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How Can a Taxonomy of Stances Help Clarify Classical Debates on Scientific Change?

Hakob Barseghyan [1,*] and Jamie Shaw [2]

1 IHPST, University of Toronto, Toronto, ON M5S 1K7, Canada
2 Department of Philosophy, Western University, London, ON N6A 5B8, Canada; jshaw222@uwo.ca
* Correspondence: hakob.barseghyan@utoronto.ca; Tel.: +1-647-801-0055

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Abstract: In this paper, we demonstrate how a systematic *taxonomy of stances* can help elucidate two classic debates of the historical turn—the Lakatos–Feyerabend debate concerning theory rejection and the Feyerabend–Kuhn debate about pluralism during normal science. We contend that Kuhn, Feyerabend, and Lakatos were often talking at cross-purposes due to the lack of an agreed upon taxonomy of stances. Specifically, we provide three distinct stances that scientists take towards theories: *acceptance* of a theory as the best available description of its domain, *use* of a theory in practical applications, and *pursuit* (elaboration) of a theory. We argue that in the Lakatos–Feyerabend debate, Lakatos was concerned with *acceptance* whereas Feyerabend was mainly concerned with *pursuit*. Additionally, we show how Feyerabend and Kuhn's debate on the role of pluralism/monism in normal science involved a crucial conflation of all three stances. Finally, we outline a few general lessons concerning the process of scientific change.

Keywords: Feyerabend; Kuhn; Lakatos; stances; acceptance; use; pursuit

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1. Preamble

The historical turn was one of the most exciting periods of 20th century philosophy of science. This excitement was largely generated by Kuhn's *The Structure of Scientific Revolutions*, which prompted intense debates about the foundations of scientific rationality, the importance of the history of science to philosophy of science, and the conception of philosophy of science itself. However, at least some of these debates were never satisfactorily resolved. Two debates which we focus on in this paper are the Lakatos–Feyerabend debate concerning theory rejection and the Feyerabend–Kuhn debate about pluralism during normal science. In this paper, we contend that these debates were not properly resolved partly because Feyerabend, Kuhn, and Lakatos conflated distinct *stances* that scientists can take towards theories. By providing a *taxonomy of stances*, we show how these debates could have been resolved.

The paper is structured in the following way. In the first section, we provide a motivation for a well-defined taxonomy of stances. In the second section, we distinguish between three stances—*acceptance*, *use*, and *pursuit*. In the third section, we show how this taxonomy would have helped clarify the Feyerabend–Lakatos and Feyerabend–Kuhn debates. We conclude by drawing three important lessons on how science changes through time.

2. Why Do We Need a Taxonomy of Stances?

There are two primary motivations for constructing an agreed upon taxonomy of stances. The first is pragmatic: it ensures that we can communicate effectively. Along with this is the more distinctly philosophical motivation: it ensures that we refrain from equivocating distinct stances and amplifying conceptual confusions. While this seems obvious, there has been a great deal of terminological confusion during and since the historical turn about the kinds of stances scientists can and have taken towards theories. This complaint was made back in 1968 by Yehoshua Bar-Hillel:

> Of the contributors to the discussions on our symposium ... who in their talks or discussion remarks referred ... to 'acceptance' and its rules, none gave much indication that he regarded the notion of acceptance as being in need of clarification. I regard this as remarkable and rather disappointing, since it was so clear to me ... that the term 'accept' (as well as, of course, 'reject') is highly ambiguous both in our ordinary speech and in our informal scientific discourse ... Without this preliminary clarification, any discussion of the rationality, justifiability, or 'logicality' of acceptance is doomed to futility. [1] (pp. 150–151).

Some historians have also indicated that there are many stances one can take towards a theory. In 1975, Robert Westman pointed out that we often assign different meanings to "acceptance":

> Acceptance may connote provisional use of certain hypotheses (without commitment to truth content), or acceptance of certain parts of the theory as true while rejecting other propositions as false, ... or acceptance of the theory as true without regarding it as a program for further research. [2] (p. 165).

