabend's pluralism rests on. These criticisms are certainly not exhaustive of all the criticisms that could be launched at this view. Since Feyerabend's pluralism requires assumptions in semantics, epistemology, methodology, the nature of HPS, and empirical assumptions about understanding, perception, social psychology, history, theory pursuit, etc., this chapter can only offer the first steps towards discovering what's salvageable in Feyerabend's view 40 years after its inception. Regardless, we must start somewhere. After this, I clarify what the task of constructing a 'logic of pursuit' consists in. This will help us better understand how to formulate Feyerabend's pluralism. After accomplishing this, I address what I see as the most prominent hole in Feyerabend's philosophy: the lack of a balance between the principles of proliferation and tenacity. I argue that a principled balance can be found, primarily, in the 'economics of discovery' of C.S. Peirce.

This chapter is broken down into three primary sections. In the first section, I address the well-known, yet, shallow criticisms of Feyerabend. I do this to make it exceptionally clear that Feyerabend's view is distinct from the caricature he is often accused of defending. Since these criticisms do not address Feyerabend's actual position, as I understand it, they do not lead to any new discoveries about the nature of Feyerabend's pluralism. In the second section, I try to clarify the nature of theory pursuit. I argue that pursuit is distinct from theory acceptance, distinguish between questions pertaining to *how* to pursue theories versus the conditions under which a theory is pursuitworthy, and try to elaborate an account of the *speed* of pursuit. Finally, in this section, I grapple with the 'paradox of pursuit' that Feyerabend leaves us with where a theory must be pursued to be pursuitworthy. In the third section, I try to elaborate a logic of pursuit that is largely compatible with the view espoused in Chapter 2, but requires some modifications. I begin by considering explanatory defenses of realism as a candidate for providing constraints on theory pursuit and find them unsatisfying. I go on to investigate Peirce's 'economics of discovery' to provide a clearer notion of the economic dimensions of proliferation and tenacity. I then draw on Duhem's 'good sense' and Polanyi's 'tacit dimension' of scientific inquiry to provide constraints on the principles of proliferation and tenacity. I conclude by summarizing those elements of Feyerabend's pluralism that require modification or clarification.

1. The 'Illiterate' Criticisms

Feyerabend reprints his responses to the torrid reviews AM faced in a section of *Science and a Free Society* titled "Conversations with Illiterates." While Feyerabend's anarchism received a (largely) negative response,[205] few commentators have come close to articulating Feyerabend's views. Although it cannot be denied that AM, like many other works of philosophy, contains ambiguities, the confusion surrounding Feyerabend's views largely came

[205] The only somewhat positive review I am aware of is by Arne Naess who, while receptive to anarchism, accuses Feyerabend's views as being triviality when compared to hermeneutical philosophy, the Frankfurt school, and much Anglo-American sociology of knowledge (Naess 1975, 185).

from egregiously superficial readings of AM from many prominent philosophers of science. Unfortunately, this strawman has been sufficiently prominent that it has blocked much of contemporary philosophy of science from being able, or willing, to engage with Feyerabend seriously. Because of this, I begin this chapter by addressing these concerns.

1.1: Anarchistic Pluralism and Old-Fashioned Relativism

Some claim that Feyerabend holds a form of relativism where no theory is epistemically superior to any other; all positions, from general relativity to Voodoo, are epistemic equals. On this reading, Feyerabend "does no more than rephrase the sceptic's claim… 'Every proposition is epistemologically on a par with any other'" (Worrall 1978, 279). "If there are no methods and hence no criteria, it follows that all substantive propositions are equally good or bad" (Gellner 1975, 334).[206] This interpretation, in some circles, survives until the present day: "I [am not] ready for the anything goes flavor of much of the post-modernist relativism…" (Wimsatt 2007, 148).[207]

It is exceptionally difficult to discern exactly what this kind of relativism is. What does it mean for theories to be 'equally good' or even 'good' at all? Is being true necessary for being 'good'? It can be frustrating managing this objection since it seems more like name-calling than an actual argument. However, given how prominent this 'objection' is, its power must be dispelled. Perhaps the argument is intended to be something like this:

(1) From some point of view, *x*, Voodoo is true.
(2) From some other point of view, *y*, not-Voodoo (germ theory) is true.
(3) 'Anything goes' means that we can adopt both *x* and *y*.
(4) ∴ 'Anything goes' entails that both Voodoo and not-Voodoo are true.

Another worry is that 'anything goes' is tantamount to abandoning all critical standards in a sort of willy-nilly free-for-all (cf. Ereshefsky 1992, 681).[208] This argument may look like this:

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[206] Similarly, Musgrave understands Feyerabend's thesis as "any theory or research programme is as good as any other" (Musgrave 1978, 192) or, conversely, as Kukla puts it: "all methodologies are equally bad" (Kukla 1977, 279). Adding these together, we get Laudan claiming that: "Feyerabend claims to have shown that every method is as good (and thus as bad) as every other" (Laudan 1987, 19).
[207] Sandra Mitchell also implies this when she writes "This is not to recommend an "anything goes" pluralism. Not all explanations are equally good" (Mitchell 2003, 189). While Feyerabend is not directly cited here, the use of 'anything goes' is clearly meant to indict *Feyerabend's* view. Even Lakatos, at times, hints that this is his interpretation of Feyerabend (cf. Lakatos 1978c, 107-8).
[208] Or, as Worrall (1991, 331) described anarchism: "skepticism plus a license for intellectual tomfoolery."

(1) If ‘anything goes’, then there are no critical standards to hold research to.
(2) Anything goes.
(3) ∴ There are no critical standards to hold research to.

While Feyerabend’s later works articulate and defend a form of relativism,[209] his mature position easily avoids both of these concerns. Feyerabend argues that all ‘standards’ (e.g., methodological standards) are limited and that limits are discovered by proliferating, developing, and applying other standards. This does not mean all these standards are ‘*equally* good’; they may be ‘good’ in different ways or different domains, or they may be useful to entertain for heuristic, pedagogical, or moral purposes. Or, we may be pursuing them to discover *if* they are good. As for theories, the notion of ‘truth’ never plays any role in Feyerabend’s view. His realism means we should treat theories *as if they are true*, but this is a far cry from the relativism he is charged with, which posits that multiple theories *are true*. Finally, as we have seen, one of the primary motivations for anarchism is maximizing testability and criticism, which is the exact opposite of lazily abandoning all critical standards.[210] After all, it is rationalism that posits that there are some things that are *beyond* criticism. The assimilation of Feyerabend’s anarchism to relativism, therefore, is more misleading than illuminating.

1.2: The ‘Strawman’ Objection

Another objection is that rationalism, the opponent of anarchism, is merely a strawman. As Nagel claims, “he is attacking what is pretty much a straw man when he argues against a notion of method according to which there are ‘firm, unchanging, and absolutely binding principles for conducting the business of science’” (Nagel 1977, 1133).[211] On this reading, despite the apparent extremism of his views, Feyerabend is proposing a benign criticism of absolutism. There are a few ways to respond to this. First, this claim seems historically false. Many, from Aristotle onwards, have assumed that science progresses according to a unique rule

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[209] Even the relativism defended in Feyerabend’s later career isn’t equivalent to the kind Feyerabend is accused of holding. See Preston (1995) and Kusch (2016) for discussions of Feyerabend’s relativism(s).
[210] Hasok Chang claims, in a Feyerabendian manner, that it is monism that encourages relaxing one’s critical standards whereas pluralism maximizes the amount and efficacy of critical standards: “It’s [reasoned] choices all the way down, unless you ask God or a dictator to come in and just tell us what to do” (Chang 2012, 261).
[211] Similarly, Gellner claims that Feyerabend’s position is ‘frivolous’ (336) and a “mélange of truisms and extravagances” (341) and Newton-Smith claims that “Feyerabend…erroneously assumes that the rationalist is committed to believing in exceptionless algorithmic rules of comparison” (Newton-Smith 1981, 134).

or set of rules that are constitutive of what science 'is.'[212] Granted, Feyerabend often intentionally reads his opponents as providing normative conceptions of methodology, but given his conception of philosophy of science as something that must be active in the development of scientific knowledge, this forced reading is entirely justifiable. Second, the strength of falsificationism and empiricism *depend* on the fact that science operates, or should operate, in a certain way. For Popper, non-falsifiable theories are pseudo-science and should be ignored.[213] For empiricists, we should not pursue theories that conflict with established facts.[214] The ensuing demarcation criteria of what constitutes *good* science have force *because they are exceptionless*. What would the practical efficacy of, say, empiricism be if we are consistently allowed to pursue 'non-empirical' avenues of research? If such pursuit options were allowed, then this would mean that there is something more basic than an empiricist methodology that trumps empirical decisions. Seen in this light, it is clear that Feyerabend is delivering a deep criticism of many mainstream views that science has (or should have) some rational structure. Finally, many of Feyerabend's arguments attempt to show why empiricism, falsificationism, etc. are committed to rationalism. Without showing how these positions can avoid these commitments, the claim that Feyerabend is attacking a strawman is empty.

There is another way of interpreting Feyerabend's criticisms. On this interpretation, Feyerabend is *trying to change the topic*. By showing how problematic these principles would be if they were applied, philosophers must clarify *the conditions of their application*.[215] Rather than continuing to compare the *logical* advantages and faults of a given method (e.g., falsificationism avoids the problem of induction and paradoxes of confirmation, inductivists avoid problems with

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[212] Psillos, for example, resists the "outright denial that there is an area of culture, viz., science, with a special relation to rationality and objectivity and/or a special claim to knowledge of the world" (Psillos 2012, 93). He provides a cursory history of philosophy of science that maintains that there is something unique about science *in general* rather than Feyerabend's view, quoted as an epigraph, that "[t]he one monster called SCIENCE that speaks with a single voice is a paste job constructed by propagandists, reductionists and educators" (Feyerabend 2011, 56). See also Psillos 2016 for further discussion.

[213] If one looks at Popper's description of his favorite pseudosciences (Popper 1962, 44-6), it is clear that there is no way of engaging in rational discourse with the practitioners *or the theories themselves*, since they aren't testable. Thus, if one wants to be rational, we should not engage with pseudoscientific research nor their products.

[214] Feyerabend's strategy was to interpret empiricism as a normative view, tries to deduce its practical consequences, and evaluates those consequences. To repeat an earlier point, it is remarkably challenging to determine what the *intended* practical consequences of any of the variations of logical empiricism are making it difficult to disagree with Popper's accusation of verificationism as mere 'name calling.'

[215] Feyerabend claims that traditional problems of epistemology (i.e., the construction of the "ideal demands of knowledge and knowledge-acquisition" (AM_B, 203)) will eventually transform into *anthropological* investigations about what results these epistemologies would produce or inhibit.

corroboration, conventionalists avoid problems of holism, etc.), methods must compare their *practical* advantages and disadvantages or else they must admit they are chimeras. Consider the following statement on the status of philosophy of science in 1970:

> Much of contemporary philosophy of science and especially those ideas which have now replaced the older epistemologies are castles in the air, unreal dreams which have but the name in common with the activity they try to represent, that they have been erected in a spirit of *conformism* rather than with the intention of influencing the development of science, and that they have lost any chance of making a contribution to our knowledge of the world. (The medieval problem of the number of angels at the point of a pin had some rather interesting ramifications in optics and in psychology. The problem of 'grue' has ramifications only in the theses of those unfortunate students who happen to have an engruesiast for a teacher) (Feyerabend 1970a, 127).

It is clear that Feyerabend wants philosophers of science to be active in the development of science and by becoming mere commentators on science, philosophers have simply 'petrified' science at a certain stage in its development.[216] Indeed, Feyerabend also writes that "I now set myself the task of widening [the abyss between theory and practice] so that the mechanisms which underlie the actual development of knowledge *will stand out and be recognized more easily*" and, similarly, "[t]he aim of this present essay is…to progress by *emphasizing* the contrast between the customary methodologies and certain important episodes in the history of thought" (emphasis added, Feyerabend 1970f, 277). It makes sense, as a tactic, to attempt to demonstrate the futility of rationalism by revealing their triviality from a practical perspective. Philosophical problems, for Feyerabend, "should not be blown up into formalistic tumour which grow incessantly by feeding on their own juices but they should be kept in close contact with the process of science" (Feyerabend 1970a, 137).[217] On this view, Feyerabend is engaging in

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[216] The 'conformism' accusation is an interesting one and is probably more than mere rhetoric. Later, in the later chapters of AM_{B1} and more prominently in *Science in a Free Society*, Feyerabend rails against the privileging of science as a *discipline* over others and insinuates that philosophers of science have aided in this phenomena through a kind of 'double speak' where 'rational reconstructions' sneak in the normative connotations of the words 'rational' and 'irrational' without any normative argument in support of them.

[217] Lakatos makes a similar point which is summarized nicely in his SEP entry:

> So what was wrong with Carnap's enterprise? In an effort to solve his original problem, Carnap had to solve a series of sub-problems. Some were solved, others were not, generating sub-sub-problems of their own. Some of these were solved, others were not, generating sub-sub-sub-problems and sub-sub-sub-sub-problems etc. Since some of these sub-problems (or sub-sub-problems) were solved, the programme appeared to its proponents be busy and progressive. But it was drifting further and further away from achieving its original objectives…Thus Carnap starts off with the exciting problem of showing how scientific theories can be partially confirmed by empirical facts and ends up with technical papers about drawing different coloured balls out of an urn. In Lakatos's opinion this does not constitute intellectual progress (Musgrave & Pidgen 2016).

propaganda rather than argumentation.[218] Therefore, either Feyerabend is *not* targeting a strawman or it is *irrelevant* whether he is targeting a strawman or not.

As Feyerabend writes, "[h]ardly any religion has ever presented itself just as something worth trying. The claim is much stronger: the religion is the truth, everything else is error and those who know it, understand it but still reject it are rotten to the core (or hopeless idiots)" (AM_{B3}, 218). Indeed, it's difficult to think of something more radical than arguing that the history of philosophy of science has been merely discussing a series of proposals.

1.3: The 'No Method' Problem

One objection is that Feyerabend is promoting a complete relinquishment of *all methods*. For example, Peter Godfrey-Smith writes that "Feyerabend claims that because some principle or rule may go wrong, we should completely ignore it…this claim is obviously crazy" (Godfrey-Smith 2003, 114). Similarly, Rom Harré reads Feyerabend as claiming that "there is *no* method in the advancement of science" (Harré 1977, 298). On this reading, Feyerabend is discarding most of the work previously done in philosophy of science.[219] This couldn't be further from the truth. Feyerabend is not claiming there is *no* method that leads to progress. He explicitly denounces this claim. Rather, the principle of proliferation promotes the exact opposite: a *plurality* of potentially incompatible methods:

> A naive anarchist says (a) that both absolute rules and context-dependent rules have their limits and infer (b) that all rules and standards are worthless and should be given up. Most reviewers regard me as a naive anarchist in this sense, overlooking the many passages where I show how certain procedures *aided* scientists in their research. For in my studies of Galileo, of Brownian motion, of the Presocratics I not only demonstrate the *failures* of familiar standards, I also try to show what not so familiar procedures did actually *succeed*. Thus while I agree with (a) I do not agree with (b). I argue that all rules have their limits and that there is no comprehensive 'rationality', I do not argue that we should proceed without rules and standards (AM_B, 242).

In other words, empiricism *begins* as a potential contribution towards science and *becomes* a highly specialized and detached discipline through intellectual inertia. See Feyerabend (1975b) for a more general discussion of this.

218 The problem of how to apply epistemologies of science was barely discussed by logical empiricists while it was a central question for Popperians. However, except for Lakatos, this question is answered without a "detailed study of primary sources in the history of science" (Feyerabend 1970a, 137). As such, it is perfectly legitimate to demand greater attention to these questions.

219 Laudan also interprets Feyerabend in this manner: "Neither Feyerabend nor anyone else has shown that all the *extant* rules of scientific methodology are inadequate, let alone that all possible rules are discredited" (Laudan 1996, 105).

Many methods have been successful. Rationalists endorse some and not others. Science will, and has, benefitted from those who have designed crucial experiments and looked for severe refutations, those who carefully qualified experimental results to constrain generalizations,[220] and those who salvaged theories against apparent recalcitrant evidence. However, each of these methods must be interpreted *anarchistically*;[221] they must be *tolerant* and even *complementary* of each other, even of methods that are their complete opposite (counter-methods). As Feyerabend writes, "the *one thing* [the anarchist] opposes positively…are universal standards though he does not deny that it is often good policy to act as if such laws existed" (emphasis added, AM_{B4} 189).

2. The Logic of Pursuit

Thus far, I have construed Feyerabend's pluralism as an argument about how theories should be pursued. Specifically, the context of theory pursuit, for Feyerabend, is the process of tenaciously developing multiple competing theories. However, it isn't clear what this entails, exactly, since it is not clear what 'theory pursuit' involves or how it should be understood. While there has been little literature that tries to explicate a logic of how we should pursue theories, there is a sense in which many philosophers of science have touched on this topic indirectly. Before detailing my proposed modifications of Feyerabend's account of pursuit, I first want to clarify the nature of the logic of pursuit. Indeed, many authors have emphasized the theoretical and practical necessity of a logic of pursuit. Achinstein, for example, writes that:

> Sometimes a scientist presents a new theory without attempting to argue that it is true or even probable. Indeed, no tests of the theory may be reported or made…Instead the aim is to show that theory is *reasonable to pursue*; i.e., that it is reasonable to try to work out consequences of the theory, apply it to more complex systems add or reformulate assumptions, devise possible ways to test it, and so forth. Doing this is important because when a theory is first proposed to the scientific community needs to know whether research on it should be encouraged, and whether tests should be planned and conducted. Funding agencies with limited resources need to know whether proposals to investigate the theory should be supported. And scientists need to know whether it is reasonable to invest their own time and energy in its pursuit (Achinstein 1993, 90).

Despite this plea, the characterization of what a 'logic of pursuit' consists in is largely ambiguous and a task that is necessary for the elaboration of Feyerabend's pluralism. In this

[220] See Chang's (chapter 2, 2004) depiction of Regnault for an example.
[221] This is Margolis's (1991) interpretation of 'anything goes.'

section, I unpack the concept of the 'logic of pursuit' and how it relates to Feyerabend's pluralism.

2.1: Pursuit and Acceptance

Theory pursuit is a neglected topic in philosophy of science, with a few notable exceptions. In this section, I will begin by rehearsing what has previously been argued; namely, that theory *pursuit* is independent from theory *acceptance*.[222] What I will add to the discussion is how some of Feyerabend's insights can be used to elucidate the distinction between acceptance and pursuit. While Feyerabend did not entirely neglect acceptance as a topic for discussion,[223] there does appear to be some consequences for acceptance given his view of theory pursuit.

Philosophers have spent millennia arguing what the conditions of theory acceptance are. For empiricists, we accept theories that are highly confirmed or probably true;[224] for constructive empiricists, we accept theories that are empirically adequate; for Popper, we accept self-consistent unfalsified conjectures, and so on. To accept a theory is to take a particular stance towards its truth-value or relationship to the world. Additionally, acceptance has its own implications for practical norms. For example, if we think that we should believe what's true, or ground science policy in sets of truths, then our conception of acceptance will dictate what must be the case to have particular beliefs or support particular policies. Pursuit, on the other hand, is the conditions under which we decide to *work on* a theory. Of course, 'working on' a theory has many different aspects; funding a theory, thinking about a theory, publishing on a theory, testing the theory, promoting the theory, and so forth. What all these aspects have in common is the goal of further developing a theory. As Laurie Whitt has pointed out, acceptance and pursuit have distinct epistemic and practical implications:

> Minimally, when scientists decide to pursue a theory they are deciding to work on it…But [this is not] true of theory acceptance. A scientists' decision to accept a theory does not entail a decision to work on it, although the latter decision may also be made. Nor is it the case that a scientist who decides to work on a theory has also thereby decided to accept it…Strong pragmatic commitments follow from [acceptance]: one must at least try to live by the moral code one finds worthy of acceptance. But one's research

[222] As I have argued elsewhere, Feyerabend often conflated these stances in his criticisms of Lakatos and Kuhn (cf. Barseghyan and Shaw 2017).
[223] In Feyerabend's later career, he argues that what we accept is a consequence of the kind of life we want to lead (what he calls 'Aristotle's principle' (cf. Feyerabend 1999)).
[224] Though, as Ben-Hillel pointed out, this task is filled with ambiguities (Bar-Hillel 1968, 150-1).

> life need not be lived in accordance with the scientific theory one regards as most worthy of acceptance. The reason is that there may be other promising theories available, theories that deserve to be developed (Whitt 1990, 470-471).[225]

Feyerabend has already provided us with provocative cautionary notes abut equating the two; barely any theory in its initial stages is worthy of acceptance. It may, of course, contingently be the case that the two coincide; scientists may, and often do, pursue theories that they also accept and may think a theory is pursuitworthy because they have a hunch it will be acceptable someday. But this does not necessarily have to be the case. As such, theory acceptance is both logically and practically independent from theory pursuit.

Despite their independence, Feyerabend still thinks that the two stances are related to each other in a few ways. First, and most importantly, pursuit is *prior to* acceptance. We may accidentally accept the correct theory, but we cannot know that we are doing so without pursuing the theory first. In a sense, the question of what theories we pursue is more fundamental that the question of what theories we accept: the answer to the latter will ultimately be dependent on the answer to the former. As Feyerabend writes himself:

> Theories become clear and "reasonable" [i.e., acceptable] only *after* incoherent parts of them have been used for a long time. Such unreasonable, nonsensical, unmethodical foreplay turns out to be an unavoidable precondition of clarity and empirical success (AM$_{P}$, 25).

This point may seem trivial, but is often underappreciated. After all, if we make poor choices of what to pursue, it can hardly be said that what we accept is well grounded. Second, every acceptable theory is always at some stage of pursuit and, potentially, only a reflection of a tentative stage of our knowledge that we must outgrow: "All achievements as transitory" (AM$_{B3}$, 266). Acceptance is always something that happens in the midst of pursuit:

> Whatever we accept we should trust only tentatively, always remembering that we are in possession, at best, of partial truth (or rightness), and that we are bound to make at least some mistake or misjudgment somewhere…secondly, we should trust (even tentatively) our intuition only if it has been arrived at as the result of many attempts to use our imagination; of many mistakes, of many tests, of many doubts, and of *searching criticism* (AM$_{P}$, 79).