Despite this plea, few have bothered to develop an explicit taxonomy of stances. One notable exception is Larry Laudan, who distinguished between "the context of acceptance" and "the context of pursuit" as early as 1977 [3] (pp. 108–114). For Laudan, these are two "different contexts within which theories and research traditions are evaluated" [3] (p. 108). The importance of distinguishing between different cognitive stances was also emphasized by Stephen Wykstra in 1980:

> To commit oneself to working on a theory is one sort of cognitive stance; to take the theory for granted in testing other theories is another; ... and to use the theory to put men on the moon, yet something else. [4] (p. 216).

Among those who have attempted to draw similar distinctions are Laurie Anne Whitt [5] and Molly Kao [6].

However, despite these efforts, we often see philosophers and historians using terms like *accept*, *commit*, *pursue*, *engage*, *embrace*, and so on in crucial turning points in their arguments and fail to define them explicitly. For example, consider this typical fragment from *The Cambridge History of Science*:

> Ultimately, it was Newton's work that *commanded the greatest scientific authority* throughout most of Europe by around the middle of the century. But even though in the field of celestial mechanics and, to a lesser degree, terrestrial mechanics Newton largely *reigned supreme*, in other areas of science—notably those based on experiment—his *shadow fell* more lightly. In France the experimental sciences proceeded without any *firm commitment* to Newtonian concepts. The *equivocal attitude* of Leonhard Euler (1707–1783) toward Newton's theory of light—ranging from an overtly anti-Newtonian *rejection* of the particle theory of light to a subsequent *debt* to elements of Newtonian mechanics in formulating his wave theory of light—is another instance of the partial and provisional character of the Newtonian *hegemony* of the second half of the eighteenth century. [7] (our italics).

This fragment alone offers a plethora of obscure stances that scientists took towards various theories (see *italics*). Yet, it is never clarified whether these are distinct stances, or whether some of them are just different names for the same stance.

This lack of clarity has certainly led to a great deal of confusion since different stances have distinct methodological and practical implications. The equivocation of stances affects both historians and philosophers of science. Whether one's interest is in reconstructing the belief systems of past communities or making sense of philosophical debates, one cannot proceed without a systematic taxonomy of stances. As we shall see in the following section, the stances we shall focus on, *acceptance*, *use*, and *pursuit*, have different significance and lead to different historiographic perspectives. It is for these reasons that we should clearly define and separate different stances.

3. A Prospective Taxonomy

At the very minimum, it is important to distinguish between *acceptance*, *use*, and *pursuit* as distinct stances that a community or an individual scientist can take towards a theory.

We define *acceptance* in the following way: A theory is said to be accepted by a community when the community takes it as the best available description of its respective domain. To accept a theory does not mean that scientists believe that a theory is *absolutely* true. It is true that historically, many communities took some of their theories to be infallible truths about the universe; consider, for instance, the stance of the medieval community of natural philosophers towards the principles of Aristotelian physics or the stance of the 19th century physics community towards the principles of Newtonian mechanics. However, this is by no means a necessary condition for acceptance. To say that a theory is accepted by a community is to say that the community takes the theory to be the *best* among the extant descriptions of whatever these theories purport to describe. It is in this sense that we nowadays *accept general relativity:* we believe it provides the best extant description of gravity and spacetime. Likewise, we accept the *modern evolutionary synthesis* as the best available description of the process of biological evolution.