This quote is quite rich, as it highlights several important points. While most would agree that we

[225] Curd (1980), Achinstein (1993), and Kao (2016) make similar points. It was first noted by Laudan (1977, 174).

should only believe things 'tentatively', Feyerabend also points out that due to the messiness of pursuit, we should always refrain from being fully committed to any claim. Pursuit, as should be clear throughout this chapter, is constrained in all sorts of contingent ways and this leads to many mistakes. Next, the final passage suggests that we can only be at our *most* confident when we have searched for criticism as effectively as possible; the procedural fact of pluralism, therefore, is a prerequisite for strong forms of acceptance.[226] Finally, and most subtly, Feyerabend has an interesting way of conceiving of the relationship between acceptance and pursuit. Recall the following quote: "I regard each piece of research [as] both as a potential instance of application for a rule *and as a test case of the rule*" (Feyerabend 1977, emphasis added fn. 1 368). Going back to Whitt, acceptance is supposed to lead to distinct practical imperatives: "The only pragmatic implications of theory acceptance are a readiness to defend the theory and willingness, at least, to use it in research" (Whitt 1990, 470-1). By contrast, for Feyerabend, when we *use* a theory, we are *both* accepting it and pursuing it (by testing it). As such, any *application* of a theory leads to a simultaneous state of acceptance and pursuit. So we are both accepting aviation science when allowing it to guide regulatory policies on flying and pursuing it insofar as this application has consequences that act as tests of the theory.

Regardless of the details of Feyerabend's own understanding of the relationship between pursuit and acceptance, we are justified in treating these as different topics and discussing their interconnections separately. For now, I will begin by clarifying what the task of a logic of pursuit consists in.

2.2: The How and What of Pursuit

Another important distinction is that between *what* we pursue and *how* we pursue it. In other words, the distinction between what is pursuitworthy and how we should pursue pursuitworthy theories. Lakatos implicitly made this distinction by arguing that 'heuristic advice' is distinct from scientific methodology. The latter is concerned with the conditions of pursuit (cf. Motterlini 1999, 3-4) whereas the former provide theory dependent rules for further developing a pre-existing research programme by telling us "what paths of research to avoid (negative heuristic), and…what paths to pursue (positive heuristic)" (Lakatos 1970, 47). Of course, others have distinct rules for how we should pursue a theory once we have it. But these rules do not tell

[226] For a more detailed analysis of the different kinds (and degrees) of acceptance, see McKaughan (2007).

us what to pursue in the first place. They only tell us *if* we accept a theory, then such and such follows to develop it. In my terms, Lakatos has distinguished between *how* to pursue a theory (follow the heuristics) and *what* to pursue.

Feyerabend has denied that there are any rules for what theories we should pursue and to what extent. His anarchism, therefore, is limited to questions of what to pursue. He says next to nothing about *how* we should pursue theories. As I mentioned before, Feyerabend's justification for the principle of tenacity is that scientists require the ability to develop theories *to overcome their problems*. This suggests that Feyerabend has a telos of pursuit in mind, 'overcoming problems', though the means of attaining this telos is left up in the air. What is even worse is that, as I mentioned in the previous chapter, Feyerabend's conception of tenacity stifle how we may go about understanding how to tenaciously pursue a theory: we can *always* re-implement the principle of tenacity such that, given any problem of a theory, and avoid the problem indefinitely in each individual instance. There is, therefore, a tension between the justification of tenacity and the means of realizing the goals of tenacity. As such, Feyerabend must have some answer to *how* we are to pursue theories to justify his answer to the question of *what* we pursue.

Since proliferation is (partially) justified for its ability to maximize testability and criticism, it seems straightforward to assume that criticism and tests are actually *taken up* or else testability and criticism would be of no value. But we cannot expect that *all* criticism will be taken up, since Feyerabend also defends the reasonableness of *ignoring* and *accidentally outgrowing* criticism (AM_{B3}, 113). I think the best way to remedy this problem comes from Feyerabend's distinction between the cranks and the respectable researcher. Recall, Feyerabend writes that cranks make "*no attempt* to proceed into new domains and to enlarge our knowledge of extra-terrestrial influences; they simply serve as a reservoir of naïve rules and phrases suited to impress the ignorant" (Feyerabend 1978a, emphasis added 96). The 'no attempt' clause is what is worth unpacking. When Feyerabend discusses the cranks, he is thinking about a *community* of cranks; he is explicitly content with a crank here and there (AM_{B1}, 47). While we may excuse scientists for postponing particular criticisms, it becomes more suspicious if this is *constantly* the case within the community. It is because of this that I propose that the norm to 'attend to criticism'[227] should be construed of as an *imperfect duty* where "determinate limits can be assigned to what should be done" but "the duty has in it a play-room for doing more or less" (Kant 1971, 393). As Kant writes:

> But a[n imperfect duty] is not to be taken as permission to make exceptions to the maxim of actions, but only as a permission to limit one maxim of duty by another (e.g., love of one's neighbor in general by love of one's parents) (390).[228]

This allows scientists to balance the various commitments they have, to their research, their other duties as an academic, citizen, to their family and friends, and so forth. While more could be said about this proposal, we see that when we think of critical norms as imperfect duties, we have shifted the question of tenacity from what *rules* we use to pursue theories to *what kind of people* pursue theories. Scientists are no longer mere executers of methodological rules, but *agents* who exercise *judgement* in balancing their various duties and interpreting how they should be implemented. That is, we have shifted from an epistemology of theory pursuit to a *virtue epistemology*[229] of theory pursuit. While I cannot spell out the details of what such an epistemology would look like, if one is possible, then this provides a solution to Feyerabend's problem of the cranks and his lack of an account of how we should pursue theories.

2.3: The Speed of Pursuit

Thus far, we have discussed theory pursuit as if it is a single indivisible unit. This need not be the case. In this section, I consider whether theory pursuit can be conceived discretely, that is, as a series of *stages*. Indeed, Feyerabend often used the language of 'stages' and research is often invested in differently at different stages. It is worth taking a methodological look at this question.

There are at least two distinct ways of conceiving of the stages of theory pursuit. One is to say that there is a logical priority of different temporal stages of theory pursuit. In other words, the logical progression of how a theory should be developed requires a particular historical

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[227] I say 'attend' rather than 'address' because it may be the case that a criticism may be deemed unsound and, therefore, can be ignored *after* showing that the criticism is unsound.

[228] Similarly, as argued by Hill (1971, 61) imperfect duties derive from more general and *vague* maxims that are "vague enough to allow considerable latitude of certain sorts" which is consistent with Feyerabend's fairly vague use of the term 'criticism.'

[229] There is, in addition to this, a social epistemological problem of organizing cranks within communities and understanding the 'crankish' properties of communities as a whole. This will be tackled, briefly, in the next chapter.

ordering. Take Whewell's 'consilience of inductions' as an illustrative example. *First*, we have a 'happy thought' in which we 'colligate' existing knowledge into a hypothesis. In stage *two,* this hypothesis becomes the nucleus[230] for further articulation of the theory. *Finally*, we *test* the theory which either leads to support of the hypothesis or its abandonment (Whewell 1840/1996).[231] As Lakatos puts it, this 'Whewellian pyramid' is "a cautious way you can go: you are not allowed to build the second floor before the first floor is proven by the facts" (in Motterlini 1999, 43). Feyerabend, I think, has already given us reasons to be suspicious of this kind of model, we often must take steps *backwards* to take steps *forwards*: "trying to develop a new theory, we must first take a *step* back from the evidence" (AM_{B1}, 176). Referring back to Whewell, our development of the theory can modify or totally abandon the 'happy thought', or 'refutations' may be reformulated to become confirming instances, and so on. Combine this with Feyerabend's view that (long-term) projections about the fate of our theories are impossible, and then it seems problematic to make the notion of a research stage *epistemic* since we cannot declare which steps backwards will continue to slide backwards and which ones will ultimately be necessary for progress in the long run. Progress is non-linear. As such, the epistemic development of a theory can only be done in a *reconstruction* and cannot be used to assess decision making during each stage. However, Rescher raises an important point here, one that is implicit in the principle of tenacity:

> It must be stressed that it would be quite unreasonable to expect prognostications about the specific *content* of scientific discoveries. It may be possible in some cases to forecast *that* science will solve such and such a problem, but *how* it will so—in the sense of what the specific nature of the solution is—lies beyond the ken of those who antedate that discovery itself (Rescher 1978b, 3-4) .

Using this distinction, we can construe Feyerabend's arguments against predicting the future of our theories as applying to their *content* rather than the *fact* that progress will be made. This allows us to move from *epistemic* stages of research to *empirical* stages of research without undermining the basic assumptions of tenacity.

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[230] Feyerabend often uses this term for the "a crystallization point for the aggregation of other… views which gradually increased in articulation" ($AM_{B3,}$ 119) which he takes from Mill.
[231] It is unclear whether Whewell held a notion of a 'definite falsification' though he often speaks as if such a notion is coherent.

This leads us to distinct way of conceiving of the stages of theory pursuit: by *empirical* markers. Let's imagine that there are regularities in theory pursuit such as marginal return functions, increases or decreases of critical capacities of scientists as theory pursuit goes on, political changes of power that disrupt tenacity, and so forth. If these regularities are robust enough, we have good reasons to suppose that they are independent of the particular content of the theories that are pursued.[232] Of course, different regularities may pull in different directions; continuation of theory pursuit may increase dogmatism but be economically efficient leaving us with a clash of values. Regardless, this method of making theory pursuit a discrete phenomenon seems to be consistent with Feyerabend's understanding of the principle of tenacity and allows for us to make decisions at various stages of theory pursuit. As we shall see, there are good reasons to think that this hypothetical scenario may be likely true in our world.

2.4: Feyerabend's Paradox of Pursuit

In the previous chapter, I outlined a paradox of theory pursuit that Feyerabend has landed us with and provides no solution for. The paradox can be outlined in the following way:

> **Paradox of Pursuit**: For any criteria of pursuitworthiness, *p*, a theory must be pursued until it reaches *p*. This forces us to posit a prior conception of pursuitworthiness, *p'*, and so on *ad infinitum*.

Philosophers have often been incessantly vague about what a criteria of pursuitworthiness may be; it is often considered sufficient to say that a theory 'shows promise.' Even in 1977, Kuhn mentions that 'fruitfulness' is the least discussed theoretical virtue and then proceeds to avoid discussing it (Kuhn 1977, 322). Within normal science, judgments of pursuitworthiness seems simple enough: we constrain the likelihoods of what may turn out to be true by our background knowledge. But what about cases of *proliferation* which *intentionally conflict with our background knowledge*? How can we decide which instances of proliferation are worth pursuing? There is an inherent paradox lurking in the background here that follows naturally from Feyerabend's conception of theory pursuit. Let's say we construct some criteria of pursuitworthiness '*p*' that rules out some instances of proliferation. How can we ever say that some theory will not meet *p if it is pursued*? We need some other criterion, *p'*, at some 'pre-

[232] Even if these 'markers' were correlated with methodological norms, Feyerabend's argument that we must violate methodological norms still stands. If violations of those norms continue to fail and instances of those norms are often successful, then Feyerabend's conjecture appears to rely more on 'mere logical possibilities', as I will discuss in the next section.

stage' of pursuit to determine if it may achieve *p*. This quickly leads to an infinite regress. As a result, any criteria of pursuitworthiness is bound to fail unless one can devise some starting point of pursuit and thereby stop the regress.

Before addressing this problem, it is worth being clear about what I mean by a 'starting point.' Many hermeneuticists, following Heidegger, recognized that inquiry does not *start* from some genesis point but, rather, we are 'always already' in the midst of pursuit. We can see Duhem mocking this idea of a starting point in the context of scientific inquiry:

> The ordinary layman judges the birth of physical theories as the child the appearance of the chick [from an egg]. He believes that this fairy whom he calls by the name of science has touched with his magic wand the forehead of a man of genius and that the theory immediately appeared alive and complete, like Pallas Athena emerging fully armed from the forehead of Zeus (Duhem 1906, 221).

Feyerabend recognizes this point as well, though he frames it differently:

> The initial playful activity is an essential presupposition of the final act of understanding…Creation of a *thing*, and creation plus full understanding of a *correct idea* of the thing, *very often are parts of one and the same indivisible process* and they cannot be separated without bringing the process to a standstill. The process itself is not guided by a well-defined research program; it cannot be guided by such a program for it contains the conditions of the realization of programs. It is rather guided by a vague urge, by a "passion" (Kierkegaard). The passion gives rise to specific behavior which in turn creates the circumstances and the ideas necessary for analyzing and explaining the whole development, for making it "rational" (AM_P, 24-5).[233]

On this view, it is misleading to think of a 'starting point' on which inquiry on a particular topic *begins*; this can only be done in retrospect.[234] Rather, we play around with ideas that we already have by virtue of being situated in particular tradition and theories *emerge out of* this playfulness. Does this not end the task of seeking a logic of pursuit? Why can it not be the case that we continually play around with ideas with theories being by-products of this playfulness? There may be an element of truth in this, but it does not answer the pertinent point raised by Achinstein earlier: when making decisions (accepting publications, funding research, choosing a theory to entertain, etc.) we need to have some guidance about what direction to go. In the case of proliferation, we have an exceptionally large amount of choices we can make. To admit that no

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[233] A more systematic discussion can be found in his criticism of creativity (Feyerabend 1987b).

[234] Even this seems problematic, as Lakatos points out, for the same reason: "How do we know that the hindsight we have the advantage of is *long enough* hindsight? Toulmin, it seems, ought to hold that 'true rationality' is revealed only in the 'closed down long run', on the day of Final Judgment, when we are all dead" (Lakatos 1976b, 238).

decision can be made on principled grounds is to admit anarchism into the realm of pursuit as well. If a logic of pursuit is possible, we must somehow diffuse the paradox of pursuit.

I want to argue that the paradox can be remedied by providing different contexts in which pursuit begins.[235] While this oversimplifies the process of articulating, developing, and testing a theory, consider two separate 'starting points' for pursuit:

(1) The context of publication
(2) The context of funding[236]

The paradox of pursuit applies to both. Freeman Dyson made this quite clear in the context of publication: "[w]hen the great innovation appears, it will almost certainly be in a muddled, incomplete, and confusing form. To the discoverer himself it will be only half-understood; to everybody else it will be a mystery" (Dyson 1958, 79-80). However, in this context, the *costs* are much lower. Journal space and the opportunity cost of developing new theories are extremely low, meaning that there is little risk in these stages of theory pursuit. As such, we can bite the bullet and accept that the criterion for when a scientist should start *thinking about* or *writing about* a theory or when an editor should *accept* the theory for publication, is largely arbitrary. As Feyerabend says, "there is no *idea*, however ancient and absurd that is not capable of improving our knowledge" (AM_{B1}, emphasis added 47). Not only is it problematic to find criteria of pursuitworthiness for these minute features of proliferation in principle, but we do not have a great deal of practical impetus to resolve this problem anyhow. As a result, the paradox of pursuit is dissolved in (1) by denying that any such *p* exists. We can accept that proliferation of *ideas* is something that happens merely by playing around with pre-existing ideas that arise merely by virtue of being situated in a particular set of intellectual traditions.

The context of funding, which will be discussed at greater length later on, is a different story. Funding and, more broadly, the allocation of resources to theory pursuit is more costly and thus we have to take the problem more seriously. Essentially, we are making a cost-benefit

[235] Curd (1980, 202) limits his logic of pursuit to the "moment when a scientist (or research group) first begins thinking seriously about a problem and ending when the theory…is first written down in a form suitable, say, for publication in a scientific journal" but never uses these practical constraints in any substantive way in his analysis.
[236] These two contexts roughly overlap with the context of proliferation and the context of tenacity respectively. I say 'roughly' because these contexts aren't precisely defined so some activities may well be a part of both (e.g., hosting a specialty conference on a new hypothesis, comparing proliferated theories to old evidence, etc.). I will try not to bicker about the precise boundaries of proliferation and tenacity, but focus on the costs of various activities.

analysis. Low costs and high possible benefits seem like a reasonable judgment to make (though, of course, this is ultimately grounded in values). In some cases of funding, especially large longitudinal grants, the costs are higher. This entails that we must try to understand how to make judgments about the possible benefits of particular decisions. Recall that theory pursuit comes in stages and that pursuitworthiness judgments are possible within normal science. If the *early* stages of pursuit are cheap enough, then we can bite the bullet on the paradox of pursuit and begin making pursuitworthiness judgments with transparent cost-benefit analyses as pursuit develops. Luckily, as will become apparent in the next section, such a scenario may well be within our reach.

3. The Economics of Anarchism

Feyerabend's pluralism has a consequence that is hard to swallow. Namely, it entails that we are unable to rank what research is likely to succeed in what ways. One worry about this view is that Feyerabend relies on 'mere logical possibilities' to support his pluralism. Achinstein, on this point, writes the following:

> There is no denying [that discovering truths with alternatives] is possible. But this is pretty weak fare. If you have a theory, then simply imagining a conflicting theory T′ (where there are no constraints on T′…) does not guarantee or even make it likely that you will discover new evidence to test T…Without more constraints on the conflicting theory T′ – that it have some plausibility or some evidence in its favor – the possibility that it will enable us to unearth new evidence to test T is mere *logical* possibility (Achinstein 2000, 39).[237]

This worry can be put another way: while we may, with infinite resources and time, pursue every imaginable alternative endlessly, we are forced in our finite world to choose particular options over others. This prompts the question of whether we can *rationally allocate resources to scientific projects*. Put another way, does the contingent fact of limited resources force us to abandon 'anything goes'? In this section, I will survey a number of positions that attempt to address this question by providing criteria under which certain projects are *pursuitworthy* and, therefore, can be preferred over differing projects.

3.1: Realism and Optimistic Meta-Inductions

[237] Feyerabend considers the objection that "[t]he exclusion of alternatives is then required for reasons of expediency: their invention not only does not help, but it even hinders progress by absorbing time and manpower that could be devoted to better things" (Feyerabend 1965b, 174). However, he never delivers a satisfactory response.

In my Master's thesis, abbreviated in Shaw (2018), I argued that the debate between realism and anti-realism is substantive, *pace* some commentators, since they have differing implications for theory pursuit. I now believe that this position is mistaken for the reasons given in this section. In this section, I show how the failure of the realism/anti-realism debate to provide criteria for theory pursuit actually indicates how a larger class of meta-inductive strategies to provide criteria for theory pursuit will fail.

Since Putnam (1975) and Boyd (1983), many have defended realism as an inference to the best explanation of the 'success of science.' For scientific theories to be as successful as they are, they must approximately reflect real aspects of nature. The most formidable counterargument to realism has been the pessimistic meta-induction (Laudan 1981), which entails that many empirically successful theories turned out to be false (i.e., incompatible with our best current theories). More specifically, Laudan conjectures that for "every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring" (35). Realists have countered in a number of ways: by qualifying their theses to be about specific *features* of theories (e.g., structure (Worrall 1989), presuppositional posits (Kitcher 1993), etc.), specific *theories* (Psillos 1999), or diluting the notion of 'approximate truth' to allow for substantive theory change (Hardin & Rosenberg 1982; Leplin 1997).[238] We now have a range of positions that provide differing inductive bases to infer what theories or features of theories will be retained in the future. A natural consequence of these views is that they make certain demands about theory pursuit: some theories in particular domains must recover those features of theories before them that were responsible for their empirical success. Notice that the structure of this argument is quite broad and appeals to historical *induction* that claim that past successes can lend support to the probability of future successes.[239] As such, it isn't just *realism*, but any abductive-cum-historical position that advances criteria for pursuitworthiness. Despite the varying limitations these positions place on theory pursuit, the

[238] The proceeding arguments do not affect this final 'thin' version of realism. As Stanford (2003, 566) has argued, these watered down views are so diluted that they have given up their right to make normative claims about pursuit.
[239] Zollman (2010, 23-4) provides two distinct justifications for meta-inductions that I will not discuss here.

fact that they provide *any* constraints places them in opposition to Feyerabend's view. It is worth considering them here.[240]

Regardless of the differences among the realist options available, to provide any normative account of pursuit, they must contain the following common core assumptions:

(1) The history of science, or some subdomain of science, shows continuity (of some kind).
(2) This history provides an inductive base from which we can make probative projections about future continuities.[241]
(3) Such projections provide criteria for particular instances of theory pursuit.

Furthermore, every realist is committed to some kind of rationalism according to which 'evidence' (broadly construed) can force us to abandon our theories and are therefore committed to a fallibillistic view of science. These projective inferences, therefore, contain a hidden *ceteris paribus* qualification: we should retain theories continuous with previous theories in a particular domain assuming we have no novel reasons to abandon the theory. In other words, realist inferences only hold in absence of methodological considerations.

Recall that, on a Feyerabendian analysis, theoretical continuity on its own is insufficient for projective inferences. *Sociological* features of historical practices must supplement historical investigations as well. The principle of tenacity assumes that, as Lakatos so eloquently put it, "[a] brilliant school of scholars (backed by a rich society to finance a few well-planned tests) might succeed in pushing any fantastic programme ahead, or, alternatively, if so inclined, in overthrowing any arbitrarily chosen pillar of 'established knowledge'" (Lakatos 1970, 100).[242] As such, a lack of funding for alternatives, conservative peer-review and citation practices,

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[240] For this section I will grant that inference to the best explanations are not viciously circular (see Psillos (chapter 10, 1999) for a defense of abductive reasoning). I also will not consider the objections that realists commit the 'base rate fallacy' (Magnus and Callender 2004) or the 'turnover fallacy' (Lange 2002). I think that both of these objections have been answered definitively by Saatsi (2005).
[241] Stanford (2006, 169-183) argues that only *retrospective* criteria are available for distinguishing those parts of theories we expect to be retained and those parts that will be abandoned making realism impossible to apply prospectively. He also argues against Psillos' (1999) strategy to use the judgments of scientists *at the time* to ground realist projections primarily by giving historical counterexamples of scientists' misjudgments of the virtues of their own theories.
[242] "If two teams, pursuing rival research programmes compete, the one with the most creative talent is more likely to succeed…the direction of science is determined primarily by human creative imagination and not by the universe of facts, that surrounds us" (Lakatos 1970, 158). Feyerabend expressed the same view (cf. Motterlini 1999, 147). This is the same view as expression by Longino (1990, Chapter 4) though she is more detailed as to what those sociological features should be.

increased professionalization and local incentive strategies, proselytizing methods of scientific pedagogy, and so forth must be taken into account for continuity to be epistemically meaningful. Of course there is theoretical continuity when theoretical discontinuity *isn't allowed*! Unless theoretical continuities are bombarded by tests from alternatives, criticized relentlessly by diverse audiences, and have their results compared to well-funded alternatives, there is no reason for the realist to take solace in historical continuities since these continuities may be mere artifacts of conservative approaches to organizing science. If realist projections are used to constrain proliferation, they have merely stacked the deck in their favor. They have eliminated what could, by their own methodological lights, have disrupted their favorite theoretical continuity. As Feyerabend writes:

> There is always the possibility that new forms of thought will arrange matters in a different way and will lead to a transformation even of the most immediate impressions we received from the world. Considering this possibility, we may say that the long-lasting success of our categories and the omnipresence of a certain point of view is not a sign of excellence or an indication that the truth or part of the truth has at last been found. It is, rather, the indication of a *failure of reason* to find suitable alternatives which might be used to transcend an accidental intermediate stage of our knowledge. This remark leads to an entirely new attitude towards success and stability (Feyerabend 1981d, 72).[243]

Even if realists could soundly provide reliable projective inferences about what parts of theories will be retained in the future, it would still have no implications for theory pursuit.