Acceptance, in this technical sense, is to be distinguished from *use*. A theory is said to be *used* when it is taken to be an adequate tool for practical application. There are many ways a theory may be useful; it may be useful in all sorts of engineering, informing policy, or serving as a pedagogical tool[1]. Clearly, a theory does not need to be accepted to be useful. Electrical engineers, for example, still use Ohm's law because of its relative simplicity compared to Maxwell's equations. Similarly, we can think of many theories that have never been accepted by a community but are still considered useful by that same community. For example, moxibustion has been used in some cases of breeched pregnancies [8], though its theoretical foundations—that the body has meridian points, or that there are yang deficiencies—are not accepted in Western medicine. The case of Ptolemy's astronomy provides another example. The accepted view on the structure of the cosmos circa 1400 was the so-called three-orb theory which suggested that each planet is enclosed in an *epicycle* carried within an *eccentric orb* situated within a *concentric orb*; the latter orb was believed to have its geometric center at the center of the universe. While this three-orb theory was the generally accepted compromise between the Aristotelian theory of concentric crystalline spheres and Ptolemy's theory of epicycles, eccentrics, and equants, it was rarely used for making actual astronomical calculations. When it came to astronomical predictions of the positions of celestial bodies and composing ephemeris for the use in horology, astrology, medicine, and navigation, astronomers used more complex versions of Ptolemaic astronomy where each planet was carried by *several* epicycles. While not generally accepted, these more complex astronomical theories were considered much more useful for making predictions [9,10]; [11] (pp. 267–270).

Acceptance and *use* should also be differentiated from *pursuit*. A theory is said to be *pursued* if it is considered worthy of further development. To consider a theory worthy of pursuit amounts to believing that it is reasonable to work on its elaboration, on applying it to other relevant phenomena, on reformulating some of its tenets, and so forth. It is often the case that a theory is not accepted, there is no practical use for it, and yet it is considered worthy of further elaboration. A good example of this are string theories, none of which are currently accepted or used by the physics community, but are considered worthy of pursuit as many physicists see great promise in them[2]. It is also often the case that a theory remains pursued for a great deal of time before it gets accepted. For instance, the heliocentric astronomy was pursued by several notable individuals throughout the 17th century

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1 Albeit very interesting, the task of differentiating between subtypes of *use* is beyond the scope of this paper.

2 While it is certainly true that some individual scientists *accept* one version of string theory or another, no version of string theory is currently accepted by the physics *community* at large. From the communal perspective, string theories are merely pursued, not accepted.

before it became accepted towards the end of that century. As a stance, *pursuit* has been extremely underappreciated in philosophy of science. Peirce highlighted that we always have many theories in front of us, and the question of which one to *pursue* is distinct from the question of which one to *accept*. He 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. [12] (p. 602)[3].

As Lakatos, Kuhn, and Feyerabend emphasized, theories are (almost) always 'born refuted' in the sense that, at the initial stages of pursuit, theories are rarely acceptable or useful. Furthermore, Lakatos recognized that pursuing a theory does not necessarily require accepting it [17] (p. 220). He points out that Maxwell pursued his kinetic theory without accepting it. The same is true of Planck in the early days of quantum theory. Similarly, Poincaré denied accepting the atomic hypothesis (pre-Perrin) but did not deny its potential to become accepted at some later point in history [18] (p. 152)[4]. It is quite clear that, both historically and logically, *pursuit* should not be equivocated with *acceptance* or *use*.

Each of these three stances also has its opposite stance. The opposite of *used* is *unused* when we do not think a theory is useful in a specific application, we say it is useless in that respect and as a result it remains *unused*. The opposite of *pursuit* is *neglect*: when nobody works on elaborating a theory, we say it is neglected. The opposite of *acceptance* is *unacceptance*: when we do not think that a theory is the best available description of its object, we say the theory is *unaccepted*. We suggest differentiating *unacceptance* and *rejection*. In our terminology, *rejection* is a special case of *unacceptance*: in order to be rejected, a theory needs to have been previously accepted. In contrast, a theory can remain unaccepted without ever being accepted in the first place. Take, for example, the M-theory: it is currently unaccepted by the physics community, but we cannot say that it was rejected, since it has never been accepted to begin with. Phlogiston theory, on the other hand, a once-accepted chemical theory, was rejected more than two centuries ago and is currently unaccepted. Our proposed taxonomy ends up as follows (Table 1):

Table 1. The suggested taxonomy of stances.