Even though the abductive route realists have taken is foreign to the arguments Feyerabend addressed during his career, it fares no better in constraining theory pursuit than traditional understandings of methodology. We can now provide principled reasons to justify Hasok Chang's frustrations: "Debates about realism can be frustrating. It is easy to get a sense that one does not know what all the fuss is about" (Chang 2001, 5). Regardless, we have learned that we cannot use historical inductions to justify what *content* theories must have and thereby place constraints on proliferation. However, maybe this isn't the end of the story. Realists

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[243] Earlier in his career, Feyerabend suggests that the pessimistic meta-induction similarly does not have methodological consequences:

> Most theories, world-views of past physics turned up to be illusions. Who ensures us that we are not falling victim to an illusion again in in the present? Answer: No one can give us such a warrant. But does it follow from this that we should formulate our theories in a more cautious manner from now on…? Such a course of action would make it impossible to carry out the scientist's most important task: to correct scientific theories (Feyerabend 1954a, 474-5).

believe that successful theories are successful because they have latched on to some aspect of reality. Is the converse true as well? Are theories that are unsuccessful such because they simply are not saying anything true about the world? Even Feyerabend himself suggests this line of reasoning in his later career:

> [N]ot all approaches to "reality" are successful. Like unfit mutations, some approaches linger for a while—their agents suffer, many die—and then disappear. Thus the mere existence of a society with certain ways of behaving and certain criteria of judging what has been achieved is not sufficient for establishing a manifest reality; what is also needed is that God, or Being, or Basic Reality reacts in a positive way (Feyerabend 1999, 215).

If this is true, then this restrains the principle of tenacity. The problem with this is that it appears to simply fall back into the problem of the 'time limit' of tenacity; how long must we stick with a degenerating theory until we can determine that it is degenerating because of its asymmetry with the world? While the balance between proliferation and tenacity may not come from realism, there are other options that seem more fruitful.

3.2: The Economics of Pursuit

Hanson famously tried to restore the plausibility of providing a logic of pursuit[244] that was inspired by Peirce. Rather than discovery being some unanalyzable act(s) of genius, scientists 'typically' employ abductive reasoning. Using the case study of Kepler's discovery of planetary elliptical orbits,[245] Hanson writes:

> It required no genius to take Kepler's idea and try it for other planets. Kepler never modified a projected explanation capriciously; he always had a sound reason for every modification he made…This is the greatest piece of [abductive] reasoning ever performed (Hanson 1958, 85).

Kepler merely needed to 'observe' the ellipses inherent in Tycho's data. Schematically, the abductive process of discovery looks like this:

(1) Some surprising, astonishing phenomena p_1, p_2, p_3…are encountered.
(2) But p_1, p_2, p_3... would not be surprising were an hypothesis of H's type to obtain…
(3) Therefore, there is good reason for elaborating a hypothesis of type H (Schickore 2014).

[244] Hanson, and others, refer to the logic of *discovery* whereas I will use the term 'pursuit' for consistency sake.
[245] For a historical criticism of this reconstruction, see Lugg (1985).

I will not detail Hanson's account, as I think its defects have been pointed out (see Plutynski 2011). Rather I will consider Hanson's source, Peirce's 'economics of discovery', as a unique alternative to Feyerabend's view.

Peirce's account of abduction has spawned a massive secondary literature. Three primary interpretations have ensued. First, the 'generative interpretation' where abduction provides a means for *creating* hypothesis (Nickles 1985). Second, the 'justificatory interpretation' where abduction serves as a means to infer general fallible knowledge claims (Misak 2000). The third interpretation, which I consider here, is the 'pursuitworthiness interpretation' where abduction provides the means to rank hypotheses according their potential fruitfulness (Rescher 1978a; Achinstein 1993; McKaughan 2008).[246]

Peirce's view of abduction has (almost) the same formal structure as Hanson's:

(1) The surprising fact, C, is observed;
(2) But if A were true, C would be a matter of course,
(3) Hence, there is reason to suspect that A is true (CP 5.189).

However, the wording of the conclusion can be misleading. On the pursuitworthiness reading, "[a]bductive reasoning makes practically grounded comparative recommendations about which available hypotheses are to be tested" (McKaughan 2008, 452). Here, A has no probative force at all. Rather,

> Not only is there no definite probability [for A], but no definite probability attaches even to the mode of inference. We can only say that the Economy of Research prescribes that we should at a given stage in our inquiry try a given hypothesis, and we are to hold it provisionally as long as the fact will permit. There is no probability about it. It is a mere suggestion which we tentatively adopt (Peirce 1976, 184).

In other words, the conclusions are conjectures. What's unique about the 'Economy of Research' is that it provides *purely practical*, as opposed to 'rational', criteria for which hypotheses to test.

> [T]here is only a relative preference between different abductions; on the ground of such preference must by economical. That is to say, the better abduction is the one which is likely to lead to the truth with the lesser expenditure of time, vitality, etc. (37-8)

[246] I will not take a stance on which interpretation reflects Peirce's 'real' views here. See Psillos (2011) for discussion.

As Rescher (1978a) points out, this means that purported views of rationality can be described in terms of cost-benefit analyses.[247] "The whole service of logic to science…is the nature of economy" (CP fn. 18 7:220). Such analyses are practically indispensable. Peirce writes:

> Proposals for hypotheses inundate us in an overwhelming flood, while the process of verification to which each one must be subjected before it can count as at all an item, even of likely knowledge, is so very costly in time, energy, and money—and consequently in ideas which might have been had for that time, energy, and money, that Economy would override either other consideration even if there were any other serious considerations. In fact there are no others (5.602).
>
> If he examines all the foolish theories he might imagine, he never will (short of a miracle) light upon the true one (2.776).

How can methodological considerations aid in the economically efficient pursuit of hypotheses? The question is exceedingly easy to answer for a Kuhnian-type conservative: we can establish "the limits of plausibility by indicating that the currently accepted scientific theories and principles of greater scope act as standards which guide scientific research" (Brown 1983, 406). As such, we can formulate a projected cost-benefit analysis during normal science. The difficulty arises when deciding which *alternative* hypotheses to consider. We can therefore make cost-benefit judgments during tenacity, but it is still unclear how to make these judges in cases of proliferation.

At some points, Peirce appeals to a notion of simplicity: if a hypothesis is "more verifiable, that is to say, would predict more, and could be put more thoroughly to the test" (CP 5.598). "The more general the thesis, the larger will be the "epistemic bang for the buck," other things being equal" (Rescher 1978a, 81). Poincaré takes a similar route:

> We all know that there are good experiments and poor ones. The latter accumulate in vain; whether there are a hundred or a thousand, a single piece of work by a real master, a Pasteur for instance, suffices to make them fall into obscurity….What then is a good experiment? It is one which teaches us something more than an isolated fact; it aids us to predict, and enables us to generalise…[Hence,] it is necessary that each experiment should allow the greatest possible number of predictions…The problem is, so to speak, to increase the efficiency of the scientific machine (Poincaré 1902, 517-8).

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[247] Keep in mind that the notion of a 'cost-benefit' analysis presupposes value judgments about what constitutes a 'cost' or a 'benefit' or what follows, practically speaking, from the results of a cost-benefit analysis.

While Poincaré is using deduction[248] and not abduction to derive his view of pursuit, the more salient point is the same: we can use a priori methods to determine what character hypotheses should, economically speaking, have.[249] The problem here, though, is comes when proliferation is combined with tenacity. As discussed in the previous chapters, the *significance* of theories change as pursuit goes on and, therefore, cannot be simply 'read off' the syntactic features of the hypotheses. This route, therefore, does not land us with a limitation of proliferation.

At one point, Peirce does discuss the dynamics of evolving theories or, in Feyerabendian jargon, the dynamics of tenacity:

> We thus see that when an investigation is commenced, after the initial expenses are once paid, at little cost we improve our knowledge, and improvement then is especially valuable; but as the investigation goes on, additions to our knowledge cost more and more, and, at the same time, are of less and less worth. Thus, when chemistry sprang into being, Dr. Wollaston, with a few test tubes and phials on a tea-tray, was able to make new discoveries of the greatest moment. In our day, a thousand chemists, with the most elaborate appliances, are not able to reach results which are comparable in interest with those early ones. All the sciences exhibit the same phenomenon, and so does the course of life. At first we learn very easily, and the interest of experience is very great; but it becomes harder and harder, and less and less worth while, until we are glad to sleep in death (Peirce 1967, 644).

In modern economic terms, Peirce asserts that theories have built-in marginal utility functions where utility ('improved knowledge') gradually descends and expenses rise. Notice how Feyerabend's argument that the value of a theory is something that can only be determined in retrospect is partially muted here since 'minor' discoveries within Peircian normal science have their value *derived from* the basic commitments of the research programs. As such, the value of the research program *as a whole* may be left up in the air for future generations to determine, but the value of incremental gains within that program are not subject to the same worry (to the same degree). Their value can be recognized much more quickly. Therefore, we can pursue a theory and make decisions at each step in a funding cycle about whether to pursue it further. As discussed before, this provides a means of demarcation of the *stages* of theory pursuit.

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[248] "It is often said that we must experiment with no preconceived idea. That is not possible" (Poincaré 1902, 518).

[249] Radnitzky (1987) makes a similar point in a Popperian way: "If a basic statement *b* corroborates the theory T… then the cost of repeating the experiment as well as the expected marginal utility of doing so are likely to be small or negligible. If *b* contradicts T…then investing time and effort into re-checking *b* holds more promise of an intellectual gain" (170).

What is remarkably impressive about Peirce's insight here is how it has received a great deal of support from what little literature in economics attempts to construct general theories to determine the marginal utility curve of scientific theories.[250] There are a few general trends, with various degrees of cross-disciplinary robustness, which are worth noting here:

(1) The operational costs of theory pursuit increase exponentially over time.
(2) Chances of 'internal revolutions' decrease linearly over time.
(3) Research duplications tend to increase over time.
(4) Publishing profiles can be represented by Gaussian functions and thereby, decrease past a certain threshold.[251]

For (1), most theories that have clearly testable dimensions have a well-defined start-off cost; what technologies are needed for testing and how much those technologies cost. However, as research progresses, new technologies are needed and more refined versions of existing technologies must be created. This is expensive and, therefore, the operational costs tend to increase with time. For example, V.S. Ramachandran's early experiments on phantom limbs were done with cardboard boxes and ordinary mirrors that have now become refined in virtual reality experiments. Scherer (1966) suggests that this trend is fairly robust and is also hinted at in Peirce's example of Wollaston. (2) is largely a function of sociological investigations into increasing conformity over time, something Feyerabend was well aware of (cf. Feyerabend 1965b, 177),[252] and the fact that there is a greater demand to be up to date with exponentially increasing amounts of studies which can be overwhelming for revolutionary experiments.[253] (3) derives from the increasing inability of scientists to coordinate themselves, due to the increasing size and complexity of research programs, such that research teams often accidentally overlap.[254] (4) is supported by studying the life-cycles of research productivity of individual scientists.[255]

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[250] Rescher (1978b) found the same results.
[251] Before detailing these observations, note that there are also serious limitations with these models such as the inability to predict future scientific labor markets (Leslie and Oaxaca (1993)). Additionally, most estimated cost models also presuppose that technological costs will decrease linearly making substantive assumptions about progress in technology that aren't well studied empirically.
[252] This is also suggested by the great deal of textual evidence provided by Rescher (1978b, Chapter 2) that as theories become more developed, there is a greater tendency to perceive that theory as 'complete' and, therefore, no revolution is forthcoming.
[253] The rise of 'big science' has also been considered an indirect cause of increased conformity since it presupposes fundamental theoretical and methodological assumptions rather than challenges them (see Stanford 2015).
[254] It is unclear, to me at least, whether 'duplications' are the same as 'replications.' If they are, fields undergoing a 'replication crisis' may consider this to be a moot point. An added difficulty comes from the fact that these 'islands of research' tend to develop their own idiosyncratic jargon making comparative assessments more difficult as time moves on (cf. Bigo and Negru 2008).
[255] See Stephan (1994, 1216-7) and the citations therein.

Prestigious researchers, whose work tends to have higher impacts and are more capable of being 'revolutionary', tend to publish less frequently towards the ends of their career.

Let's say that this theory of theory pursuit is correct and there exists marginal utility models that we can construct in advance of pursuing a theory. This provides us with a few crucial notions that Feyerabend's philosophy lacks. First, we can begin to make sense of the notion of *risk* without resorting to attempting to predict the future content of a theory. All instances of proliferation begin as equally risky in the sense that we do not know how they will turn out in the long term. Research within established theories is less risky, not because of realist intuitions, but because of extant institutional forces that aid tenacity. This is a feature that Feyerabend's methodology must have to make comparative judgments about pursuitworthiness and one he explicitly shucks off:

> By speaking of risks it assumes that the progress initiated by progressive phases will be greater than the progress that follows a degenerating phase; after all, it is quite possible that progress is always followed by long-lasting degeneration, while a short degeneration (say, 50 or 100 years) precedes overwhelming and long-lasting progress (Feyerabend 1976, 215).

This, Feyerabend points out, is "a version of Hume's problem" (fn. 25 215); but this makes it impossible to provide a view of risk. Second, the principle of tenacity is now equipped with a notion of *prospective success*. Feyerabend only argues that theories inconsistent with extant theories are likely to lead to progress, giving a higher degree of prospective success to novel theories.[256] We can reasonably expect research within a research programme to provide minor successes *before* the marginal utility function decreases or extends asymptotically, whereas research *after* this point will be less likely to be successful but, if it is, will have a greater impact. Because of this, tenacity after the point of decreasing marginal returns *is slowed down*, rather than terminated altogether. This kind of tenacity seems more like an internal revolution rather than marginal gains on existing knowledge.

[256] "If they *contradict* a well-confirmed point of view, then this indicates their usefulness as an alternative. Alternatives are needed for the purpose of criticism. Hence metaphysical systems that contradict observational results or well-confirmed theories are *most welcome* starting points of such criticism. Far from being misfired attempts at anticipating, or circumventing, empirical research that have been deservedly exposed by reference to experiment, they are the only means we possess for examining the assumptions implicit in our observational results" (Feyerabend 1965b, 183).

The empirical literature on each of these points is quite messy and makes use of many inconsistent idealizing assumptions and some fine-grained questions have yet to be researched. As such, this model is not empirically reliable enough to make concrete policy decisions. Additionally, the introduction of cost-benefit analyses into tenacity does not, on its own, dictate anything; this requires value judgments as well. However, I think it provides us with an interesting avenue, if true, for how the principle of tenacity may be adjusted.

3.3: Good Sense and the Tacit Dimension

Thus far, this dissertation has focused on *explicit* rules of methodology. However, there are also *implicit* rules of methodology that have implications for Feyerabend's views. More broadly, this is the *tacit* approach to scientific methodology. In this section, I will consider the approaches of Duhem and Polanyi and recover what constraints they place on theory pursuit that are reasonable given what we have learned in the previous two chapters.

Duhem famously describes good sense as "reasons which reason does not know" which are "vague and uncertain" and "do not fall under the principle of contradiction" (Duhem 1906, 217). They are implicit and arise from the knowledge scientists have of their trade and its history.[257] Good sense is necessary for the cessation of pursuing a hypothesis or, in other words, the termination of tenacity.[258] Furthermore, good sense "do[es] not reveal [itself] at the same time and with the same degree of clarity to all minds. Hence, the possibility of lengthy quarrels between the adherents of an old system and the partisans of a new doctrine, each camp claiming to have good sense on its side, each party finding the reasons of the adversary inadequate" (ibid). So, at some periods of history, good sense is *pluralistic*.[259] However,

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[257] See chapter VII, secs. 3 and 4 for Duhem on the historical education of scientists and the 'evolutionary growth' of hypotheses.

[258] Much of the growing literature on good sense wrongfully argues that good sense plays a crucial role in theory *choice* (Stump 2007; Ivanova 2010). Duhem is explicit that good sense is "the judgement of hypotheses which *ought to be abandoned*" (emphasis added, Duhem 1906, 216). Chapter VII is devoted to the choice of hypotheses where Duhem provides three requirements for theory choice (cannot be self-contradictory, cannot contradict other established theories, and the "totality of experimental laws" can be derived from them "to a sufficient degree of approximation" (220)). He then writes: "So long as [the physicist] respects [these constraints], the theorist enjoys complete freedom, and he may lay the foundations of the system he is going to construct in any way he pleases" (ibid).

[259] This is in contrast to Descartes' view, where "Good sense is the best distributed thing in the world... In this it is unlikely that everyone is mistaken, it indicates rather that the power of judging well and of distinguishing the truth from the false - which is what we properly call 'good sense' or 'reason' – is naturally equal in all men, and consequently that the diversity of our opinions does not arise because some of us are more reasonable than others

> this state of indecision does not last forever. The day arrives when good sense comes out so clearly in favor of one of the two sides that the other side gives up the struggle even though pure logic would not forbid its continuation… for Foucault's experiment was *not* the crucial experiment that Arago thought he saw in it, but by resisting wave optics for a longer time Biot would have been lacking in good sense (218).

Good sense converges in the long run and at *this* stage that tenacity must be slowed down.[260] Notice that good sense is a feature of a *community* at a particular *stage* of research. For good sense to provide criteria for tenacity termination, it must have two features: it reflects a *consensus* and *specific enough to determine particular actions*. Good sense is useless unless it can be translated into workable practical imperatives.[261]

Another similar argument comes from Polanyi. While the two thinkers approach to methodology are certainly not compatible, there is a sense in which Polanyi further elaborates on Duhem's conception of good sense. Here, I will mention three ways in which Polanyi elaborates upon Duhem's insight:

1) The *generalization* of good sense.
2) How to *recognize* when good sense is operative.
3) The *appraisal* of tacit knowledge.

For 1), Polanyi argues that tacit knowledge is omnipresent in intellectual activities. Tacit knowledge is similar to good sense in its *origin* (from repeated practice within a community) and its ineffable character. However, tacit knowledge is much broader; it applies to theory *appraisal*, which involves every kind of assessment of a theory (i.e., interpretative, critical, acceptance, its pursuitworthiness, etc.) This is distinct from Duhem's conception of good sense which is *only* operative where the 'logic of science' is silent. For 2), Polanyi asserts that "what is being criticized is, every time, *the assertion of an articulate form*" (Polanyi 1958, 264). In other words, propositions. However, what is critic*al* is "our personal acceptance of an articulate form" or "the mind granting this acceptance which is said to be acting critically" (ibid). Consider Polanyi's brief case study of D.C. Miller's experimental 'disproof' of special relativity. He writes "[t]he

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but sole because we direct our thoughts along different paths and do not attend to the same things" (Descartes 1637/1988, 20). For Descartes, good sense is *natural* and, therefore, universal amongst humans, whereas, for Duhem, it is *learned*. It would be hard to imagine how relatively sophisticated debates in science can be reduced down to our 'naturally endowed' ability to reason, even assuming such a thing exists.

[260] It is unclear whether good sense, in such instances, provide *definitive* ends to theory pursuit or adjusts its *speed*.

[261] Scientists may have the same good sense but differ as to what the appropriate practical response is. This would likely be an important qualification, since good sense as an inherently vague intuition would not be accompanied by detailed practical guidance.

experience of D. C. Miller demonstrates quite plainly the hollowness of the assertion that science is simply based on experiments which anybody can repeat at will. It shows that any critical verification of a scientific statement requires the same [tacit] powers for recognizing rationality in nature as does the process of scientific discovery" (13). Polanyi rejects the 'textbook' narrative that the Michelson-Morley experiment (of 1887) was a crucial experiment against the ether:

> [The 1887 Michelson-Morley experiment] admittedly substantiated its authors' claim that the relative motion of the earth and the 'ether' did not exceed a quarter of the earth's orbital velocity. But the actually observed effect was not negligible. Or has, at any rate, not been proved negligible up to this day. The presence of a positive effect in the observations of Michelson and Morley was pointed our first by W. M. Hicks in 1902 and was later evaluated by D.C. Miller and his collaborators in a long series of experiments extending from 1902 to 1926, in which they repeated the Michelson-Morley experiment with new, more accurate apparatus, many thousands of times (12).

One might have expected that when these results were presented at the American Physical Society on December 29th, 1925, there would be either an "instant abandonment of relativity" or the community would "suspend judgment in this matter until Miller's results could be accounted for" (13). However, "[l]ittle attention was paid to the experiments, the evidence being set aside in the hope that it would one day turn out to be wrong" (ibid).[262] Polanyi thinks that it is tacit knowledge that guided this disavowal, knowledge that could not be put into cogent, explicit arguments.[263] Why appraise this move as an acceptable feature of scientific practice? The success of relativity over ether theories cannot be invoked, since this came decades after the fact. On this, Polanyi writes:

> the forces contributing to the growth and dissemination of science operate in three states. The individual scientists take the initiative in choosing their problems and conducting their investigations; the body of scientists controls each of its members by imposing the standards of science, and finally, the people decide in a public discussions whether or not to accept science as a true explanation of nature…*any attempt to direct these actions from*

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[262] Polanyi cites C.G. Darwin's remarks that "We cannot see any reason to think that this work would be inferior to Michelson's work…What happened? Nobody doubted relativity. There must therefore be some unknown source of error which had upset Miller's work" (quoted in Polanyi 1958, fn. 1 13). Feyerabend argues that the mistake with Miller's experiments was not found until 1955 (AM$_P$, fn. 110 116). See also Lakatos (1970, fn. 6 76-7) and Feyerabend (1972, fn. 34 148) for further discussion.

[263] Polanyi comes to the same conclusion in his analysis of Mach's rejection of Newton (11-2) and Rayleigh's experiments demonstrating that hydrogen atoms striking a metal wire could transmit up to 100eV (Polanyi 1962, 58).

> *outside must inevitably distort or destroy their proper meaning* (emphasis added, Polanyi 1951, 58).

The choice of problems and their pursuit "bring into play intellectual powers which are otherwise hidden and assert creative forces of a unique kind" (Polanyi 1946, 6). "Discovery", Polanyi writes, "only comes to a mind immersed in its pursuit. For such work the scientist needs a secluded place among like-minded colleagues who keenly share his aims and sharply control his performances" (Polanyi 1962, 67). Even more boldly, Polanyi claims that some 'great scientists' can have tacit *foreknowledge* of future discoveries:

> [The Great Scientist] can have tacit foreknowledge of yet undiscovered things. This is indeed the kind of foreknowledge the Copernicans must have meant to affirm when they passionately maintained, against heavy pressure, during one hundred and forty years before Newton proved the point, that the heliocentric theory was not merely a convenient way of computing the paths of planets, but was really true (Polanyi 1967, 23).

Appraisal, therefore, is a task for practicing scientists. Ignoring Miller's results on some commonly held 'hunch' is justified just because the relevant community held it.

Polanyi provides most of the implications for the organization of science in his marvellous paper "The Republic of Science." Polanyi is also a pluralist and suggests a 'polycentric spontaneous order' of science where scientists are compelled by their own peculiar interests to contribute to the growth of public knowledge (55-6). Wherever there is natural growth or declination in knowledge, grants must be awarded or declined accordingly (63). Such pluralism must be constrained by requiring a "sufficient degree of plausibility" (56), as determined by their peers.[264] In Lakatos' words, we arrive at the view that:

> There can be only a *case law*, no *statute law* for deciding what is scientific and pseudoscientific, what is a better and what is a worse theory. It is the jury of scientists which decides in each separate case and as scientific autonomy…is upheld, nothing will go very wrong (Lakatos and Zahar 1976, 176).

[264] Polanyi has an interesting view of who counts as a peer. There are specialists who have "competent judgment only over a small part of science" (60) which includes their discipline and its 'overlapping or neighboring' disciplines. Science, as a whole, is a chain of these overlapping disciplines. Therefore, "[e]ach link in these chains and networks will establish agreement between the valuations made by scientists overlooking the same overlapping fields, and so, from one overlapping neighborhood to the other, agreement will be established on the valuation of scientific merit throughout all the domains of science" (ibid). This makes it possible to make a "comparison between the value of discoveries in fields as different as astronomy and medicine" (63). Therefore, we have a broad community consensus, which is vague and unspoken, and more concrete consensus that are more fully articulate at local levels.

Polanyi is not alone in this endorsement. Toulmin, at times,[265] also denies the possibility of normative methodology in a similar way. In the same vein, Kuhn writes:

> To astronomers the initial choice between Copernicus' system and Ptolemy's could only be a matter of taste, and matters of taste are the most difficult of all to define or debate. Yet as the Copernican revolution itself indicates, matters of taste are not negligible. *The ear equipped to discern geometric harmony* could detect a new neatness and coherence in the sun-centered astronomy of Copernicus, and if that neatness and coherence had not been recognized, there might have been no revolution (emphasis added, Kuhn 1957, 177).[266]

What implications does this have for scientific methodology? Lakatos' rescue of rationality consists in having theories of rationality *anticipate* and thereby *explain* basic judgments of scientific experts on particular cases. This allows us to revise previously held basic judgments (Lakatos 1974b, 351). On this picture, good sense and tacit knowledge are merely implicit kinds of rationality that can be accommodated into formal models. As Zahar puts it, "Lakatos is interested, not *in the reasons* given by scientists as to why they choose certain theories, but only *in the fact that* they choose them" (Zahar 1982, 406).[267] This, as others have noted, forms the basis of much of our current practices of peer review (cf. Avin 2015a) which institutionalizes tacit knowledge. However, the pertinent question of this approach is how it is *appraised*; why should we accept a tacit approach to scientific methodology?

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[265] Toulmin is exceptionally difficult to place within this tradition. On the one hand, he explicitly equates his own views of rationality to Polanyi's (Toulmin 1976, 661) but then says that scientific praxis can be criticized by external standards and wants to extend the Popperian program of making all scientific practice susceptible to rational criticism: "however far these procedures may tend to be left 'tacit' in actual scientific practice…does not exempt them from being brought into the open and exposed to rational [i.e., philosophical] criticism" (665), a point he makes earlier in his career as well (Toulmin 1970, 40). However, he also writes how "[o]nly the practitioner can understand the training and practice, discipline and method, strategy and imagination called for in the supreme execution of his activity" (Toulmin 1961, 13) though we may judge the merit of the *products* of this activity on philosophical grounds. At other times, he invokes Wittgenstein's insight that linguistic symbols presuppose 'forms of life' to have meaning and, therefore, the products are only partially appraisable by rational standards (cf. Toulmin 1982, 106-7). I will leave this alone for now, since it is unclear to me what parts of science can be subject to rational criticism on Toulmin's view.

[266] Given that this quote is from *The Copernican Revolution*, which Kuhn wrote as a textbook and not a treatise, this textual evidence should be taken with a grain of salt. Kuhn's brief positive remarks on Polanyi in *Structure* confirms the suspicion that Kuhn shared this view (Kuhn 1962, 160).

[267] "Up to the present day it has been the scientific standards, as applied "instinctively" by the scientific élite in particular cases which have constituted the main – although not the exclusive – yardstick of the philosopher's universal laws" (Lakatos 1971, 122). However, in Lakatos' savage review of Toulmin, he mocks the idea of the scientific élite having a "hot line to the Cunning of Reason" (Lakatos 1976b) and calls it "just about the worst of all possible philosophical worlds" (241) so it isn't clear that Lakatos held the view Zahar attributes to him. See also Lakatos 1978c, 111-120 for further discussion.

The answer, in a nutshell, is to avoid cranks. Polanyi writes "[s]cientific publications are continuously beset by cranks, frauds and bunglers whose contributions must be rejected if journals are not to be swamped by them. This censorship will not only eliminate obvious absurdities but must often refuse publication merely because the conclusions of a paper appear to be unsound in the light of current scientific knowledge" (Polanyi 1962, 60). "Only the discipline imposed by an effective scientific opinion can prevent the adulteration of science by cranks and dabblers" (62). As we saw, this is a concern that Feyerabend shares. It is worth seeing what remains of this tacit approach once we filter it through a Feyerabendian analysis.

According to the Polanyiites, 'false' judgments of the scientific élite are really just verbal shortcuts to more profound resistances among scientists. These resistances can be appraised on the grounds of their *reliability*.[268] To respond to this, let's first note that tacit knowledge conflicts with the maxim of testability since it introduces untestable features of methodology. However, it may be the case that recourses to tacit knowledge are *unavoidable*; we are forced to include them in our account of methodology. Let's begin by repeating what we learned: meta-inductions on the reliability of tacit knowledge presuppose the existence of very stringent conditions. The relevant community (or communities) must acknowledge it, such a community must be in a pre-existing state of diversity, and the sociological conditions must not assume rationalist pretenses.[269] Assuming these conditions are met, we are still left with the question of whether tacit knowledge of this kind is *avoidable*. Tacit knowledge comes in degrees. There is a massive difference between "I can't quite put my finger on *x*" and the expectation that scientists constantly explore their instinctual convictions about the nature of their trade such that they can be brought to bear on everyday decision-making. We can expect scientists to put some of their gut feelings into arguments but maybe not others. But this distinction appears valid because some decisions must be made *now*; we cannot wait for tacit knowledge to be made explicit. I think this is where a Feyerabendian must appeal to the tacit dimension. We accept the *conclusions* of arguments of scientific peers when they are convergent from different sources in communities organized in a particular way when attempts to make the tacit knowledge underpinning it explicit are foregone. However, this is a *concession out of pragmatic necessity*.

[268] I will grant that these judgments actually are reliable though this claim appears far-fetched (cf. Stanford 2006, 174).

[269] Additionally, as feminist philosophers have made apparent for decades, that 'extra-scientific' judgments often play roles in scientific decision-making must be taken into account.

Tacit knowledge, when and where it appears,[270] is to be regarded as unfortunate. Wherever it seems pragmatically workable, tacit knowledge must be attempted to be made explicit.

But perhaps even this minor concession isn't allowable without introducing a major conceptual difficulty. Lakatos put this problem best:

> Everyone…is bound to use normative third-world criteria, whether explicit or hidden, in establishing criteria for a scientific community. Merton, for example, no doubt decided what theories to select as scientific before he characterized the institutionalizations of science…But if one must have *some* idea of what constitutes science before one knows which communities ought to count as scientific, then one must first decide what constitutes scientific progress [methodology]. From the solution of this normative problem one can *then* proceed to the empirical problem of what socio-psychological conditions are necessary (or most favourable) for producing scientific progress. This is precisely how demarcationists approach the sociology of science. They regard the problem of quality control of products, different problems will face the sociologist according to which answer he presupposes (Lakatos 1978c, 114).

More succinctly put, is a conception of scientific methodology logically prior to the sociological conditions under which that methodology is best implemented? Perhaps I have committed a category mistake by introducing tacit knowledge into a Feyerabendian methodology, which rests on inconsistent assumptions, instead of a sociological method of expediting pluralism. I don't think this is a problem for the view adopted here, though it does help to clarify the role and justification of tacit knowledge. Tacit knowledge does not 'trump' methodological commitments, on my view; those stand or fall on the basis of critical discussion. As such, tacit knowledge cannot be used to stifle proliferation or terminate tenacity. But it can be used for the heuristics of pursuit; the ways in which scientists gauge the value of projects *relative to some extant research program*. Tacit knowledge, therefore, has the function of aiding scientists in determining what projects are more valuable than others given a tentatively shared set of commitments. To be clear, tacit knowledge does not necessarily promote complete and utter conformity; internal diversity within research programs may still be a virtue. But this diversity would be constrained by standards that remain more general and commonly accepted.

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[270] It seems reasonable to distinguish between productive features of tacit knowledge, which can help accelerate research, and tacit knowledge used for the sake of excluding cranks. Since excluding cranks is the sole negative function of tacit knowledge, on my view, we only need to regard the tacit disavowal of research as unfortunate.

4. Chapter Summary

Accepting Feyerabend's views without critically engaging them would be profoundly contrary to the spirit of Feyerabend's philosophy. The material discussed in this chapter has provided us with resources to critically assess and revise Feyerabend's views. To summarize this chapter, I have defended Feyerabend from some of the most prominent, yet superficial, criticisms of his pluralism. The rest of the chapter can be divided into two categories: *clarifications* and *modifications* of Feyerabend's pluralism. The clarifications can be summarized as follows:

1) Feyerabend's pluralism is a methodology of theory *pursuit*, not theory acceptance.
2) Theory pursuit comes in stages that are demarcated by sociological criteria.
3) Methodology is logically prior to historical meta-inductions.

Each of these clarifications is in keeping with Feyerabend's pluralism, but he did not spell them out himself. The results of the modifications of Feyerabend's pluralism can be summarized thusly:

Feyerabend's Pluralism	**Feyerabend's Modified Pluralism**
The principle of tenacity extends indefinitely.	Levels of investment will vary according to cost-benefit analyses at differing stages of pursuit.
There are no rules for how to pursue a theory.	Practitioners within a research tradition have an imperfect duty to 'attend criticism.'
Proliferation is unconstrained.	Only the proliferation of *ideas* is unconstrained. Proliferation of *costly theories* is constrained.

Table 2: Modifications to Feyerabend's Pluralism

This modified version of Feyerabend's pluralism may be called 'Feyerabendian' in the sense that it accepts Feyerabend's view that proliferation and tenacity are epistemically unconstrained and constitute the basis of scientific methodology. It also accepts that value judgments are also necessary to make pluralism a bona fide normative position. What changes I've recommended are in keeping with the spirit of Feyerabend's thought, but which he never elaborated. After all, Feyerabend was happy to allow value judgments and practical considerations to influence scientific development but he never followed through on what the interaction between these values and considerations and his anarchism may be. Similarly, Feyerabend knew there must be some balance between proliferation and tenacity, but never provided one himself. The reasons for these absenses are likely due to Feyerabend's personality (he never liked to be settled down with a 'scholarly account') and the dialogues he was engaged in. Despite the fact that I have argued that Feyerabend's anarchism provides the seeds for a viable conception of methodology, his anarchism was also partially rhetorical; it was meant to shock and challenge his audience. The rhetorical effectiveness of *AM* would have been severely muted if it had been detailed in the way I have done so in this Chapter making it reasonable, given his goals at the time, that he never elaborated an economics of theory pursuit. Regardless, we live in a more enlightened age which provides us with the opportunity o make these developments to be reflected upon in a more open-minded fashion that Feyerabend could have expected.

In total, this provides us with a clearer and workable version of Feyerabend's pluralism from which we can move forward. In the next chapter, I will attempt to do just this by demonstrating what implications Feyerabend's pluralism has for models of how we should distribute resources within scientific communities.

Chapter 4
Feyerabend's Well-Ordered Science

> "Even in the best of times, managing science can be compared to herding cats; it is not done well, but one is surprised to see it done at all" (Holton et al. 1996, 364).

There is a sense in which the question of how science should be organized is as old as philosophy of science itself. Even Plato, in the *Republic*, had thoughts about the division of cognitive labor within communities of inquirers: divide them according to their natural talents as demonstrated and cultivated within a general education system. That being said, science has evolved in how its communities have been ordered throughout history. In ancient Greece and China, natural philosophers gathered observations, constructed theories, and (mostly) passed them on orally, and 'science' was not a separate discipline but a part of inquiry more generally.[271] In the middle ages in Europe, gathering knowledge of the natural world was concentrated in academic centres with the growth of many *Studium Generale* that "laid far greater emphasis on science than does its modern counterpart and descendent" (Grant 1984, 68) which were intellectually separate from the Church (cf. Rashdall 1895, Chapter 1).[272] After the scientific revolution, curious individuals funded privately, mostly by wealthy entrepreneurs, were the primary producers of scientific knowledge. However, during the 19th century, we see the rise of 'the scientist' or the *professionalization* of science as its own discipline. This transformed the nature of the organization of science. As George Daniels writes:

> Recent writers on the history of science in America generally agree that conditions underlying the pursuit of science changed drastically during the nineteenth century. By the middle of the century, the earlier pattern of gentlemanly scientific activity was rapidly becoming obsolete. The amateur was in the process of being replaced by the trained specialist—the professional who had a single minded dedication to the interests of

[271] Ancient Chinese science was far more 'communitarian' than ancient Greek science in the sense that it involved more collective projects and tended to be more technologically oriented whereas the Greeks were far more theoretical and speculative. See Lloyd (1996) for a masterful comparison of the two means of organizing scientific communities.

[272] This is the case from at least 1231 with Pope Gregory IX's declaration of the *Parens Scientiarum*.

> science. The emergence of a community of such professionals was the most significant development in nineteenth-century American science (Daniels 1976, 63).

After World War II,[273] the professionalization of science completely changed, as Steven Shapin put it, to "bring about a state of affairs that had no substantial historical precedent or ancestry" (Shapin 2008, 64). The central part of this change was the movement towards *centralized public funding*. After the 'success' of the Manhattan project and the need for internationally coordinated technologies during the Cold War, science became an instrument in the service of the state and, indirectly, the general populace. No longer was it the case that scientists could investigate whatever phenomena struck their fancy; now, they have a *responsibility* to produce particular kinds of knowledge with particular goals in mind. Vannevar Bush, who successfully managed the majority of the American military research and development (including the Manhattan project) during the War and orchestrated what became the National Science Foundation, sought to remedy the apparent tension between science as 'traditionally practiced',[274] an unmitigated pursuit of truths about the natural world, and the newly formed social obligations of science. He explicitly recognizes this tension:

> Much of scientific research done by Government agencies is intermediate in character between the two types of work commonly referred to as basic and applied research. Almost all Government scientific work has ultimate practical objectives but, in many fields of broad national concern, it commonly involves long-term investigation of a fundamental nature. Generally speaking, the scientific agencies of Government are not so concerned with immediate practical objectives as are the laboratories of industry nor, on the other hand, are they as free to explore any natural phenomena without regard to possible economic applications as are the educational and private research institutions (Bush 1945, 233-4).

Bush's own resolution involved defending the autonomy of science from social and political interests. The interests are addressed indirectly:

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[273] There were certainly other forms of government funding of science before this time that were gradually increasing in terms of funds and size (cf. Bush 1945, 233). However, as Bush also makes clear, there was no such thing as a 'unified science policy' or a centralized body for distributing resources (in the U.S. at least; See Wilholt and Glimell (2011) for a discussion of funding policy in continental Europe during this time).

[274] This narrative, that the social obligations of science came along with public funding, is certainly a simplification as the variegated relationships between states and scientists have often resulted in similar obligations. However, it is true that the institutional freedom of science was more pronounced than the 'instrumentalist' view of science throughout European and post-settlement Americas (cf. Wilholt 2010 and the citations therein).

> Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for the exploration of the unknown (235).

> Discoveries pertinent to ... progress have often come from remote and unexpected sources, and it is certain that this will be true in the future (237).

This view was met with a mixed reaction. Some, most notably Senator Harley Kilgore, being skeptical of *laissez-faire* approaches in general in the wake of Roosevelt's New Deal (Bush himself was an anti-New Deal conservative), wanted greater control over the kinds of projects scientists pursued (cf. Kelves 1977).[275] This debate reflected not just debates about the relationship between science and society and political debates about academic freedom (cf. Wilholt 2010), but methodological debates about the nature of scientific progress. Fascinatingly enough, philosophers have scantly discussed these developments, despite their significance for our understanding of science until recently, even though some have gone so far as to claim that the methods of organizing research in the latter half of the 20th century fundamentally reorient how science should be understood (cf. Fahrbach 2011 and Wray 2013). For Feyerabend's pluralism to have concrete implications, this historical context must be taken into account. The goal of this chapter is to do just this and discuss the implications Feyerabend's (modified) pluralism has for models of how centralized agencies should distribute their funds amongst diverse and, often, competing research projects.

Before beginning, it is necessary to make a few idealizing assumptions to simplify our discussion of how to distribute resources. For the sake of this Chapter, I will assume that our hypothetical scientific community is *sufficiently diverse*. By 'diverse', I do not mean diversity of *content*; this will be under consideration. Rather, I mean diversity in a general sense; scientists with different theoretical orientations, critical dispositions, perspectives, values, and so forth. This may, and most likely would, extend to diversity of 'embodied perspectives' as feminist philosophers have often argued for (see also Harding 1993). While it is an exceptionally difficult question as to what this 'diversity' would look like and what constitutes a 'sufficient amount' of diversity, I will have to sideline this topic for another day. Second, I will limit my discussion to *public* funding. Given that R&D funding far surpasses public expenditure on science, this is, in

[275] See Berman (2012) for a fascinating overview of the development of 'academic science' as being driven by economic (i.e., 'practical') interests during the 20th century.

one sense, a hefty limitation. On the other hand, many corporations use similar structures for allocating their resources meaning that the analysis provided in this chapter will still be of some use for private companies. The primary difference, though, is that private corporations are (arguably) under no obligation to spend their money on the 'common good.' Public funding, however, clearly has such an obligation to keep in mind. Finally, I will assume a basic level of altruism and honesty from scientists and administrators. This is a non-negligible omission since disincentivizing fraud and governmental corruption are both important issues (cf. Relman 1989 on fraud and Kellow 2007 on corruption). Regardless of these omissions, I hope that the analysis in this chapter will still shed light on the methodological challenges[276] of funding distribution models and aid in the development of reasonable science policy.

The structure of this chapter is as follows. In the first section, I try to unpack the notion of a 'well-ordered science.' I begin, in the first section, by outlining Philip Kitcher's account of this notion, which captures the central issues I want to focus on. I then outline three models for resource allocation, those of Kitcher himself, Michael Strevens, and Michael Weisberg and Ryan Muldoon, and show how they unwittingly share some of the central assumptions of a well-ordered science. In the second section, I analyze the concept a well-ordered science from a Feyerabendian perspective. Specifically, I focus on the distinction between basic and applied research, the notion of 'urgent science', and purported ethical constraints on theory pursuit. By doing this, I show how the concept of a well-ordered science falls short of a general funding policy and has a much more limited role than its proponents recognize. In the third section, I focus on the literature on peer-review as it is pertinent for understanding the role allotted to tacit knowledge in Chapter 3. Here, I survey the empirical literature on peer review to show that it is inherently conservative or, to put it in more familiar terms, is counter-productive to proliferation. I go onto divide peer review into two distinct contexts: the context of publication and the context of funding and analyze each separately. I conclude by suggesting that there is a role for *public review*, an emerging alternative to peer review, and show how it is (mostly) compatible with the Feyerabendian position outlined in the previous chapters. I conclude by summarizing these

[276] To be exceptionally clear, this chapter cannot, by its very nature, make any *direct* demands of funding bodies since I am focused on the methodological aspects of funding. This must be combined with value judgments and feasibility constraints to make policy suggestions to this or that funding body.

findings and gesturing at future avenues for developing a more robust account of a Feyerabendian model of resource allocation.

1. On a Well-Ordered Science

The title of this Chapter is an homage to the notion of a 'well-ordered science' developed by Philip Kitcher in his paper "The Division of Cognitive Labor", Chapter 8 of *The Advancement of Science* and, finally, in Chapter 5 of *Science in a Democratic Society*. Therein, Kitcher brings together a host of questions under the heading of how science should be organized. While Kitcher's own account is much too ambiguous to be examined in its own right,[277] it sets the stage for the kinds of questions that are relevant for distributing resources. Specifically, it highlights, but unwittingly assumes, methodological assumptions that seem problematic given what we have learned in the previous three chapters. In this section, I will outline the conception of a 'well-ordered science' and what assumptions it rests on.

1.1: What is a Well-Ordered Science?

Before beginning on the details of Kitcher's account,[278] it is worth noting what is meant by science being 'well-ordered.' Kitcher focuses his account on public spending[279] and contrasts his account with what he calls the 'autonomist' view of science. He writes,

> "We already know," [champions of autonomy] declare, "that directed scientific research does badly; that it has been wonderfully fruitful in the past for brilliant scientists to explore their hunches, that unanticipated benefits come from inquires into apparently impractical questions, and that the source of science is unpredictable." Arguments like these are often made from the armchair - or *ex cathedra*. The autonomist has a few bits of anecdotal evidence…In fact, little is known in any systematic way about the responsiveness of scientific research to social directives. The basis for any hypothesis about the bad effects of something like well-ordered science is extraordinarily thin….So far, the social study of scientific knowledge cannot deliver a statistical basis from which anyone can project the likely effects of attempts to plan different kinds of research (Kitcher 2001, 119).

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[277] To be clear, Kitcher's own discussion of a well-ordered science is quite muddled with numerous contradictory asides and several claims that appear to be in tension with each other. It is because of this that I am sidelining many parts of the text to outline what I take to be the central positions that Kitcher wishes to defend.

[278] This view is adopted and slightly modified by Cartwright (2006) and also defended, in a different pretext, by Alexandrova (2017, section 4.2).

[279] Kitcher restricts his ideal to public research stating, quite crudely, "privatization of scientific research will probably make matters worse" (Kitcher 2001, 126). See Pinto (2015) for a criticism of this.

While Kitcher does not elaborate on who the proponents of this view are, we have already seen Polanyi defend the autonomy of science. Lakatos also, at one point, defends the autonomist view:

> In my view, science, as such, has no social responsibility. In my view it is society that has a responsibility – that of maintaining the apolitical, detached scientific tradition and allowing science to search for truth in the way determined purely by its inner life. Of course scientists, as citizens, have responsibility, like all other citizens, to see that science is *applied* to the right social and political ends. This is a different, independent question, and, in my opinion one which ought to be determined through Parliament (Lakatos 1978d, 258).