	Yes	**No**
Is a theory taken as the best available description of its domain?	Accepted	Unaccepted
Is a theory considered useful in practical applications?	Used	Unused
Is a theory considered worthy of further development, elaboration?	Pursued	Neglected

A few clarifications are in order. First, we need to appreciate that these stances are *not* mutually exclusive: it is quite possible to accept, use, and pursue the same theory. An obvious example is the contemporary stance on evolutionary biology, or the medieval stance on the humoral theory. Second, the stances are logically independent from one another as they do not *necessarily* presuppose one another. While there have been many instances of the same theory being accepted, used, and pursued at the same time, these stances have also often been taken independently of one another. In other words, it is possible to accept a theory without using or pursuing it, or using it without accepting or pursuing it, etc. Third, we do not think that these three stances exhaust the space of possible stances one may take towards a theory. There may very well be other types of stances that scientists can take towards theories (e.g., a theory is considered *scientific*, a theory is considered a *contender*, etc.).

[3] The difference between *acceptance* and *pursuit* is also recognized by Hanson [13] (p. 85), Achinstein [14], and Brown [15] (pp. 90–91). As for the distinction between *acceptance* and *use*, it is emphasized by Wykstra [4] (p. 216) and is also implicit in Bunge's *Treatise* [16] (p. 114).

[4] In fact, after Perrin's experiment, Poincaré triumphantly announces the reality of atoms and, thereby, proclaims that the atomic hypothesis should be accepted [19].

We merely argue that, at minimum, it is important to distinguish between these three stances, as they featured prominently, albeit often tacitly, throughout the history of the philosophy of science.

It is also important to note that scientists may take different stances towards different *parts* of a theory. It is possible to accept only some parts of theories, use some other parts in practice, and pursue others. Quantum physics provides a number of great examples of this phenomenon. While some propositions of the theory are nowadays both accepted and used by the physics community, others are merely used without being accepted. For instance, the physics community clearly accepts the standard model of particle physics as the best available description of certain aspects of the microworld (quarks, leptons, bosons). One great indication of this is the fact the recent discovery of the Higgs boson was clearly portrayed as a discovery of a new type of particle that actually exists in the world. Compare this with the current communal stance towards the collapse postulate: there is generally no agreement in the physics community as to whether the collapse of the wave function describes any real physical process. Thus, albeit useful, the idea of the collapse is far from accepted.

In addition, it is vital to keep in mind that our taxonomy of stances merely attempts to capture what scientists *actually* do (descriptive), not what they *ought to* do (normative). Whether one can *legitimately* accept a theory as the best description of anything is an important normative question that is in the heart of the debate between scientific realists and anti-realists (for a thorough discussion, see [20]). While essential for epistemology, the normative question of legitimacy of a given stance is irrelevant to our task. Our task is descriptive: we want to clarify what different stances are actually taken by scientists towards theories. It is a historical fact that some theories have been *accepted* as correct descriptions of their respective domains, some have been treated as *useful* calculating devices, and some have been considered worthy of *pursuit*. Whether scientists are epistemologically justified to accept anything, or whether they should merely use their theories is an important normative question, but its outcome is irrelevant to the task of describing the actual stances scientists take towards theories[5].