Despite the dismissive account of the evidence of the autonomist view, which is a much more complicated ordeal than Kitcher suggests, we get a hint of what it means for science to be 'well-ordered.' On Kitcher's view, what distinguishes the well-ordered view from the autonomist view is the *origin of the goals of research*. For autonomists, it is determined by purely methodological considerations: the 'inner life' of science. Kitcher's view, as we will see, demands that these goals come from democratic deliberations.

Kitcher recognizes the point, stressed by Poincaré in the previous chapter, that scientists encounter excessive amounts of mundane facts. The hope, then, is that science aims not just for discovering any old fact, but *significant facts*.[280] However, significance is not construed in purely epistemic terms. Kitcher invites us to consider the following three options:

A. The aim of Science is to discover those fundamental principles that would enable us to understand nature.
B. The aim of Science is to solve practical problems.
C. The aim of Science is to solve practical problems, but, since history shows that the achievement of understanding is a means to this end, seeking fundamental principles…is an appropriate derivative goal (Kitcher 2001, 109).

A is essentially the autonomist view; we have some conception of scientific methodology which provides us with an *epistemic* notion of significance.[281] On this view, scientists should pursue

[280] This is explicitly a normative claim and isn't descriptive of at least some scientific practices. See Feyerabend (1965b, 156-7) for an overview of the museum set up by the Royal Society that displayed random facts without any sort of 'selective principle.' This included the most trivial facts imaginable, such as the following: "1661, July 24: a circle was made with powder of unicorn's horn, and a spider set in the middle of it, but it immediately ran out several times repeated. This spider once made some stay upon the powder" (Weld 1848/2011, 219). See pg. 93 for an appraisal of the museum as being "necessary for the welfare of science."

[281] It's worth mentioning that Kitcher's earlier account of realism (cf. Chapter 5, Kitcher 1993) is in tension with this conception of significance since he allows us to distinguish different parts of theory from which we can discern a purely epistemic notion of significance.

epistemically significant research and the political and ethical questions come *afterwards* about how to apply such knowledge once we have it. I interpret 'practical problems' in B and C to be problems that have some ethical or political impetus behind them. C is essentially Bush's view, as outlined earlier in this chapter. There is some ambiguity as to what Kitcher's own view is. Kitcher thinks B is too narrow for the reasons given by C. He writes:

> Often the best route to potential gains down the road is to investigate quite recondite questions: Thomas Hunt Morgan's wise decision to postpone considerations of human medical genetics and concentrate on fruitflies prepared the way for the (ongoing) revolution in which molecular understandings are transforming medical practice (ibid).

Still, Kitcher thinks, C is limited since it "fail[s] to recognize the ways the ethical project has expanded the scope of human desires, equipping us with richer notions of what it is to live well" (ibid). I am unsure what this objection amounts to. Regardless, Kitcher later argues that the goals of research should be determined *democratically*. Kitcher provides an ideal deliberative democracy in which various participants with different backgrounds give 'tutored' preferences for what the goals of research should be. Ideally, this deliberation should lead to a consensus of "the entire spectrum of their society's projects, they judge a particular level of support for continuing research…and they agree on a way of dividing the support among various lines of investigation" (115). This suggests that Kitcher actually holds B where what counts as 'practical' is determined by a particular community according to certain democratic procedures. After all, if C were true, then at least some projects would be beyond the purview of democratic decision making since they are 'autonomist' in the sense that they are guided by scientists own preferences.

Finally, Kitcher thinks that the *process* of scientific inquiry is subject to ethical regulation. We should not engage in Tuskegee-type experiments, regardless of what their results may be. Kitcher seems to think that these constraints are justified independently of democratic deliberations. Additionally, theory *pursuit* is constrained by ethical parameters. Kitcher makes this clearer elsewhere; we should not pursue lines of research that would interfere with the prospects of underprivileged groups pursuing their conception of the good life (e.g., sociobiology) (Kitcher 1997, 296-7). These constraints are also independent of democratic decision-making. From all of this we can gather a few central features of a well-ordered science:

(1) The goals of research are determined democratically.

(2) Some goals of research are ethically forbidden.
(3) Science is regulated by certain ethical principles.

Kitcher leaves it up in the air as to *how* scientific communities should reach the given goals of research. This leaves open a question of crucial importance: do the *ends* determine the *means* of research? In other words, do democratically decided goals determine what particular research projects should be funded? On my interpretation, Kitcher thinks that this is the case. If he did not, then both B and C would be equally desirable on his account. Kitcher, I think, wants a much more substantive 'ordering' that a mere statement of preferences from citizens. As a result of this, Kitcher's well-ordered science actually has four criteria:

(1) The goals of research are determined democratically.
(2) The goals of research determine the content of the research.
(3) Some goals of research are ethically forbidden.
(4) Science is regulated by certain ethical principles.

I will now go on to show how more detailed models of funding distribution unwittingly make use of the notion of a well-ordered science.

1.2: Kitcher on the Division of Labor

Recently, there have been several models that have attempted to make the notion of a 'division of labour', presupposed by Kitcher's presentation of a well-ordered science, using more precise using tools from economics, decision theory, game theory, and population biology. In the next three sections, I will go over three models that attempt to make this notion more rigorous; namely Kitcher's 'division of labor' model, the Weisberg-Muldoon model, and the Strevens-model.

Kitcher's defense of the division of labor begins with a hypothetical situation:

> Imagine that the objective degree of confirmation of the phlogiston theory just prior to noon on April 23, 1787, was 0.51, that of the new chemistry 0.49. At noon, Lavoisier performed an important experiment, and the degrees of confirmation shifted to 0.49 and 0.51, respectively. Allowing for a time lag in the dissemination of the critical information, we can envisage that there was a relatively short interval after noon on April 23, 1787, before which all rational chemists were phlogistonians, and after which all were followers of Lavoisier (Kitcher 1990, 5).

Clearly, allocating all possible resources to Lavoisier's research program in this instance would be an absurd way to organize a community. Kitcher writes "[w]ith the evidential balance between the two theories so delicate, you would have preferred that some scientists were not quite so clear-headed in perceiving the merits of the time theories, so that the time of uniform decision was postponed" (5-6). This makes it seem as if the division of labor is appropriate when we have theories *of similar merit* and, thereby, denouncing the pursuit of theories without sufficient degrees of merit. However, Kitcher then writes:

> In the 1920s and 1930s, Wegner's claim [continental drift] seemed to face insuperable difficulties, for there were apparently rigorous geophysical demonstrations that the forces required to move the continents would be impossibly large. Despite this, a few geologists, most notably Alexander du Toit, continued to advocate and articulate Wegner's ideas. I suggest that the distribution of cognitive effort was preferable to a situation in which even the small minority abandoned continental drift (7).

This appears to invoke some version of the principle of tenacity. If this is true, then we can pursue theories with *any degree of merit*. We are left without clear conditions for theory pursuit. However, the answer is implicit in his model. Kitcher asserts that each theory has a corresponding probability function that it will be true, $p(n)$, where n is the number of allocated resources. These probability functions have the following characteristics:

(1) P increase monotonically with n
(2) The value of p is 0 when n is 0.
(3) $P(n)$ approaches a limiting value as n goes to infinity (12).

(2) is straightforward since, clearly, we cannot achieve a goal without investing any resources in it. (3) assumes that theories have some intrinsic point at which they start making decreasing marginal returns. This is consistent with the Peircian view outlined in Chapter 3. (1), however, is more worrisome. Consider two functions:

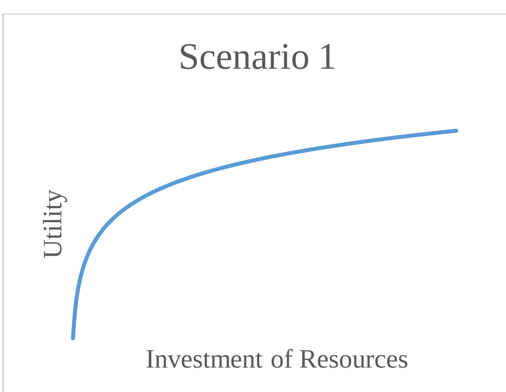

Figure 2: $Kitcher_1$

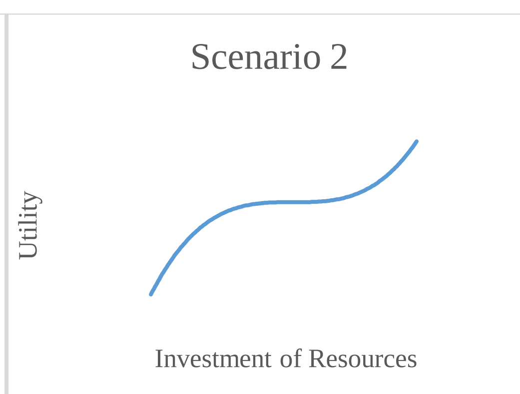

Figure 3: $Kitcher_2$

How are funders who are beginning to receive marginal returns supposed to know when they are scenario 1 or scenario 2? 2 may be worth continuing to pursue for future gains, but the same is not true for 1. Kitcher provides no answer to this question. He only argues that a 'philosopher-monarch', with perspicuous knowledge of their probability functions, may allocate resources amongst a scientific community. Kitcher is aware that this is an idealization, but he thinks it is merely an exaggeration of what scientists already do when they judge scientific theories. This leaves us without an answer to perhaps the most challenging question pertinent to distributing funding: how do *we* determine what the probability functions for research programs are?

Regardless of Kitcher's silence on this question, it is clear that any question to how we may know the value of $p(n)$ will also serve as a criteria for statement (2) about a well-ordered science. However, it is worth mentioning that the kinds of value judgments that go into these decisions will be slightly more complicated in that they will involve both the *goals* of research and whether a particular *cost-benefit* assessment is appropriate. While this model is silent on the relationship of theory pursuit to democracy or our values, it is perfectly compatible with the other tenets of a well-ordered science.

1.3: The Strevens Model

Michael Strevens' model is grounded on what Merton called the 'priority rule' whereby genuine discoveries are made by scientists who uncover them *first*. Strevens uses this observation to try to handle the resource allocation problem for *competing* research programs that aim to make the same discovery. He makes the following idealized assumptions:

(1) Every research program has a single goal. There are only two possible outcomes of the program's endeavors: total success, if it realizes the goal, or total failure, if it does not.
(2) Different research programs have different intrinsic potentials.
(3) A program's chance of success - that is, the probability that it will achieve its goals - depends on two things, its intrinsic potential and the resources invested in the program (Strevens 2003, 61).

(1) is a justified idealization in contexts where two research programs try to make the same discovery and the discovery itself is the main goal. However, as Strevens later notes, you can relax this standard with a research program with many success functions and this only adds to the complexity of the model. For (2), Strevens writes that a research program "has more intrinsic

potential that another if, given any fixed level of investment, the one has a higher chance of success than the other" where the "success functions for two programs do not intersect unless they entirely coincide" (ibid). He further assumes that the success function "does not change over time, and…that the central planner knows at all times the true form of the success function" (64). He gives no account of how we could determine what the intrinsic potential of any research program would be. If the success function "change[s] in unforeseeable ways" then "optimality may be out of reach" (67). This is not just true of this model, but is a "universal obstacle to achieving the best possible outcome when information is limited" (ibid). (3) follows from (2) and the trivial point that discovering truths requires investing resources into means of discovery.

Following Peirce and Kitcher, Strevens recognizes that optimal resource allocation is not devoting all resources to the theory with the highest intrinsic potential. Strevens mathematical presentation of the problem of resource allocation begins with a simple case:

(1) $\mathrm{V_T}(\mathrm{n_T}) = V_1S_1(n_1) + V_2S_2(n_2)$

Where V is the utility of the discovery of a research program, S is its success function, and n is the number of resources allocated to the research program, and the suffix T represents the total value for society as a whole. Additive cases, like those represented by (1), hold when they pursue 'independent' goods (e.g., a cure for cancer and the discovery of the Higgs boson). Nonadditive cases, which will represent the completion Strevens is interested in, is represented thusly:

(2) $\mathrm{V_T}(\mathrm{n_T}) = V(S_1(n_1) + S_2(n_2) – S_{12}(n_1 – n_2))$

Where the final term is the probability that, given a particular funding distribution, both research programs will be successful. Strevens then simplifies the formula by assuming that the utility function is built into the success function and moves on to the case of optimality of resource distribution. Assume that adding a resource improves the research program's chance of success, expected returns are given by:

(2') $\mathrm{V_T}(\mathrm{n_T}) = S_1(n_1) + S_2(\mathrm{n_T} - n_2)$

Additionally, Strevens assumes that success functions yield decreasing marginal returns (that $S(n + 1) – S(n)$, represented by $m(n)$), which is crucial since it allows for discovering constrained maximums which solves the optimization of resource allocation problem. We then get the local maximum defined as:

(3) $m_1(n) = m_2(n_T - n)$

Strevens' model, quite straightforwardly, inherits the assumption of Kitcher's model discussed in the previous section.[282] Similarly, Strevens is silent on how to determine how we could come to know the success function of a research program and there is no functional difference between Strevens' 'success functions' and Kitcher's 'intrinsic potentials.' It is therefore fair to say that Strevens has inherited a basic assumption of a well-ordered science.

1.4: The Weisberg-Muldoon Model

The Weisberg-Muldoon model (WM model) follows Kitcher in attempting to model an optimal strategy for allocating resources in the service of pre-defined goals. As such, their ideal community pursues significance as determined by the relevant community. They model their community on what they call an *epistemic landscape*, after fitness landscapes in population biology, where a single landscape corresponds to a 'topic.'[283] Scientists use an 'approach', defined quite broadly to include their research questions, instruments and experimental techniques, methods, and background theories. We end up with an epistemic landscape like this:

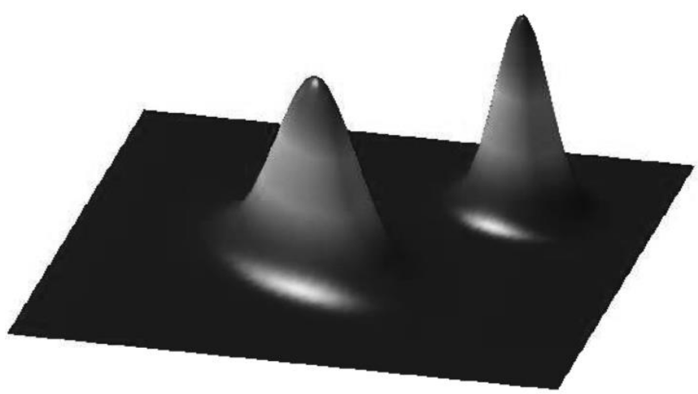

Figure 4: Example of an Epistemic Landscape (taken from Weisberg & Muldoon 2009, 230).

Here, the z-axis corresponds to degrees of significance and each point on the x-y axes (the topology is discrete), represents the choice of approach. Scientists are allocated randomly to

[282] Strevens' model replaces Kitcher's asymptotic assumption with the discovery of local maxima.

[283] Topics can defined broadly or narrowly. The WM model is aimed to represent "the topic that a specialized research conference or advanced level monograph might be devoted to" (Weisberg & Muldoon 2009, 228).

different points on the landscape (at zero-significance) and move according to different rules. Weisberg and Muldoon give three algorithms corresponding to different scientific 'attitudes'. First there are 'controls', with the following rule:

1. Move forward one patch.
2. Ask: Is the patch I am investigating more significant than my previous patch?
    a. If yes: Move forward one patch.
    b. If no: Ask: Is it equally significant than my previous patch?
        i. If yes: With 2% probability, move forward one patch with a random heading. Otherwise, do not move (Weisberg and Muldoon 2009, 231).

Controls do not adjust their behaviour according to the behaviour of other scientists and are guaranteed to find a local maximum in finite time (232). Next are 'followers' with the following rule:

> Ask: Have any of the approaches in my Moore neighborhood been investigated?
> Ask: Have any of the approaches in my Moore neighborhood been investigated?
> If yes: Ask: Is the significance of any of the investigated approaches greater than the significance of my current approach?
> If yes: Move towards the approach of greater significance. If there is a tie, pick randomly between them.
> If no: If there is an unvisited approach in my Moore neighborhood, move to it, otherwise, stop.
> If no: Choose a new approach in my Moore neighborhood at random (240).

And, finally, what Weisberg and Muldoon call 'mavericks':

> Ask: Is my current approach yielding equal or greater significance than my previous approach?
> If yes: Ask: Are any of the patches in my Moore neighborhood unvisited?
> If yes: Move towards the unvisited patch. If there are multiple unvisited patches, pick randomly between them.
> If no: If any of the patches in my neighborhood have a higher significance value, go towards one of them, otherwise stop.
> If no: Go back 1 patch and set a new random heading (241).

Simulations are run with communities with various mixes of controls, followers, and mavericks. Progress is defined as the number of maxima discovered and the speed at which they are discovered. Since I am more interested in the philosophical basis of the model, I will forgo analyzing the results and focus on the assumptions undergirding their model.

Weisberg and Muldoon make several idealizing assumptions explicit.[284] First, there is no notion of research cost in the model. Each move is, therefore, identical in cost.[285] Next, scientists are permitted to move according to their algorithm. Additionally, Weisberg and Muldoon are explicitly following a Kuhnian model: "All of our agents are doing normal science" (249) though their division of labor doesn't exactly line up with Kuhn's (cf. Kuhn 1962, chapter 3 and 4). This is because each agent is presupposing achievements that came before them and attempt to build upon this knowledge, rather than undermine it as a Popperian critic may attempt to do. Finally, and most importantly, the notion of significance is static; it doesn't evolve as research continues (Weisberg and Muldoon 2009, 232). This is compounded by the idealization that each topic can be understood without reference to other topics.[286]

While Weisberg and Muldoon make several idealizing assumptions explicit,[287] the WM model is interestingly different from Kitcher's and Strevens'. First, they explicitly acknowledge that significance is a value-laden term and implicitly adopt premises (1) and (3). The question of whether they adopt (2), however, is somewhat complicated. Each movement algorithm has scientists move according to whether the last approach was fruitful or not. This assumes that what was fruitful will continue to be fruitful and what had zero-significance will lead towards a dead end. This is slightly different from Kitcher and Strevens models since, on the WM model, our projections of future content will change as research evolves. However, it still requires that we *predict* future significance nonetheless. Second, the very idea of a maximum presupposes that there is no further progress that could be made with that approach. In other words, since all possible movements at a maximum will be regressive, by definition, we can predict what that

[284] Two assumptions have already been criticized. Thoma (2015) criticizes the assumption that agents can only move locally (i.e., coding of the controls have "those agents behaving like lethargic random walkers" (433) within their Moore neighborhood), and that the purported benefits of the division of labor are more limited in higher dimensional landscapes (425)) especially given the fine-graininess of the distinction between adjacent patches (462). Alexander et al. (2014) criticize the breadth of kinds of agents chosen.

[285] The notion of 'cost' cannot be embedded into the notion of 'significance' since this would change the notion of the epistemic landscape. If it was, the epistemic landscape would change as costs change which is not the notion that is simulated.

[286] It is possible to embed one topic within another, but this would not work with differing views of significance since scientists would have inconsistent movement algorithms.

[287] Two assumptions have already been criticized. Thoma (2015) criticizes the assumption that agents can only move locally (i.e., coding of the controls have "those agents behaving like lethargic random walkers" (433) within their Moore neighborhood), and that the purported benefits of the division of labor are more limited in higher dimensional landscapes (425)) especially given the fine-graininess of the distinction between adjacent patches (462). Alexander et al. (2014) criticize the breadth of kinds of agents chosen. These criticisms are distinct from the ones I am concerned with in this paper.

future research cannot be progressive once we've reached a maximum. In these two ways, the WM model presupposes that goals determine what paths of research scientists should follow.

2. Feyerabend's Well Ordered Science

In the previous section, I outlined the notion of a well-ordered science and some of its variegated explications. There is a wealth of points of contact between the Feyerabendian pluralism I have developed in the previous three chapters and the various defenses of a well-ordered science. This section will provide a critical analysis of some of the themes present in the notion of a well-ordered science grounded in the methodological lessons we have already learned.

2.1: Applied and Basic Research

The notion of a well-ordered science reopens an extremely old debate about the relationship between truth and practicality. Clearly, I can hardly give a full examination of it here. But it is worth looking at what assumptions are hidden in a Feyerabendian well-ordered science.

One view was captured by the famous dictum: knowledge is power.[288] On this view, pure unadulterated knowledge about the world is prior to our ability to act in the ways we desire. This has sometimes been called the 'cascade view': applied knowledge cascades from basic knowledge (cf. Adam et al. 2006). On the opposite side, we see William James defending the view that practical markers are necessary signs of truth. Some take this to amount to the view that basic knowledge emerges out of applied contexts (the 'emergentist view'). However, if we take Feyerabend's account of pursuit seriously, it isn't always clear which comes *first*. What appears to be knowledge grounding practical action may turn out to have been a useful falsehood and what appears as 'true' can have as yet unforeseen practical benefits meaning that any piece of information can be 'knowledge' at one point in history and 'useful' at another. We have seen how Kitcher is already ambiguous about what he takes the *goals* of research to be and his answer appears to depend on the question of whether truth or practical applications comes first. In the

[288] This quote is normally attributed to Bacon, though Bacon's actual formulation is that *knowledge itself is power*, which is extremely different. The phrase 'knowledge is power' first occurs in Hobbes' *Leviathan*. Bacon's own view of the relationship between society and science is much more complicated than usually thought (cf. Urbach 1988).

20th century, we see Vannevar Bush echoing the same sentiment when justifying the value of 'basic research':

> Discoveries pertinent to medical progress have often come from remote and unexpected sources, and it is certain that this will be true in the future. It is wholly probable that progress in the treatment of cardiovascular disease, renal disease, cancer, and similar refractory diseases will be made as the result of fundamental discoveries in subjects unrelated to those diseases, and perhaps entirely unexpected by the investigator. Further progress requires that the entire front of medicine and the underlying sciences of chemistry, physics, anatomy, biochemistry, physiology, pharmacology, bacteriology, pathology, parasitology, etc., be broadly developed (Bush 1945, 237).

Indeed, a great deal of social scientific literature has confirmed repeatedly how knowledge with no immediate practical benefits has, in turn, been necessary for future practices and addressing all kinds of social problems. Our (modern) knowledge of the greenhouse effect depends on our knowledge about blackbody radiation, our knowledge of mental illnesses depend on electrochemistry, and so on *ad nauseum*. It appears as if basic research conforms extremely well to Feyerabend's argument that we cannot predict the content of future research. However, I don't think that Feyerabend merely recites this point, but helps us to understand how to understand the basic/applied distinction. Recall that, for Feyerabend, the distinction between what counts as a 'fact, 'successful theory', and even 'progress' are *reflections of tentative stages in our knowledge* (AM_{B1}, 167). Why not extend this view to the distinction between basic and applied research? Form this, it follows that we can only distinguish between basic and applied research *given a certain timeline*. The content of basic or applied research can only be understood relative to a state of knowledge which is constantly in flux. Now, recall from Chapter 3 that we granted that we can make reasonably accurate projections about the value of research within normal science. These projections will be, by their very nature, short term. In these kinds of situations, we can determine which research is 'practical' and what isn't. But this only covers funding during periods of tenacity, it does not tell us how to fund proliferated competitors which is an essential part to a general funding policy. I think that the answer to this question will come out more clearly when we think about the cases that Kitcher claims are 'rare': urgent science.

2.2: Urgent Science

One issue Kitcher, and Feyerabend for that matter, never discuss is the issue of *time frames*. That is, how much time will it take to achieve certain results. Kitcher's well-ordered science can easily be interpreted as focused, almost exclusively, on short-term gains. Feyerabend's pluralism has some interesting consequences if the consideration of time frames is dropped. If we had an infinite amount of time, we could simply proliferate over and over again with little tenacity and achieve whatever results we want. However, proliferation has extremely little benefits in the short term and, thus, we would only tenaciously develop established theories if we only looked at our immediate next steps in the growth of knowledge. Regardless, an analysis of science that is *needed urgently* is clearly of vital importance for understanding Feyerabend's pluralism in action.

Kitcher claims that cases of 'urgent science' are rare (Kitcher 2001, 136). However, there are a wide variety of *prima facie* cases where scientific knowledge is urgently needed; climate change, diseases (especially outbreaks and epidemics), mental illnesses, and so forth come readily to mind.[289] Even scientific knowledge that aids luxury items can bolster economic activity and increase spending power. It could even be argued that *all* cases of applied science are 'urgent' in a sense; why not be as expedient as possible? In this section, I will consider the importance of urgent science in our conception of a well-ordered science.

Recall that we are interested in two different contexts of funding:

(1) Funding during tenacity
(2) Funding proliferated alternatives

However, let's assume that some research within tenacity is expected to be practical within the near future, a projection we are allowed to make, and that some is not. For example, it is still an open question as to whether individual neurons compute. Though an answer to this question would be essential for future foundations of neuroscience, there is no expected answer in the short term (cf. Adolphs 2015). Compare this to questions about causal linkages between stroke recovery prescriptions and SSRIs, which have begun to be answered, and we can reasonably expect more robust answers in the near future. This leaves us with three categories:

[289] These cases also make demands on a large number of sub disciplines that seem less urgent such as virtual reality technology, edaphology, etc.

(1a) Funding within ‘urgent’ tenacity
(1b) Funding within ‘basic’ tenacity[290]
(2) Funding proliferated alternatives

Furthermore, we can subdivide ‘2’ to make room for various aims of proliferation. Some instances of proliferation are acutely formulated to rival existing research whereas others are so drastically distinct that the relationships between the proliferated theory and the existing theory are difficult to access. For example, we know what a competitor to, say, ‘demand side’ approaches to carbon emission mitigation approaches might look like; maybe they deny the premise that purchase power is causally relevant to supply curves. However, in the long term, we cannot make *any* claims about what is or is not a competitor since we have given up our right to make claims about the future content of proliferation. As such, we can further subdivide our taxonomy into four categories:

(1a) Funding within ‘urgent’ tenacity
(1b) Funding within ‘basic’ tenacity
(2a) Funding proliferated competitors
(2b) Funding proliferated alternatives more broadly

Finally, it is worth pointing out that the ratio between proliferated theories and normal science itself is subject to a ‘time frame’ based analysis. Recall that proliferation is for the sake of maximum criticism; criticism takes *time* to formulate, address, reformulate, and so on. This is to ensure the robustness, so to speak, of our knowledge (cf. Carrier 2017). But robustness at the expense of *expedience* is a value judgment. This means that the ratio of which category we fund will ultimately depend on our (value-laden) conceptions of urgent science.

Recall that, for Kitcher, our choice of what research to fund is ultimately a value decision. I agree with this judgment, but our conceptions of well-ordered science still come out differently. On the Kitcherite view, citizens choose what research is funded without qualification. On my view, the following value judgments must be considered: what research, to what degree of reliability, is needed *urgently*? Without any sense of urgency, the goals of the citizenry are completely irrelevant to funding policy since they will be achieved as offshoots of other research. Once we answer this question, we can begin to consider how this will affect our

[290] I label them 1 ‘a’ and ‘b’ since they lie on a spectrum whereas 2 is a distinctive *kind* of funding altogether.

funding levels of the aforementioned categories. More specifically, we will (roughly) have the following spectrum:

> Urgent science (all funding goes to 1b) ←→ moderately urgent science (funding goes to 1a and 1b) ←→ less urgent science (funding goes to 1a, 1b, and 2a) ←→ Even less urgent science (funding goes to all four categories) ←→ No urgency at all (all funding goes to 2b).

Notice that this spectrum is about our *general* funding policy. *Prima facie*, most funding will fall in category 4 since 2b has *long-term* benefits and our funding should not be too short sighted. But I digress since this presupposes the value questions inherent in the nature of urgent science that I have not analyzed. Furthermore, of course, this spectrum is continuous with greater and lesser funding being adjusted and some boundaries will be context specific depending on the cost-benefit analyses of individual research grants. Regardless, this gives us a rough idea of the kinds of questions that must be answered to have a socially responsible funding mechanism that is consistent with Feyerabend's pluralism.

2.3: Ethical Constraints on Theory Pursuit

Kitcher claims that we have grounds for censoring certain kinds of research on the basis of ethical principles. Most of the debate centers around issues of freedom of expression and the freedom to pursue our own academic interests. In this section, I will forego this discussion and take a closer look at this claim.

Kitcher's worry is, essentially, that the very act of pursuit will be harmful to some group. If we accept the claim that we cannot make long-term projections about the value of proliferation, then Kitcher's constraints are inherently *deontic* since they must be, in principle, agnostic to some of the consequences of pursuing proliferated theories. Therefore, his ethical constraints must be of the form: "We should not pursue theory *x* because of ethical reason *y*."[291] Second, one issue Kitcher does not address and is faintly recognized by political philosophers of science is the degrees of separation, so to speak, of theory pursuit from its consequences. There are many instances, to be sure, where the pursuit of controversial theories have been exploited to justify actions and policies that have caused concrete harms to individuals. While sociobiology is

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[291] It may be the case that consequentialist analyses will be fruitless in this case due to epistemic limitations. See Millgram (2000) and Lenman (2000) for a discussion of this issue more generally.

one example Kitcher champions, a more acute example, I think, comes from David Irving's hypothesis that the Holocaust never existed in the sense that there was no systematic extermination of peoples during the Third Reich. In his libel case against Penguin Books and Deborah Libstadt, there was consideration of using the testimony of Holocaust survivors about their experiences on concentration camps, who were 'Zionist conspirators' according to Irving's hypothesis. This, for obvious reasons, could cause very direct and concrete harms to some individuals even though the hypothesis itself is merely being entertained.[292] But the censorship of sociobiology or holocaust denial is only one way this problem could be avoided; as Feyerabend pointed out, we can use *propaganda*, we can increase scientific literacy,[293] we can criticize applications of scientific knowledge on purely ethical grounds, or we can interpret it instrumentally as Osiander did with the Copernican hypothesis, and so forth. Behind this observation lies a general principle of the ethics of theory pursuit.

Feyerabend broaches this problem indirectly in appendix 3 of AM_{B1}, where he is discussing the cranks. He rejects Lakatos' argument that some standard of rationality is needed to rid us of 'intellectual pollution' but rather, suggests that the problem is best tackled by *education*. He writes:

> We must stop the scientists from taking over education and from teaching as 'fact' and as 'the one true method' whatever the myth of the day happens to be. Agreement with science…should be the result of examination of choice, and *not* a particular way of bringing up children. It seems to me that such a change in education and, as a result, in perspective makes it clear that there are many ways of ordering the world, that the hated constraints of one set of standards may be broken by freely accepting standards of a different kind….Remove the principles, admit the possibility of many different forms of life, an such phenomena will disappear like a bad dream (AM_{B1}, 218-9).

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292 In actuality, Libstadt did not want survivors to go through the process of testifying; there were plenty of volunteers, which changes the ethics of this particular situation. I will sideline this since I am using this example as an illustration.

293 What I have in mind here is teaching the distinction between acceptance and pursuit. In Oreskes fascinating discussions of climate change deniers and tobacco companies trying to erode consensuses and thereby trying to forestall (or totally eliminate) particular lines of action on these issues (cf. Oreskes and Conway 2010), it is assumed that *pursuing* multiple lines of inquiry means there is no consensus of *acceptance*. Indeed, as she rightfully points out, many members of the public make this mistake making it the case that such cases of proliferation are harmful to concrete policy only because of such mistaken preconceptions. On my view, proliferation is not the problem but erroneous understandings of science that can be combated by other means.

While his view of education is extremely underdeveloped,[294] the salient point is clear: pluralism in education is a necessary *prerequisite* for science. This is, as evidence by the last sentence, an empirical claim and therefore may be true or false in different contexts. But the deeper point will sound familiar, since it was invoked in the previous chapter in our acceptance of tacit knowledge: proliferation is ethically justifiable even after particular *sociological* conditions are met. In his own words: "We make sure [that proliferation will be successful] by considering (historical, sociological, physical, psychological, etc.) tendencies and laws which tell us what is possible and what is not possible under the given circumstances and thus separate feasible prescriptions from those which are going to lead into dead ends" (AM_{B3}, 149). This is simply restating Feyerabend's contention that humanitarianism constitutes the foundation of his pluralism: pluralism cannot be implemented if the correct conditions are not met. This makes Kitcher's argument contingently true in non-ideal societies and not a principle of a well-ordered science. So, rather than Kitcher's censorship via ethical principles, we have the conditional claim: "If society is structured in way x, then do not pursue theory y" or, an even more restricted conditional: "If society is structured in way x and cannot be restructured in ways x_1, x_2, etc., then do not pursue theory y" where x_1 and x_2 are societal structures that prevent the harms caused by the pursuit of y.

3. Peer-Review and Methodological Censorship

In this conception of a well-ordered science, I have bracketed the question of how we can go about recognizing the prospective value of research. Recall that I argued that tacit knowledge can be granted a particular role within a Feyerabendian science. Specifically, we can use certain kinds of tacit judgments to alter the direction of tenacity within sufficiently diverse communities. The modern incarnation of the tacit based approach is peer review. Peer-review is often regarded to be the safeguard of the quality of scientific research.[295] Taking anarchism seriously, however, immediately problematizes this statement: what counts as a 'quality' piece of research? In this section, I outline the existing problems with peer-review and survey an alternative. I then show how this alternative is consistent with the Feyerabendian view outlined in the previous chapters.

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[294] A similar conception of a 'liberal education' can be found in Dewey. See Fairfield (2009) for an outline of Dewey's philosophy of education and its influence.

[295] This view of peer-review began around 1900 (Csiszar 2016, 306). When Whewell first proposed the idea to the Royal Society of London in 1831, it was meant to help the public visibility of scientific publications (307).

3.1: Biases and Reliability

Peer-review is often broken down into three distinct assessments: novelty, significance, and methodological soundness (Frank 1996). The National Institute of Health (NIH) also uses these metrics along with the investigator(s) institutional credentials and resources (NIH 2014) and the National Science Foundation (NSF) requires a broader notion of significance which not includes the potential impact on the local discipline but its potential social impact as well (NSF 2012). The most common worry about peer-review has to do with *bias*. There is a massive amount of literature that suggests that peer-review is inherently *conservative* (Gillies 2008; Stanford 2015); it privileges extant standards of 'quality' research over those that are innovative. In fact, regression analyses of 32,546 applications to the NIH showed the independent contribution of 'novelty' was only 1.4 points, in contrast to 6.7 points for 'significance' (Lee 2015, 1275). This was also recognized in a recent panel discussion on CIHR and details some of its consequences:

> Now that the percentage of NIH grant applications that can be funded has fallen from around 30% into the low teens, biomedical scientists are spending far too much of their time writing and revising grant applications and far too little thinking about science and conducting experiments. The low success rates have induced conservative, short-term thinking in applicants, reviewers, and funders... Young investigators are discouraged from departing too far from their postdoctoral work, when they should instead be posing new questions and inventing new approaches. Seasoned investigators are inclined to stick to their tried-and-true formulas for success rather than explore new fields. One manifestation of this shift to short-term thinking is the inflated value that is now accorded to studies that claim a close link to medical practice (Naylor et al. 2015).

Furthermore, the metric for methodological soundness is often contaminated by this conservatism; in surveying 288 NIH reviewers for the 'Director's Pioneer Award Program', "the most innovative projects involved methodological risk" (1277) suggesting that methodological soundness is *antithetical* to innovation.[296] The second worry of bias is that peers will promote their own pet-project at the expense of others due to lack of charity, unwillingness to engage with foreign approaches, lack of knowledge of other approaches and their previous applications, or sheer stubbornness ("researcher narcissism" as Gillies (2014, 8) puts it).

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[296] Similar results were found in ethnographic studies of the European Research Council (Luukkonen 2012).

Still, we may think, peer-review as a whole is a mostly reliable means of excluding cranks and ensuring that only quality research is disseminated and funded. One strategy to measure this reliability is to compare the 'impact'[297] of funded research with its reviews and provide counterfactual analyses on the impact of unfunded research. However, these analyses are highly conjectural and unclear making them quite limited (Dinges 2005). Another strategy is to assess the consistency of peer-review. If peer-review is to be a reliable source of credence, then peers should agree, more often than not, about what to fund.[298] Many empirical studies have supported the view that interrater reliability, across a range of cultures in widely varying disciplines, is below acceptable standards (by any reasonable view of 'acceptable') (Cole et al. 1981; Hargens & Herting 1990; Marsh et al. 2008; Graves et al. 2011). Therefore, peer-review as currently practiced cannot be said to provide a reliable means of determining what research will be successful. It seems as if Kitcher's dismissive view of a science ordered by coin tosses and reading tealeaves (Kitcher 2001, 120) isn't that far off of what peer-review is already doing.

However, as Carole Lee notes, we would expect that interrater reliability would be low *if the reviewers come from different sub-disciplines*.[299] If peer-review is pluralistic, then we would expect inconsistencies. Fortunately, these inconsistencies, often, do not amount to strict contradictions since reviews often focus on different parts of their work. Qualitative research of reviewer comments from 400 reviews of 153 manuscripts across a range of disciplines showed that "comments offered by pairs of reviewers rarely had critical points in common, either in agreement or in disagreement. Instead critiques focused on different facets of the paper" (Lee 2012, 865).[300] The issue of peer-review, therefore, is at the level of the *editor*. Since interrater reliability is low in the sense that peers often provide conflicting *final verdicts* (i.e., accept, accept with minor revisions, reject, etc.), the final verdict must somehow take these two non-

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[297] Impact is often understood bibliometrically by the number of citations in journals with certain estimates of viewership. Some have been developing broader or alternative ways of measuring impact that include use of studies in curriculum, dissemination at conferences, and other qualitative dimensions (e.g., 'influence') (cf. Godin & Doré 2004; Bornmann 2012).

[298] Notice that this method allows for reviewers to disagree with *why* they chose to fund or not fund a project while agreeing on the final verdict. This means we are bracketing the rationality of peer-review and relying on the brute fact that peers decided to fund or not fund a project. This study is thus pertinent to the Polanyi/Toulmin/Lakatosian view of methodology discussed in the previous chapter.

[299] Lee argues that this description of scientific practice is Kuhnian, citing Kuhn's 1977 paper on theory choice. However, Kuhn only expects pluralistic standards to be operative in *revolutionary science*. The view that peer-review is, and should be, pluralistic is much more in line with a Feyerabendian view.

[300] The study comes from Fiske and Fogg (1990).

overlapping reviews and make a decision. But this just leads to a new kind of bias which Lee has called 'commensuration bias' (Lee 2015). This comes from the problem of "mak[ing] interpretative decisions about how to weight the relative importance of quantitatively different peer-review criteria" (Lee 2015, 1273). The worry about stifling innovation, then, comes into play at the editorial level.

Before considering the implications this has for Feyerabend's well-ordered science, it is worth revisiting what was argued in Chapter 3 about peer review. Ultimately, we accepted tacit knowledge within a diverse community when it cannot be formulated in a timely fashion. The former criteria can be operationalized quite easily by the *conclusions* that reviewers reach (accept, reject, etc.) The latter is trickier. Reviewers give *reasons* for their decisions. Are these reasons just psychological shortcuts of tacit knowledge, as Polanyi argued, or are they something to be taken at face value? The Feyerabendian view is to privilege explicit, testable statements over tacit knowledge and, therefore, we shall take reviewer *comments and their validity* at face value. As Lee argued, the editor must be in a position to assess the reasonableness of the reviewer comments and 'add them up', so to speak. This only seems possible, however, given some general level of consensus of what is 'reasonable' which is reflected in the editor's decision. As such, peer review is inherently meant for *normal science* where conservatism is a part of ensuring the 'quality' of research.[301] Proliferation, on the other hand, seems like it would suffer a great deal from the conservatism of peer review. This suggests that peer review best serves the interests of tenacity rather than proliferation.[302]

3.2: Public Review Publications

Peer-review is also extremely costly. Herbert et al. (2013), for instance, estimate that 550 working years went into proposals submitted to the NHMRC in March of 2012. This is on top of the opportunity cost of reviewer hours and the administrative costs of funding bodies. This makes it not obvious that peer-review is better than nothing.[303] Luckily, the current incarnation

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[301] It becomes much more complicated when considering cases of 'micro-proliferations' *within* existing research programs. My initial reaction to this is to suggest that micro-proliferations are cases of non-urgent science and, therefore, peer review should be playing a lesser role in evaluating the prospects of research anyways.

[302] Gillies (2008) argues that peer-review is effective at eliminating 'false positives' (i.e., bad research that gets funded) but it isn't effective at identifying true positives.

[303] I should mention that there are attempts to improve peer-review. For instance, Jayasinghe et al. (2003) have found various markers for referee bias (North American reviewers give higher reviews than Australian reviewers,

of peer review is not the only means of attaining ‘quality research’ nor has it been the only approach historically. When it was first conceived by Whewell, peer review was not intended to determine whether papers should be rejected or not, but a means of producing commentary articles that would accompany the paper in the journal (Csiszar 2016). Recent developments in online publishing archives have garnered an increasing amount of attention for providing ‘informal’ means of dissemination (roughly, the blogosphere). In this section, I will outline this means of disseminating information and analyze it via the position developed in this dissertation.

One locus of this discussion is the physics preprint server arXiv, which has become an exemplar for archive publishing. Archive publishing has been around for decades (cf. Traweek 1988) and has become an increasingly popular means of disseminating information.[304] What is noteworthy, for our current purposes, is the surrogate arXiv, similar archives, and the blogosphere use for excluding proposals. This surrogate is exceptionally weak:

> ArXiv allows only the most limited forms of peer review and editorial control. There are barriers to access, but they are barriers that merely keep out contributors who cannot master basic forms of participation in the community of physicists: people who cannot format their articles in LaTeX, people who have not before published on arXiv or who cannot find an endorser who has, people who do not have an academic email address and people whose contributions do not have the right look and feel, as judged by the moderators (Sismondo 2016, 583).

The first few criteria are entirely pragmatic and indirectly exclude papers with ulterior motives extremely well (e.g., advertisements, self-promotion, etc.). The final criteria is the only one that directly identifies the content of contributions, and moderators are often *laissez faire* with the standards they act on. As such, with the exception of the occasional dictatorial moderator, the form of ‘peer-review’ implemented meets the standards of Polanyi’s exceptionally broad and vague community standards discussed in the previous chapter. However, even this exceptionally loose demarcation criteria has not been without backlash. Consider the case of the ‘string wars’, where basic methodological debates or the boundary work of science has become prevalent (cf. Dawid 2013). Physicists like Peter Woit argue that moderator interference is a form of

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reviewers nominated by the researcher give higher reviews that those nominated by a granting agency, and scientists with fewer proposals tend to give higher ratings). However, these results still only improve reliability measures to 0.47 (the standard threshold for reliability is normally in between 0.8-0.9 (Marsh et al. 2008, 162)). This being said, enough tenacity on this research program may allow us to reformulate and better implement peer-review.

[304] See Gunnarsdóttir (2005) for a discussion of the niches that traditional peer-review publication journals are being relegated to.

censorship “primarily driven by the moderators’ desire to paint as intellectually illegitimate and supress any commentary that is critical of string/M-theory research” (quoted in Ritson 2016, 615). This claim highlights the difficulty of distinguishing between the cranks, in Feyerabend’s terms, and legitimate researchers with unorthodox positions. While there is a great deal of consensus that there *are* cranks (622), there has been little agreement about how to define a crank. The closest available definition has been tacit: “you know who you are” (Jacques Distler, quoted in Ritson 2016, 615); “when you see one, you usually know it’s a crackpot” (Luboš Motl, quoted in Ritson 2016, 618). The only definitions on a market comes from John Baez’s ‘crack-pot index’ which gives points for specific kinds of claims that cranks often make and attributes the status of ‘crackpot’ to those who pass a certain threshold of points[305] and negative definitions.[306]

However, there is a potential way around this conundrum. As Ritson notes, the blogosphere provides a means of *public review*.[307] Far from blog posts and commentary being mere ephemeral phenomena, they allow for ongoing dialogues formulating criticisms and suggested improvements for more polished papers; “the ability of those within and outside the group to read, comment on and challenge blog posts has come to serve as a form of informal peer review” (Riesch and Mendel 2014, 54). Furthermore, since blog discussions have their own means of bibliometric evaluation (Ritson 2016, 610), the blogosphere indirectly provides a way of discerning the relevance of papers to certain topics as well as revealing their potential impact. However, public review does not *censor* material, except indirectly. Papers or data that do not pass peer review, on the other hand, never get a chance to make an impact. Public review still allows for the possibility that ideas that would have failed to pass peer review can be revisited. This provides an alternative to peer review- methods of disseminating information. Now, what would a Feyerabendian say about all of this?

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[305] Some of the more fun claims include “40 points for comparing yourself to Galileo, suggesting that a modern-day Inquisition is hard at work on your case, and so on” and “20 points for naming something after yourself.” Some more methodologically loaded criteria include “50 points for claiming you have a revolutionary theory but giving no concrete testable predictions” or “5 points for using a thought experiment that contradicts the results of a widely accepted real experiment.” See http://math.ucr.edu/home/baez/crackpot.html for the full list.

[306] “A crackpot is not an outsider, a crackpot is not a contrarian or a crackpot is not an individual with an in-principle disagreement” (Ritson 2016, 621). As Ritson correctly notes, “these arguments merely shift the disagreement [where]…there is no consensus opinion on which to fall back” (ibid).