Finally, and most importantly, the definitions of these stances are not to be confused with the *requirements* that different communities employ to decide whether or not any of these stances are to be taken towards a given theory. For instance, the definition of *acceptance* does not specify how exactly different communities come to accept different theories. Many philosophers have attempted to outline a set of requirements that a theory should satisfy *in order to become* accepted. For instance, when Duhem argued that a theory ought to recover extant experimental laws [25] (p. 220), he outlined a certain *method* of evaluating a theory's acceptability, i.e., for deciding whether the theory is to be considered the best among competing theories. Similarly, for logical positivists, a theory would be acceptable if it was the most probable given the available evidence. For Popper, a theory would be acceptable if it had corroborated excess empirical content over its predecessor [26] (pp. 226–330). For Lakatos, a research programme would be acceptable if it was more progressive than its rival programmes [27]. For Laudan, a research tradition would be more acceptable if it solved more empirical and conceptual problems than its rivals [3]. All these methodological dicta attempted to prescribe conditions that a theory should satisfy in order to *become* accepted as the best available description of its object. This goes for any attempt to formulate rules for theory acceptance.

It is safe to say that the acceptance or unacceptance of a theory by a community is decided by the specific methods of theory evaluation employed by that community at the time of the assessment[6]. Naturally, different communities can employ different methods of theory evaluation. It is also clear that these methods can change through time; the method of theory acceptance employed by a community nowadays may or may not be the same as it was ten years ago[7]. Our definition merely states what

[5] Both Van Fraassen and Cartwright seem to miss this point when they conflate *acceptance* and *use* [21] (pp. 4, 151, 197); [22] (p. 2). For critical discussion, see [23] and [24] (pp. 34–40).

[6] See [28].

[7] See [24] (pp. 129–132, 217–225) and references therein.

it *means* to say that a theory is *accepted*; it is not to be confused with the criteria scientists employ to decide the acceptability of a theory. The same goes for *use* and *pursuit*.

The taxonomy we have developed does not merely capture the actual stances that communities take towards theories, but also the stances that implicitly or explicitly were of central concern for most of the thinkers of the historical turn. In the next section, we will see how this taxonomy would have been helpful in clarifying the nature of some prominent debates during the historical turn.

4. Application of the Taxonomy to Two Classical Debates

Despite the fact that thinkers during the historical turn sometimes had a sense that there is a variety of stances that one can take towards a theory, they failed to properly articulate these distinct stances. A careful reading of the works of Kuhn, Lakatos, and Feyerabend reveals many cases of equivocation of two or more of these stances; these conflations often resulted in conceptual confusion and mutual misunderstanding. In this section, we elucidate confusions that arose in two major debates: the Lakatos–Feyerabend debate on the criteria for abandoning theories and the Kuhn–Feyerabend debate on pluralism during normal science. In our discussion of both cases, we will first present the debates within their original terminology and will then show how they could be clarified by applying the taxonomy of *acceptance*, *use*, and *pursuit*.

4.1. Lakatos and Feyerabend on Theory Rejection

Just before Lakatos died, he and Feyerabend were working on a dialogue about whether or not normative methodology could provide universal criteria for theory appraisal[8]. Lakatos would argue that such criteria were, in principle, possible, while Feyerabend would argue that they were not. In their correspondence, the crux of their disagreement appeared to surround their answers to the question of the possibility of rational criteria for *abandoning* a theory. However, as we show in this section, Feyerabend and Lakatos were actually providing answers to two distinct questions, because they were essentially talking about two different stances—*acceptance* and *pursuit*. More specifically, they were concerned with the opposites of these stances—*unacceptance* and *neglect*. But let us start by presenting the debate as it was conceived by Lakatos and Feyerabend.

For Lakatos, the unit of methodological appraisal is *research programmes* defined as sequences of theories sharing a certain fixed hard core. Rationality, for Lakatos, is concerned exclusively with the ways in which research programmes are *modified*[9]. Since there are many ways in which a research programme can be modified, it is important for Lakatos's methodology to articulate which types of modifications are progressive and which are regressive. According to Lakatos, a modification to a research programme is progressive if all of the following conditions are met [27] (pp. 32–34); [30] (p. 112, fn. 2):

1. The modification increases the empirical content of the theory; i.e., it introduces excess empirical content.
2. At least some of this excess empirical content is corroborated in experiments and/or observations.
3. The modification is consistent with the positive heuristic of the programme.