[307] Ritson discusses how the ‘trackback’ feature on arXiv, which allows authors to leave links to blogs (among other online sources) in their contribution. This creates a natural connection between public review and arXiv.

Remember that Feyerabend holds the principle of testability that states that we should attempt to maximize the amount of criticism launched at our theories. It would be unwieldy to expect all criticisms to be addressed,[308] but it seems as though maximal *readability* is an effective way of maximizing *potential* criticism. Furthermore, as Ritson notes, these forms of publication offer a window into 'science in the making' meaning that criticism is not aimed solely at the final product, but at the preliminary stages as well (Ritson 2016, 621).[309] This is also conducive to maximizing the means of *proliferating* and ensuring that peer-review does not methodologically censor even crankish ideas, which Feyerabend argues are perfectly legitimate to consider when put into non-crankish hands. This suggests that public review, for the sake of proliferation at least, is a preferable option on Feyerabendian grounds.

4. Chapter Summary and Future Directions

Before summarizing this chapter, I would like to briefly discuss a possible concrete means of implementing Feyerabend's pluralism at the level of funding policy. While this chapter has focused on the methodological implications Feyerabend's views have for dividing funds, this is far from implementable. Because of this, I would like to briefly overview the view of 'funding by lottery' and briefly assess its strengths and limitations from a Feyerabendian perspective.[310]

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[308] There is some anecdotal evidence that too much information can be overwhelming. Polanyi defends scientists ignoring strange sounding conclusions (his example is an article in *Nature* that concluded that the time of gestation for some mammals will always be multiples of π (see Polanyi (1963, 376)). Naess (1972, 92-4) gives an interesting 'rationalist' method from sorting through the papers of cranks and those with legitimate ideas. Feyerabend indirectly responds to this worry by stating that what information one chooses to skim, or focus on in detail, is completely up to the individual:

> Having listed to one of my anarchistic sermons, Professor Wigner replied: 'But surely, you do not read all the manuscripts which people send you, but you throw most of them into the wastepaper basket.' I most certainly do. 'Anything goes' does not mean that I shall read every single paper that has been written – God forbid! – it means that I make my selection in a highly individual and idiosyncratic way, partly because I can't be bothered to read what doesn't interest me…partly because I am convinced that Mankind, and even Science, will profit from everyone doing his own thing…There are of course so-called 'thinkers' who subdivide their mail in exactly the same way, come rain, come sunshine, and who also imitate each other's principles of choice - but we shall hardly admire them for their uniformity, and we shall certainly not think their behavior 'rational': Science needs people who are adaptive and inventive, not rigid imitators of 'established' behavioral patterns (AM_{B1}, 215).

Furthermore, some empirical studies suggest that ignoring information is often done *fruitfully* by 'outsiders' (or younger less well-read 'insiders') (cf. Stephan 1994, fn. 41 1220). Zollman's (2010) model comes to the same conclusions, though there is still systematic ways of ignoring particular kinds of information.

[309] Ritson additionally argues that this enhances science's publicity.

[310] Another alternative, which has been approved by the Parliament in the Netherlands in June 2016, is a 'peer to peer' method of distributing grants where scientists self-select who will get grants with certain conditions attached

The funding by lottery method allocates funds randomly within an established community. The method is not entirely novel. Francis Edgeworth (1888, 1890), for instance, when discussing grading criteria, suggests introducing 'chance elements' into more fine-grained determinations. The argument behind this is that graders can reliably make coarse-grained distinctions between grades, but these abilities become less reliable at finer grains. Modern adaptations of this view in the context of funding policy (see Avin 2015a, §6.2.1), use a 'border zone' approach, with the following features:

(1) There are coarse-grained borders which establish various cut-off rates.
(2) Cut-off rates distinguish between fundable proposals, not-fundable proposals, and those within the range of fine-grained decision-making.
(3) Those with the fine-grained spectrum are each given an equal number of lottery tickets that can be used to determine which of those proposals get funded.
(4) Since the notion of 'fine-grained' versus 'coarse-grained' is context-sensitive, the degree of separation between cut-off points is context-sensitive.[311]

This view still requires peer-review, but it gives it much less authority. Peer-review can only determine the crankish proposals and the bona fide good proposals. Without going too deep into this model, I will spend some time discussing the Feyerabendian basis of this method.

The introduction of random chance is essentially a cost-effective method of recognizing Feyerabend's worries about projecting future successes: it can't be done reliably, especially in cases of proliferation. This is consistent with the view established in Chapter 3 where we do not even attempt to order proliferation 'rationally' at early stages in their development. Rather, we proliferate wildly and diversely and see what happens. As such, funding by lottery seems appropriate in the early stages of proliferation. However, a noted problem with the lottery method is the problem of *continuous funding*. As Avin (2015b, 115-6) notes, we are, probabilistically speaking, increasingly unlikely to fund the same project repeatedly. The principle of tenacity demands that continuous funding is reliably available. Additionally, determining which proliferations are ready to be tenaciously pursued is a different kind of judgment that determining what to proliferate. In the former case, we can begin making shorter-

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such that those grants get re-distributed until they are spread throughout the community. I will not discuss this alternative here (see Bollen et al. 2017 for a description of the model assumptions of the method).

[311] One notable critic of this system is Barbara Goodwin (2005). Her primary criticism, that doesn't concern considerations of justice, is as follows: Since some candidates will be close to the cut-off points, there is a good chance that those slightly above or below a border zone have been placed there wrongfully. This point can be repeated until all border zone collapse until all projects are admitted into the lottery.

term projections about the future fruitfulness of a theory and begin making funding decisions on different grounds that randomly choosing which proliferated theories should be invested in. As such, a funding by lottery mechanism may not be best suited to cases of normal science and is, therefore, limited from a Feyerabendian perspective.

In this chapter, I have outlined the idea of a well-ordered science and some of the details of structural features of scientific communities that order of science. I have also attempted to show which structural methods of organizing would be compatible with a Feyerabendian science and made criticisms, from a Feyerabendian perspective, on proposals that are conflict with such a science. Despite these efforts, there remains hosts of questions that need to be answered before this proposal can be put into practice. However, I hope to have provided the first steps in understanding how Feyerabend’s anarchism can contribute to scientific progress.

Concluding Remarks

> "I want to defend society and its inhabitants from all ideologies, science included. All ideologies must be seen in perspective, one must not take them too seriously. One must read them like fairtytales which have lots of interesting things to say but which also contain wicked lies" (Feyerabend 1975b, 181).

This dissertation has implicitly applied the principles that were outlined within it. For instance, Feyerabend argues that many ideas throughout history are abandoned before their fecundity has been fully appreciated; one of the contentions in this thesis is that Feyerabend's pluralism and its implications haven't been fully appreciated. The thesis can also be read as the proliferation of a 'new' conception of science which can now proceed to show its worth by being reinjected into contemporary debates and seeing what results. However, we now must tenaciously develop Feyerabend's pluralism to show how it may contribute to the growth of philosophical and scientific knowledge. While the second half of this dissertation attempted to do just this by critically engaging Feyerabend's pluralism and analyzing its implications for funding policy, I would like to conclude this dissertation by pointing towards avenues for future research.

As previously mentioned, Feyerabend's pluralism makes substantive assumptions in many areas of philosophy and empirical assumptions about the nature of us as inquirers. As such, it would be silly to think that a full rehabilitation of Feyerabend's pluralism would be possible within a single dissertation. However, I have demonstrated that Feyerabend's theoretical pluralism was abandoned prematurely as it was never well understood. I have also demonstrated how its application can lead to new discoveries in contemporary discussions about the logic of pursuit and models of resource allocation. In these concluding remarks, I will continue to motivate future interest in Feyerabend's philosophy in three main ways. First, I will try to address criticisms that may reasonably be leveled against the position outlined in this dissertation. These responses will be, and can only be, preliminary to more thorough discussions in future research. Second, I will show ways in which Feyerabend's account may be extended or supplemented. This will point to future areas of collaborative research. Finally, I will highlight some points of contract between Feyerabend's pluralism and contemporary accounts.

1. Problems and Responses

Feyerabend was, ironically, a systematic thinker. While this term normally has monistic connotations, in Feyerabend's case, trying to understand the nature and implications of a radical pluralism occupied his entire career. My thesis has focused on his pluralism in the context of scientific theories and methods, but he eventually extended this view to cultures, forms of life, and, more broadly, anything that captured his interest. In his more confident moments, he explicitly states this much:

> It would seem to me that the task of philosophy, or of any enterprise interested in the advance rather than the embalming of knowledge, is to encourage the development of such new modes of approach, to participate in their improvement rather than to waste time in showing, what is obvious anyway, that they are different from the status quo (Feyerabend 1963c, 175).

Feyerabend was a systematic thinker in the sense that he applied his pluralism to every intellectual venture he encountered. As a result of the breadth of Feyerabend's philosophical ambitions, his pluralism is subject to critical onslaughts from many different arenas of thought. I have already outlined his responses to the prominent criticisms he faced, but there are innumerably more that could be launched. In this section, I will begin this task by suggesting a number of problem areas that remain to be examined in Feyerabend's pluralism and gesture at possible responses.

1.1: The Self-Refutation Problem

Worries about foundationalism go back to ancient Greek and Roman philosophy. Agrippa's trilemma famously postulates that all foundationalist views ultimately depend on an infinite regress, since any proof requires a further proof and so forth, a dogma that is taken as unquestionable, or will be circular.[312] Popper gives his own answer to this problem:

> The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of *dogmas*, but only in so far as we may desist from justifying them by further arguments (or by further tests). But this kind of

[312] Agrippa actually describes five tropes including 'discrepancy' (i.e., extant disagreements) and 'relativism' (see Diogenes 1925, Book IX, sec. 88-9). However, these tropes are what *lead to* the problem of foundationalism rather than problems for foundationalism itself (see Bailey 1990, 28-9).

> dogmatism is innocuous since, should the need arise, these statements can easily be tested further (Popper 1935, 87).

This answer is compatible with Feyerabend's view that we should subject every feature of scientific practice to criticism. However, Popper never tackled the corollary question: What about applying this anti-foundationalism to methodology as well?[313] While this question seems unfair since falsificationism is adopted by dogma as well, admitting this would be devastating for Popper since he cannot, then, *rationally persuade anyone* to adopt falsificationism which was, quite clearly, his intention. Watkins summarizes this conundrum quite nicely:

> a rationalist…exposes himself to a taunt of *tu quoque* from an irrationalist: the latter can retort to this kind of rationalist: you object to my committing myself to my fundamental position by an unreasoned act of faith; but you too do this, except that you do it surreptitiously whereas I do it openly (Watkins 1969, 58).

This is the self-refutation objection in a nutshell: any view of methodology will ultimately be unable to ground itself and, therefore, will be unable to rationally coopt others into accepting it. Feyerabend took this worry more seriously than Popper did but still, to my knowledge, never gave a satisfactory answer.[314] The goal of this section is to show how Feyerabend can address this worry.

Alan Bailey summarizes Feyerabend's predicament. He writes:

> The global sceptic about rational justification is engaged in attacking our customary view that some beliefs and actions can be rationally justified…But the attempt to offer reasons [for this attack] would appear to be completely self-defeating. If these putative reasons are indeed good reasons, then they will merely provide an illustration of the thesis that some beliefs can be justified rationally…Thus it seems to follow that the argumentation employed by the global sceptic must be wholly incapable of providing any genuine support for his criticism (Bailey 1990, 27).

Bailey goes on to say that Feyerabend "most closely conforms to the role and strategy of Sextus' Pyrrhonism" (40). Sextus, on Bailey's interpretation, avoids the problem of self-refutation by merely *using* rational arguments that rationalists would be forced to address because of *their* belief in rational argumentation. Therefore, Feyerabend and the Pyrrhonian sceptic only give

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[313] Bartley's 'comprehensive critical rationalism' attempts to address this by subjecting the tenets of falsificationism to criticism themselves (Bartley 1962). I will not discuss this attempt here (cf. Watkins 1971 for a criticism and Rowbottom 2011, Chapter 1 for a historical overview).

[314] In a sense, Feyerabend's later work on cultural relativism can be seen as a posited solution to the self-refutation objection. However, this analysis lies outside the scope of this thesis.

"chains of thought that very much look like rational arguments" (38). Without engaging in any comparison of Sextus and Feyerabend,[315] it is worth seeing if Feyerabend's pluralism has the resources to avoid the self-refutation objection.

Bailey, like most authors of the secondary literature, interprets Feyerabend as holding anarchism as the conclusion of a *reductio* and not a positive position in its own right. Since I reject this interpretation,[316] I cannot help myself to Sextus's escape from the problem. However, I think another path may be available. If the foundations of Feyerabend's pluralism are arbitrary, then he must bite the bullet by taking the 'dogma' route out of Agrippa's trilemma. Feyerabend recognizes that most[317] inquiry starts with some set of assumptions. However, they aren't 'foundations' in any strong sense since there is nothing forbidding criticizing them or experimenting with inconsistent alternatives. Rational or not, each alternative is tolerated to be explored. But these pseudo-foundations, for Feyerabend, are still *appraised*; we should use many inconsistent dogmas rather than resting on a particular dogma. Put another way, we can accept the arbitrariness of particular dogmas for the sake of progress, but what justifies the appraisal of the distribution of dogmas? How can Feyerabend avoid this meta-level of arbitrariness? Or, should we take Feyerabend at his word that his view may be accepted or rejected "depending on mood, the weather, etc." (Feyerabend 1976, 203).

Recall that for Feyerabend, anarchism is a prerequisite for *any* kind of progress and not just progress conceived in a particular way.[318] However, as I also argued, 'any' actually means 'any *reasonable* definition of progress'; namely, definitions that presuppose the principles of fallibilism and testability.[319] This leaves the global skeptic enough room to attempt to

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[315] Feyerabend rejects the comparison. He writes "*[e]pistemological anarchism* differs from skepticism…While the skeptic either regards every thesis as equally good, or equally bad, or desists from making such judgments altogether, the epistemological anarchist has no compunction in defending the most trite, or the most outrageous thesis" (AM_{B1}, 189). However, this statement seems a bit naïve since a Pyrrohnian skeptic uses Aenesidemus's ten tropes to defend 'trite and outrageous theses', and Feyerabend is committed to being *ultimately* agnostic about the truth of any claim or else his fallibilism would make no sense. See Neto 1991 for further discussion.
[316] I have implicitly rejected this reading in my reconstruction. I provide the argument for this in my (2017).
[317] Feyerabend recognizes that some inquiry begins without a well-defined problem, but from irrelevant activities or guided by a 'vague passion' (AM_{B1}, 26). However, the appraisal of such activities remains within the scope of normative methodology.
[318] Bailey claims that Feyerabend holds a Popperian notion of progress (40-1). As discussed in the previous chapter, Feyerabend never holds a clear notion of what constitutes progress.
[319] The definitions of progress given in Feyerabend's day (increasing generality, increasing explanatory scope, greater predictive abilities, increased puzzle solving, etc.) are, perhaps unsurprisingly, fairly continuous with more present day notions (see Mizrahi 2013).

methodologically collapse anarchism by maintaining some ‘unreasonable’ notion of progress by dogma and showing the limits of ‘reasonable standards.’ I think Feyerabend simply has to bite the bullet here. While this strategy seems unsatisfactory, Feyerabend doesn’t actually have to give very much up. Remember, Agrippa’s trilemma is motivated by *existent discrepancy*. While there is certainly disagreement about what progress is, nearly everyone agrees in one disjunct of Feyerabend’s *disjunctive* notion of progress. As such, there is little to motivate the global skeptics attempt to show that Feyerabend’s position is self-refuting. While the global skeptic may reply that with enough tenacity, they could make their ‘unreasonable standard’ reasonable, thus forcing the problem of discrepancy on Feyerabend. This, however, merely shows that anarchistic pluralism is defeasible via *proliferation*; something Feyerabend would happily accept.[320] Ironically, then, Feyerabend’s pluralism contains the seeds of its own potential destruction.

A proper appraisal of Feyerabend’s position has to be based on a proper understanding of the role of arbitrariness in his philosophy. Remember, ‘arbitrary’ is not identical with ‘unquestionable’; we can always criticise arbitrary actions and they remain foundational only in a Pickwickian sense (cf. Popper 1970). The attractiveness of Feyerabend’s views depend on the generality of his promises for which his pluralism is conducive.

1.2: Feyerabend’s Conception of HPS

In the first chapter, I introduced Feyerabend’s conjecture that for any thesis of scientific methodology, there exist historical counterexamples to it. These counterexamples, Feyerabend claims, are not a result of insufficient knowledge, but were essential to progress. However, I also showed how Feyerabend held the view that history evolves in an inherently unpredictable manner. These two claims seem to be inconsistent since the former argument requires a *counterfactual* argument whereas the latter presumes that no such argument can be provided. Feyerabend’s conception of HPS, therefore, appears to be inconsistent. It is the goal of this section to suggest a remedy for this situation.

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[320] It is important to note that skepticism does not play a substantive role in Sextus’ philosophy of *action*. While skeptics *ultimately* holds no beliefs, they do hold ‘beliefs’ in the more deflated sense (e.g., “I am thirsty so I will drink this water”) (Sextus 1990, 20-25) cf. Morrison (2011) for a discussion of Sextus’ views of everyday beliefs. Feyerabend’s views on action, however, are less obvious.

Feyerabend has shown, if his historical reconstructions are correct, that rationalism *was* violated and that progress ensued as a result. This is certainly a problem for rationalism since it is meant to provide *exclusive* rules for theory pursuit. This historical criticism, therefore, is sufficient for showing that rationalism is not the only means of making progress. However, perhaps a rationalist may respond that following their rules may be more *efficient* or *reliable*. While this would be a major concession, this requires being able to compare how progress actually ensued with how it would have ensued if a different set of rules were followed. This response is not available if the unpredictability claim is accepted but, then, neither is Feyerabend's contention that such violations were *essential* for progress. In other words, this debate is about the modal properties of certain features of theory pursuit. The argument: "For methodological theory *M*, there exists counterexamples *x*, *y*…*n*" only goes through if we can discuss counterfactual possibilities of theory pursuit in a sensible and plausible manner.

What, then, are we to make of the unpredictability thesis? We have seen that if it is wedded to the counterexample thesis then we can only conclude that rationalism qua exclusive thesis, in a strict sense, is false. If it isn't, Feyerabend can regain his claim that such violations were essential for progress. But I don't think that the unpredictability thesis solely has the function of modifying the counterexample thesis. Rather, as we saw in the latter half of this dissertation, it plays a crucial role in Feyerabend's own positive philosophy. Specifically, predicting the future of theories cannot be used to constrain proliferation and is necessary for the indefiniteness of tenacity. This both suggests that Feyerabend actually held the unpredictability thesis and that it should be seen as a separate claim from the counterexample thesis. While Feyerabend did not seem to have recognized this tension himself, it appears to not strike too deep into his conception of HPS.

1.3: Where Is the World?

Harry Collins famously claimed that the "natural world has a small or non-existent role in the construction of scientific knowledge" (Collins 1981, 3). After reading Feyerabend, one could easily get the sense that Feyerabend believes this as well and has implicitly adopted a kind of solipsism or succumbed to the 'problem of the empirical basis' (cf. Haack 1991). After all, the principle of tenacity presumes that *any* theory can be pursued indefinitely even if it has no empirical content. Feyerabend discusses this matter later in his career, where he reincorporates the external world into his methodology. In his 1989, Feyerabend defends what Tambolo (2014, 203) calls ‘the resistance thesis’:

> I do not assert that any [form of life] will lead to a well-articulated and livable world. The material humans…face must be approached in the right way. It *offers resistance*; some constructions (some incipient cultures - cargo cults, for example) find no point of attack in it and simply collapse (Feyerabend 1989, 405).[321]

The world resists our attempts to live in it according to certain theories. We cannot claim *anything* about nature and expect it to perform as expected.[322] While Feyerabend never makes this claim explicitly in his mature or early career, he never denounces it either.[323] However, what is pertinent is *what methodological consequences follow* from the resistance thesis. The difficulty with this comes when the resistance thesis is combined with what Tambolo calls the ‘pliability thesis’: the world is “more pliable than is commonly assumed” (145). More specifically, as Matt Brown puts it, the world “doesn’t disclose a single, coherent description of the world, but a plurality of overlapping perspectives… which are all perspectives on the same world, but don’t add up to an absolute view of the world” (Brown 2009, 219). The world does not resist us to uniquely force any particular theory. Let’s take a closer look at this claim.

If we recall Feyerabend’s early writings on the pragmatic theory of meaning, we see that a languages characteristic does not determine a its meaning. The characteristic would be determined solely by the world.[324] Therefore, while the resistance thesis does not uniquely determine *theories* it does determine *the characteristic*. This means that any theory must present *some* interpretation of the characteristic that has been established. However, causally isolating the utterance-phenomena relation requires theoretical assumptions and therefore the characteristic itself is theory dependent. Denying the theory could mean denying the legitimacy of the characteristic (the utterances could be artifacts). Even if we could somehow fix the

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[321] Psillos (2017) defends a similar view where metaphysical realism simply implies that there can be a gap between what we know and what there is and, therefore, is a source of fallibilism.

[322] The debate between Dawkins and Bloor parallels this worry. Briefly put, Dawkins (amongst others) think that our ability to fly, thanks to particular theories, refutes any sort of ‘anything goes’ story; “show me a cultural relativist at thirty thousand feet”, Dawkins writes, “and I’ll show you a hypocrite” (quoted in Bloor 2008, 13). Bloor’s response, which is along the lines that I offer via Feyerabend here, is that this is actually an attack on a kind of naïve idealism whereby we can live in a dream world and expect it to behave according to our wishes (Bloor 2008, 17-20).

[323] The closest Feyerabend comes to this claim in his earlier career is when he writes that “[e]mpiricism, insofar as it goes beyond the invitation not to forget considering observations, is….an unreasonable doctrine” (1969c, 134-5) suggesting that experience of the external world must be *somewhere* in scientific reasoning.

[324] Remember, being a sentence on Feyerabend’s account requires that its utterance is causally dependent on the phenomena of interest.

characteristic, the principle of tenacity allows us to continue theory pursuit without satisfying this constraint immediately. In the second block quote just offered, Feyerabend suggests that theories like this will simply 'disappear.' But this just means that its proponents lack a certain degree of tenacity. Because of this, surprisingly, the resistance thesis does not seem to have any substantive methodological implications.

1.4: On the Discovery/Justification Distinction

One response to Feyerabend that could be made is that his pluralism is simply an acknowledgement of what was commonly known: 'anything goes' in the context of discovery. What matters is how theories are to be justified. Perhaps it is the case that Feyerabend is making the age-old mistake of thinking that hallucinating benzene rings has implications for the logical relationship between evidence and theories. This understanding of the distinction between the contexts of discovery and justification often goes back to Reichenbach:

> Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes (Reichenbach 1938, 5).