Consequently, if any of these conditions are *not* satisfied, the modification is considered regressive (ad hoc)—ad hoc$_1$, ad hoc$_2$, and ad hoc$_3$ respectively—and the programme is considered degenerating.

Since, for Lakatos, any methodology is "characterized by rules governing the (scientific) *acceptance* and *rejection* of theories or research programs" [30] (p. 103), his methodology is also supposed to provide criteria for theory rejection as well as "rules for the 'elimination' of whole research

[8] Lakatos shifts the unit of appraisal from *theories* to *research programmes* or series of theories. Feyerabend's definition of *theory* is explicitly broad enough to include the notion of *research programmes* [29] (p. 203, fn. 2).

[9] Lakatos credits Popper with this insight [17] (p. 221).

programmes" [30] (p. 112)[10]. According to Lakatos, "within a research programme, a theory can only be eliminated by a better theory, that is, by one which has excess empirical content over its predecessors, some of which is subsequently confirmed" [30] (p. 112). As for eliminating a whole research programme, Lakatos says that it can be eliminated (or 'shelved') only if it is superseded by another programme that "progressively explains more" [30] (p. 112). But because the overall progressiveness of a research programme can only be evaluated after a series of modifications to the programme, according to Lakatos, there can be no such thing as instant rationality—there is no single event that can force the rejection of the whole programme. After all, "research programmes may get out of degenerating troughs" [27] (p. 77). Thus, "one must realise that one's opponent, even if lagging badly behind, may still stage a comeback. No advantage for one side can ever be regarded as absolutely conclusive" [30] (p. 113). As a result, according to Lakatos, scientists "may rationally stick to a degenerating programme until it is overtaken by a rival *and even after*" [30] (p. 117). However, says Lakatos almost in passing, scientists who opt to stick to a degenerating programme "can do this mostly only in private"; while degenerating research programmes can still be rationally pursued, we should withhold public support of such scientists: "editors of scientific journals should refuse to publish their papers . . . Research foundations, too, should refuse money" [30] (p. 117).

Feyerabend's criticism targets Lakatos' criteria for rejecting research programmes. According to Feyerabend, Lakatos's standards "have practical force only if they are combined with a *time limit*" [31] (p. 215), after which a degenerating research programme must be abandoned; this is where Lakatos's methodology fails to provide any substantive criteria of theory rejection. Indeed, says Feyerabend, if Lakatos's standards allow one to stick to a degenerating programme even *after* it is superseded by a rival programme, they are merely "a verbal ornament, as a memorial to happier times when it was still thought possible to run a complex and catastrophic business like science by a few simple and "rational" rules" [32] (p. 78).

He points out that in Lakatos's methodology, "one cannot rationally criticize a scientist who sticks to a degenerating programme and there is no rational way of showing that his actions are unreasonable" [33] (p. 121). Thus, Lakatos's methodological rules merely "describe the situation in which a scientist may find itself. They do not yet advise him how to proceed" [33] (p. 121). He says that Lakatos's "standards do not proscribe, or forbid any particular action, while they are perfectly compatible with the "anything goes" of the anarchist who is therefore right in regarding them as mere embroideries" [33] (p. 124). After all, in Lakatos's methodology, "one can do whatever one wants to do if occasionally one remembers (or merely recites?) The standards which" do not actually forbid anything [33], (p. 139, fn. 38). Since Lakatos's methodology cannot, on its own grounds, provide rules for the acceptance or rejection of research programmes, it is a chimera. Thus, Feyerabend argues, Lakatos is nothing but an anarchist in disguise [32] (p. 77)[11]. This aspect of the debate between Feyerabend and Lakatos is nicely reconstructed by Motterlini:

> *Paul*: Are you saying that if a research programme is judged better than a rival one, scientists *ought* to work on the allegedly superior one?
>
> *Imre*: . . . I am giving you criteria for progress and stagnation within a programme, and the rules for the 'elimination' of entire programmes. Should a programme explain in a more progressive way more than a rival programme accounts for . . . then . . . the rival one may be 'rejected' or simply 'shelved'. You cannot at this point fail to understand what the *pragmatic* meaning of 'rejecting' a programme is: very simply, it means the decision to *cease working on it*.

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10 Note that the terminology of this fragment is still that of Lakatos. We are not yet applying our taxonomy.
11 Musgrave agrees with this interpretation [34] (p. 478). See [35] for a discussion.

> *Paul*: OK, but it is easy to see that standards of your kind have practical force only if combined with some *time limit* after which to keep working on a degenerating programme would be 'irrational'. If you accept the idea of the time limit, then unfortunately, arguments very similar to the ones you used against naïve falsificationism backfire against your own standards. Consider that if it is unwise to reject faulty theories the moment they are born because they might grow and improve, then it is also unwise to reject research programmes on a downward trend because they might recover . . .
>
> *Imre*: Don't get me wrong here. My methodology . . . has no intention of handing out *advice* to the scientist on how to arrive at good theories or on which of two rival theories he should work on . . .
>
> *Paul*: And yet, at the beginning, the bold project of "the *logic* of scientific discovery" was aimed at describing those rules which govern the *acceptance* and *rejection* of scientific theories . . . What then is the point of laying down the rules which may be either followed or ignored? . . . Your *standards* are only *verbal ornaments*.
>
> *Imre*: There is freedom ('anarchy', if you like) in choosing which programme to work on, but the products *must be judged.* You are conflating *methodological appraisal* of a research programme with *heuristic advice* on what to do. [36] (pp. 3–4).

While Motterlini does an admirable job extracting the gist of the Lakatos–Feyerabend debate, it is safe to say that Lakatos and Feyerabend themselves did not quite manage to understand each other. We argue that their misunderstanding was mostly due to the lack of a taxonomy of stances and the resulting conflation of *acceptance* and *pursuit*.

With this new taxonomy at hand, we can now clearly see that, at times, Lakatos and Feyerabend were talking past each other as they were addressing two distinct questions. While Lakatos's main focus was on theory *acceptance* and *rejection*, Feyerabend was talking about theory *pursuit* and *neglect*. For Lakatos, the main question was "what makes a theory or a research programme accepted as the best available description of its domain?" In contrast, Feyerabend's question was "when can a theory or a research programme be legitimately *pursued*"?

Thus, the main point of Feyerabend's criticism is that Lakatos does not provide any standards for deciding when one has to cease *pursuing* a theory (i.e., when one has to start neglecting a theory). This criticism stems from Feyerabend's tacit assumption that theories of rationality must provide criteria for *pursuing* or *neglecting* theories. Feyerabend is never explicit about this, but throughout his entire career he interpreted methodologies as attempts to provide grounds for which theories are to be pursued. For example, when he rejects the *consistency condition*—the idea that one should pursue theories consistent with previously established theories—he does so to allow for the *pursuit* of incommensurable theories [37]. Even in his first paper as a graduate student, Feyerabend argues that we must *pursue* theories that violate Schrödinger's *anschaulich* criteria to attain more comprehensive theories [38]. It is not surprising, therefore, that Feyerabend accused Lakatos of self-contradiction. He read Lakatos as saying that there are rules for choosing which programme to *pursue* but, at the same time, we are free to *pursue* whatever programme we please [29] (pp. 215–216).

Lakatos attempted to deflect the criticism by noting Feyerabend's inadvertent conflation of *methodological appraisal* of a research programme with *heuristic advice* on how scientists have to proceed. But the idea of keeping score on what theory is the best without providing any guidance on what theory to work on struck Feyerabend as completely pointless. This was partially due to Feyerabend's assumption that methodologies *are* supposed to lay down criteria of pursuit, and partially due to Lakatos's choice of terminology—after all, one could argue, what is the point of a methodological appraisal if it does not yield any practical advice whatsoever? We think that a clearer language of *acceptance* and *pursuit* would help to see the difference between two distinct issues.