Popper endorses the same distinction as does Feigl (1970, 4), Salmon (1970, 68-72), Scheffler (1967, 69-73) and Siegel (1980, 299-304).[325] As Reichenbach and Popper make exceptionally clear, the context of justification is the bearer of normative content and where methodological reasoning is formulated. In chapter 14 of AM_{B1}, Feyerabend responds to this worry:

> Inventing theories…we often make moves that are forbidden by methodological rules. For example, we interpret the evidence so that it fits our fanciful ideas, we eliminate difficulties by *ad hoc* procedures, we push them aside, or we simply refuse to take them seriously. The activities which according to Feigl belong to the context of discovery are, therefore, not just *different* from what goes on in justification, *they are in conflict with it* (AM_{B1}, 167).

This shows there are no distinct moments in history which are 'justification' contexts or those that are 'discovery' contexts. Since normative methodologies must be applicable at *some* moment in history, "they can and must be used *here and now*" (145), they must isolate particular

[325] See Hoyningen-Huene (2006b) for some differences between them.

moments when we can *apply* a 'logic of justification.' But how do we know these instances aren't just tentative stages of research that will be overcome? As Feyerabend writes,

> In most cases…our methodologies project all the various elements of science and the different historical strata they occupy on to one and the same plane, and proceed at once to render comparative judgments. This is like arranging a fight between an infant and a grown man, and announcing triumphantly, what is obvious anyways, that the man is going to win (147).

This point can also be seen in light of the principle of tenacity. Whatever the results of a 'logic of justification' may be (i.e., a theory has low degrees of evidential support, particular postulates are better supported than others, etc.), we can always continue to develop our theories in whatever way we choose. The logic of justification is *subservient* to the logic of pursuit.

There are other ways of raising a similar complaint against Feyerabend. One of Feigl's criticisms of Feyerabend's conflation between the two distinctions is that whatever 'products' appear in the context of justification could have come about in many different ways (Feigl 1974, 2). As such, there can be no logic of pursuit since pursuit is itself a multifaceted phenomenon whereas justification is not. Ultimately, Feyerabend accepts this view since proliferation is unconstrained; but this is a hard fought philosophical discovery suggesting that his view is far from trivial. Another way of conceiving of the discovery/justification distinction is that results from the context of justification provide us with evidence for what to *accept* rather than what to *pursue*. This, roughly, is Grünbaum's position (Grünbaum 1973, 86).[326] This objection would be fair if Feyerabend was concerned with the *acceptability* of scientific theories. However, he is interested in the conditions of *pursuit*. Additionally, as Feyerabend has made obvious, it is not the case that the logic of pursuit is merely an empirical question to be explored by psychology and sociology exclusively. It is, partly, a *methodological* question; when should we pursue or abandon theories? Is the pursuit of theories problem solving or is it the competition of theories? Articulating a logic of pursuit is a perfectly respectable philosophical task of the utmost importance.

2. Extensions and Supplementation

[326] The principle of tenacity is operative here as well: when we believe that a theory (or proposition) is *true* we are not merely claiming that the 'evidence' to-date supports the theory, but that it is *genuinely a true description of the world*. This means we must *guess future states of research* and hope that future sciences will be consistent with our current set of 'knowledge.' See Lakatos (1978a, 220-1) for further discussion.

There are many details of Feyerabend's pluralism that have not been filled in and many points of contact between Feyerabend's pluralism and other views that have not been made explicit. In this section, I consider ways in which Feyerabend's pluralism can be fleshed out in a more detailed manner and how it may contribute to other discussions.

2.1: Maximizing Serendipity

In AM, Feyerabend repeatedly stresses the importance of luck in theory pursuit. He writes:

> [T]he actual development of institutions, ideas, practices, and so on, often [starts from] some irrelevant activity, such as playing, which, as a side effect, leads to developments which later on can be interpreted as solutions to unrealized problems… if we do exclude them, will this not considerably reduce the number of our adaptive reactions and the quality of our learning process? (AM_B, 175-6).

We are often engaging in a great deal of guesswork about what research will be conducive to what kinds of progress. Contemporary research supports this claim by detailing the roles *serendipity* plays in scientific discovery (Roberts 1989; García 2009).[327] This research details the ways in which discoveries were made by accident. In some cases, this means that we discovered something that we did not expect to discover. For instance, Brownian motion was originally observed when Brown was seeking to understand the fertilization of pollen grains of *Clarkia pulchella* but became a significant discovery for thermodynamics. In other cases, we attained a discovery by means that were completely serendipitous. Consider Goodyear's discovery of the vulcanization of rubber:

> The actual discovery was accidental—Goodyear had not planned to heat the rubber compound he was working with when it (accidentally) came into contact with a hot stove. However, he was *in general* looking for a method to enable rubber to withstand the cold, and such a method was revealed by his mistake… Goodyear was looking for just such a solution, but found it in an unexpected place (Copeland 2017, 9).

In a sense, this follows from Feyerabend's historical conjecture that science develops unpredictably since we cannot predict accidents. This way of describing the situation makes it seem as if luck is beyond our control and, therefore, something we must simply hope for. But

[327] Kuhn recognized this feature of discovery as well, which he called a "type [of discovery] that occurs more frequently than the impersonal standards of scientific reporting allow us easily to realize" (Kuhn 1962, 57). See also his analysis of the discovery of X-rays (57-8).

this is not necessarily the case. As Pasteur famously remarked, luck favors the prepared mind. In other words, the *realization* that one has luckily stumbled up a significant discovery depends on a mind capable of recognizing the discovery itself.[328] I think that a general framework can be provided, which follows naturally from Feyerabend's pluralism, which makes sense of this aspect of theory pursuit. Perhaps ironically, this framework comes from one of Feyerabend's heroes: Ernst Mach.[329]

Mach recognizes the importance of serendipity in theory pursuit: "the most important inventions are brought to man's notice accidentally and in ways that are beyond his foresight" (Mach 1896, 166). He supports this by providing many toy examples and episodes from the history of science. However, these discoveries required a "capacity to profit by experience" (ibid). This capacity, conveniently enough, is best developed through *pluralism*. Rather than a mind with "a powerfully developed mechanical memory, which recalls vividly and faithfully old situations" (167) (i.e., a Kuhnian scientist), we require a mind that is "excit[ed] by mutual contact of widely different trains of ideas" (ibid). It is a feature of one's mental capacities, not the world itself, which can transform an anomaly into a discovery or a discovery into a different kind of discovery. This view is obviously compatible with Feyerabend's ethics, since it requires that individuals familiarize themselves with many perspectives, and, therefore, can be seen as supplementing Feyerabend's pluralism. However, we can see how this kind of pluralism, which Feyerabend contends is causally connected to theoretical pluralism, can maximize our chances of lucky discoveries. Some suggest that accidental discoveries are quite common ("35.2% of all the anticancer drugs now in clinical use were discovered by serendipity" (Hargrave-Thomas et al. 2012, 6)) while others suggest that they are more infrequent (Jeste et al. 1979). Given the ambiguities of what counts as an 'accidental discovery' and the lack of attention to this issue, we are far from having any comprehensive account of how important accidental discoveries are across the sciences. However, this does not diminish the fact that having a 'prepared mind', an indirect result of theoretical pluralism, remains an important addendum to Feyerabend's pluralism.

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[328] To be clear, it is often not the case that a scientist recognizes the significance of a discovery *immediately*. This often requires retrospection. As Nickles writes: "What we retrospectively interpret as revolutionary breakthroughs typically begin life as rather normal work. Over time, by telescoping historical development, scientists whiggishly invest these charmed cases with far more meaning than they originally possessed" (Nickles 1997, 128–129).

[329] See Lenox (1985) for a discussion of the education necessary for maximizing serendipity.

2.2: A New Kind of Inductive Risk

The argument from inductive risk posits that since we are always uncertain about the validity of a theory, we are always taking on some degree of risk when relying on that theory for practical purposes (Douglas 2000). Since risk is value-laden concept, the reliability of a theory is always, at least partially, a value-based decision. As has been pointed out, this can roughly be characterized by saying that the acceptance of a theory is only fully determinate when combined with value judgments (Steel 2010). In fact, the majority of the literature of inductive risk addresses the question of what to accept (or rely on).[330] In this section, I want to consider the idea that Feyerabend's account of theory pursuit has provided us with a new kind of inductive risk.

Recall that, for Feyerabend, history evolves in unpredictable ways. What knowledge we accept as genuine knowledge changes as history evolves. Additionally, his principle of maximal testability demands that we do our best to ensure that our knowledge changes by relentlessly challenging what we think we know. So not only is it the case that we *may* change our beliefs, but we should try our best to *force* a change of beliefs. This prompts the following question: how can we accept theories knowing that they will likely change in possibly substantive ways in the future? In other words, we seem to be taking a certain amount of *risk* when we accept a theory not just because theories are fallible, but because theories simply are reflections of tentative stages of research that we expect will be outgrown. Before analyzing whether this constitutes a *new* kind of inductive risk, it is worth seeing what assumptions this problem rests on.

As should be clear by now, the unpredictability thesis makes it such that we cannot be confident that our future states of knowledge will be compatible with our current knowledge. However, this kind of inductive risk also presupposes that *realism* is true; that accepting a theory means accepting it as a literally true depiction of the world. However, our *reliance* on theories may not require this realist assumption for a couple of different reasons. First, many incompatible theories can be compatible with the facts of what is *reliable*. Our theories of

[330] Often the question of what to accept (i.e., believe) is conflated of the question of what to use (cf. Barseghyan 2015, 30-42). However, 'belief' is often cashed out in behavioristic terms making this conflation relatively harmless.

aeronautics have changed massively over the past century, but the fact remains: planes fly![331] More generally speaking, a great deal of applied science is, at least somewhat, theory free in the sense that theories may change but the ability to manipulate the world stays the same (or is only modified in minor respects) (cf. Stokes 1997, Chapter 1). While future knowledge may qualify the scope of current practical knowledge or improve its specificity, on this picture, it doesn't *undermine* it. This suggests that we can be instrumentalists about our theories (i.e., they lead to or are consistent with kinds of action) for the sake of acceptance and, therefore, we are simply left with traditional forms of inductive risk.

While appealing, I think Feyerabend has given us several reasons to be suspicious of this line of thought. Recall that Feyerabend's pluralism is not simply true for *theories* but *values* as well. Our theoretical discoveries often make dramatic changes in our values. Think of the transformation the Copernican revolution led to how humans understood their place in the cosmos and similar transformations from evolutionary biology, research on consciousness, and so on *ad nauseum*. This leads us to believe that Feyerabend's pluralism does pose a new kind of inductive risk: theoretical discoveries can undermine the values from which we accept theories even if they don't undermine the 'practical knowledge' itself. Second, not all practical knowledge is always clear cut; even the fact that 'planes fly' is not clear cut depending on our standards (e.g., the distinction between gliding and flying, whether flying presupposes safe landings, whether flying presupposes maintaining certain air speeds or air time, etc.). Can we reliably remove methane emissions from the ozone? Methods for doing so are very difficult to evaluate, for a number of different reasons (e.g., detecting methane concentrations, carbon diffusion, etc.). Regardless, we can easily imagine changes to our theoretical knowledge changing our ability to evaluate its effectiveness, thereby potentially undermining our practical knowledge. Finally, the *significance* of our knowledge changes as our theories change. The correlation of serotonin levels to depressive episodes is not of great importance if we discover that serotonin does not have the direct link that we think that it does. Our judgments of inductive risk will change when, say, we prescribe SSRIs when we change our views on the causal processes underlying depression. For these three reasons, we can submit that theory change provokes inductive risk even at the level of practical knowledge.

[331] The semantic point that what it means to say that "planes fly" is theory dependent seems moot, here, since sitting on a plane and expecting it to arrive is not a linguistic act.

Finally, it is worth considering whether this is a new kind of inductive risk, or a new way of repeating the same old problem. The problem of inductive risk comes from the fallibility of our theories. However, this fallibility is normally cashed out in terms of *inductive* fallibility; our theories are only probably true and, therefore, we have to take on a certain degree of risk that the theory may lead us astray. However, the kind of inductive risk that follows from Feyerabend's thinking is quite different since it stems from a different kind of uncertainty. This point makes sense when we consider the epistemology of Donald Rumsfeld:

> Reports that say that something hasn't happened are always interesting to me, because as we know, there are known knowns; there are things we know we know. We also know there are known unknowns; that is to say we know there are some things we do not know. But there are also unknown unknowns – the ones we don't know we don't know. And if one looks throughout the history of our country and other free countries, it is the latter category that tend to be the difficult ones (U.S. Department of Defense 2002).

Traditional conceptions of inductive risk focus on the risk of 'known knowns.' Some philosophers have focused on the risk of 'known unknowns' by considering the risks inherent in 'complex sciences' (e.g., climate science) (Steele 2014). What Feyerabend's thought broaches, is how to understand the 'unknown unknowns.' For example, if we were to discover that conscious pain continues after brain death, this would have implications for how we treat these patients. Since we have no reason, if we take Feyerabend seriously, to forbid this from potentially being true, our decision-making *now* presupposes risks about what we may discover in the future. On this picture, we are constantly taking risks that we don't even recognize *are* risks (since the recognition requires future unexpected knowledge). Without going into detail, these are clearly distinct problems even though they both stem from a kind of fallibilism. It because of this that I think it is fair to suspect that Feyerabend has paved the way for thinking about a new kind of inductive risk.

2.3: The No-Alternatives Argument and Unconceived Alternatives

There are several contemporary positions that presuppose theoretical pluralism. Two specific views include the 'No Alternatives Argument' (NAA) and Kyle Stanford's 'Problem of Unconceived Alternatives' (PUA). In the former case, the presence or lack thereof of alternative theories affects the confirmation status of particular theories. This suggests that the number of alternative theories, of a specific kind, is required for a full evaluation of the confirmation of a theory (Dawid et al. 2015; Dawid 2017). In the latter case, the presence of alternative theories undermines the explanatory defense of realism (Stanford 2006). While I will not assess the coherence of these positions here (cf. Chall (2018) for a criticism of NAA and Chakravartty (2008) for a critical discussion of PUA), it is worth showing how Feyerabend may contribute to these discussions.

Feyerabend's theoretical pluralism depends on the act of *proliferation*, not on the mere *existence* of theoretical pluralism. Theoretical pluralism is an achievement. This seemingly obvious point gets missed out too easily when philosophers merely comment on scientific practice and derive their views from *extant* practices. Consider Dawid's claim: "The fact that scientists don't find alternatives to a theory at hand can be explained by the hypothesis that there are few or no possible alternatives to that theory" (Dawid 2017, 4). This, strictly speaking, is either incomplete or false. This 'fact' only shows that proliferation has not happened successfully. An additional argument must be given that proliferation would have failed but, if we take Feyerabend seriously, we cannot make this argument in advance. At best, we can provide contingent reasons why proliferation was constrained (e.g., limited resources) which means that the probability assigned will only be true because of contingent circumstances of theory pursuit. Herzberg puts this point quite provocatively:

> Via the notion of an 'acceptable' alternative theory, sociological (and human cognitive) factors play a pivotal role in the No-Alternatives Argument. But whenever merely contingent historical and sociological factors may preclude scientific reflection about certain otherwise viable alternatives to a dominating hypothesis H, it becomes absurd to count the absence of such reflection as *evidence* for H (Herzberg 2014, 381).

While the 'absurd' claim may be too strong, the NAA does require an assessment of the conditions of proliferation to show whether the probability assigned is the result of a genuine lack of alternatives or an artificial lack of alternatives brought on by needlessly conservative practices. The same is true of PUA. In fact, Stanford appears to have recognized this point implicitly. In his defense of PUA from the objection that science has become better organized throughout history and, thereby, minimized the problem of PUA, he rightly attempts to show how proliferation has been thwarted by various structural features of scientific communities (Stanford 2015). While 'proliferation' is not mentioned here, it is implicit that the possibility of proliferation undergirds the real threat of the PUA.

Theoretical pluralism, and the extent to which it is possible, always depends on theories that were proliferated and developed at earlier stages. This makes views that depend on theoretical pluralism or, conversely, views that depend on the absence of theoretical pluralism dependent on the limits of proliferation. As a result, Feyerabend's principle of proliferation is of direct interest to proponents of the NAA and PUA.

3. Pluralism: Then and Now

Pluralism has become an exceptionally hot topic within the philosophy of science and philosophy of the sciences. The literature on pluralism is, perhaps not accidentally, extremely scattered with many kinds of pluralism addressing many different topics. In a sense, this dissertation adds to this confusion by reviving an additional kind of pluralism. To remedy this, I will begin to put Feyerabend's pluralism in dialogue with contemporary formulations of pluralism and see what points of contrast emerge.

3.1: Pluralism: the Middle Path?

Many philosophers construe pluralism as a 'middle ground' between two extreme positions: relativism and realism (Mitchell 2003; Giere 2006; Wimsatt 2007). Relativism, ironically enough, is routinely associated with Feyerabend's position (Ghiselin 1987, 135-136; Wimsatt 2007, 147; Mitchell 2009, 108) and dismissed out-of-hand for being clearly untenable. On the other end of the spectrum, there is 'realism', which may better be called 'monism', or the view that for each domain there is one and only one true description of the entities or processes within that domain. A defensible pluralism, some contend, retains from relativism the view that there can be multiple inconsistent and *true* descriptions within a domain and, from realism, that these descriptions genuinely latch on to features of reality. The realism is justified abductively (the descriptions are constrained on evidentiary grounds) and the 'relativism' is justified by some conception of what scientific theories or models 'are.' It is worth taking a moment to see how this pluralism fares in light of the Feyerabendian insights garnered in this dissertation.

First, it should be mentioned that it is often unclear what the normative implications of this kind of pluralism are meant to be. As is often the case, the normative content is *implied* but not spelled out explicitly. If the normative content cannot be spelled out, then this kind of pluralism may end up being a mere 'commentary' on science at some particular stage in its development. While this is not a criticism, per se, it does suggest an important avenue for future elaboration of these positions.[332] Second, the primary opponent of this kind of pluralism is often considered to be reductionism. But on a Feyerabendian analysis, it isn't clear in what sense they are 'opposed.' While it may be granted that the scientific practices these pluralists describe are not 'reductionist', we can still pursue reductionist research programs and use them to point to identify problems with anti-reductionist programs, formulate new tests, and so forth. Philosophers could join in these tasks and defend reductionism if they wish. It may even be the case that they *should* if we follow Feyerabend's sophist motto that we should make "the weaker case the stronger and thereby retain the motion of the whole" (AM_{B3}, 22). Once we look at things in this manner, the opposition between pluralism and reductionism seems to be of little consequence.

This analysis is, in actuality, somewhat of a non-sequitur since Feyerabend's pluralism is in the context of *pursuit* whereas these other pluralists seem to be more interested in the case of *acceptance*. As such, we need to know what implications pursuit has for acceptance before we can fully understand the implications Feyerabend's pluralism has for these contemporary discussions. At this point, I think what a Feyerabendian can contribute to this discussion is to show the limits of what normative consequences can be drawn from such a pluralism. As such, its contribution is largely negative by delimiting what we can learn from this particular kind of pluralism though it points to some important aspects that require closer attention than they are currently being given.

3.2: Pluralism and the End of Science

In the introduction to *Scientific Pluralism*, Kellert et al. synthesize several versions of pluralism into what they call 'moderate pluralism' (Kellert et al. 2006, xi-xii) On this view, pluralism is valuable only insofar as it is expedient towards achieving the age-old unity of science ideal. This is an old view. Hegel and Peirce were both pluralists, in a sense, but thought that the end of inquiry would yield a unified account. This contrasts with 'radical pluralism' which states that, even at the 'end of science', science will ultimately be disunified (xiii). The

[332] Giere claims that his position is solely descriptive (Giere 2006, 88). Mitchell, in 2003, claims that her position is only solely descriptive (Mitchell 2003, xiii) but then in 2009, claims that her position does have normative implications (Mitchell 2009, ix) which is close to what Wimsatt suggests of his position (Wimsatt 2007, 5). However, these remarks are often made in passing and more detail is required on this note.

justification for this, normally, comes from a metaphysical view (cf. Dupré 1993; Waters 2017). Some maintain that emergent phenomena (e.g., consciousness, organisms, ecosystems, etc.) cannot be explained by reducing them to more basic, fundamental elements of nature. Others maintain that theories are and can only be partial representations of nature. Therefore, a complete representation of nature will inevitably require multiple theories (Giere 2006). Others defend a 'dappled world' approach where the world is constituted by patchworks of local facts with no general laws (Cartwright 1999). This explores a topic Feyerabend never discusses: the end of inquiry. However, his view does have direct implications for this way of thinking.

Recall that, for Feyerabend, we are in the process of articulating and applying conceptions of normative methodology. As pointed out, any methodology must have a "point of attack" in historically situated material. They must be able to make recommendations "here and now" (AM_{B1}, 145). Let's assume that inquiry actually does have an end and it does not continue indefinitely. How could we ever identify 'the end of inquiry' once it has happened? For Feyerabend, we proliferate whatever contradicts theories in place. Whatever the theoretical state of affairs of the end of inquiry would be, Feyerabend maintains that we should proliferate against it. Presumably, if we are at the end of inquiry, and Feyerabend's pluralism is in the context of pursuit, we should stop proliferating and, at best, maintain a healthy amount of diversity to keep our critical capacities alive. This makes it clear that, if Feyerabend's pluralism is mistaken, we have to be able to not only *recognize* the end of science but predict that the end of science will continue to remain stable in the future. How could such an assumption be justified? Most likely, through the same reasoning these philosophers have established their characterization of the end of science (e.g., metaphysical reasoning). But, surely, this reasoning itself is fallible and so it could be shown wrong via proliferation. This all suggests that the very idea of characterizing the end of science and suggesting that such an end has normative implications, requires a hypothetical commitment to infalliblism. This assumption, I believe, no philosopher would want to be committed to. If this assumption is to be avoided, proponents of such views must meet provide an answer to the Feyerabendian question: how do we know we are at the end of inquiry, once it has arrived?

Final Remarks

I hope that these concluding remarks have provided some sense of what a future engagement with Feyerabend's pluralism might amount to. While these discussions are too quick to be considered definite, I hope they will at least provoke interest in taking Feyerabend's pluralism seriously in these dialogues. After all, this is treating Feyerabend's position in a manner that is consistent with itself: it treats it *fallibly* and as an approach that will demonstrate its merits when interacting with other positions. While I personally remain convinced that there is something deeply correct about Feyerabend's philosophy, and believe that the argumentation in this dissertation should encourage acceptance of the conclusions reached therein, this should dissuade not us from abandoning the spirit underlying this dissertation; a spirit of critical openness, humility, and a fundamentally forward looking orientation.

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