What Lakatos was trying to say is that, there are certain criteria that help "keep score" on which of the competing research programmes is currently the best description of its domain, i.e., which of the two is to be *accepted*: to determine which programme to accept, we should look at the respective track-records of different research programmes and see which one is more progressive. Yet, for Lakatos, these rules do not tell us which research programme is worth *pursuing* (developing, elaborating). Moreover, Lakatos argues that we should not impose any limitations on which of the competitors are to be *pursued*; in fact, one may rationally *pursue* a degenerating programme even after it is no longer *accepted* [30] (p. 117), for it is not easy, if not impossible, to tell from the outset which idea is capable of growing into an acceptable theory. He agrees with Feyerabend that degenerating programmes can and have often made glorious comebacks[12]. Lakatos's position is that we should allow different competing programmes to be elaborated, but it does not mean that we should not keep score as to which one is currently the best description of its domain; "*the scores of the rival sides ... must be recorded and publicly displayed at all times*" [30] (p. 113). To use our taxonomy, this amounts to saying that there is freedom in choosing which idea to *pursue*, but the results must be judged and the best extant programme is to be *accepted*.

It would have undoubtedly helped if Lakatos had emphasized that the main reason we should keep the score is to know which of the competitors is to be *accepted* as the best available description of the domain. But because Lakatos did not sufficiently emphasize this important point, and because Feyerabend had a tacit assumption that methodologies are supposed to instruct us on theory *pursuit*, Feyerabend ended up mistakenly believing that Lakatos had provided *no* rational rules whatsoever. On the contrary, Lakatos did actually formulate rules for *acceptance*, but was unable to provide a response that was right at his fingertips.

Lakatos has clearly contributed to the confusion with his cursory advice to withhold public support of degenerating research programmes. Recall his remarks that a degenerating research programme should be pursued "mostly only in private" [30] (p. 117). Contrary to his own official decision to refrain from giving heuristic advice, here Lakatos seems to touch upon *pursuit*. Indeed, if one advises to withdraw public support of a certain research programme, then perhaps one has some idea which programmes *are* and which programmes *are not* pursuit-worthy after all. Surely, Lakatos would still insist that scientists are free to pursue *any* research programme whatsoever. However, given that a pursuit of a research programme is often a very costly undertaking, withholding public support of a degenerating programme would make the pursuit of that programme virtually impossible. It is easy to see how Lakatos's remarks would only strengthen Feyerabend's conviction that methodologies do attempt to formulate rules for theory *pursuit*.

In brief, a systematic taxonomy of stances helps show how Lakatos and Feyerabend inadvertently conflated the question of theory acceptance with the question of theory pursuit and how it may have been resolved. While Lakatos was primarily concerned with outlining the rules that would tell us which of the competing programme is to be *accepted*, Feyerabend's main objective was to show that there can be no rules limiting which programmes can be rationally *pursued*.

4.2. Feyerabend and Kuhn on Paradigm-Monism

Kuhn was notorious for his lack of terminological precision in *Structure*. He often used his own terms in multiple distinct ways [40] and rarely used the terminology of other philosophers he was debating with. Consequently, this led to many instances of misunderstanding. We believe that our taxonomy can help introduce some clarity into Kuhn's position. Specifically, our taxonomy helps to show how Kuhn and Feyerabend were talking past each other in their debate on pluralism during normal science.

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12 See [39] (ch. 4) and [29], especially (p. 66, fn. 20) for his defense of the revival of classical physics in the 1960s.

According to Kuhn, mature science is characterized by often long-lasting periods of the so-called *normal science*, when "the members of a mature scientific community work from a single paradigm" [41] (p. 162). Among other things, *paradigms*, for Kuhn, are "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" [41] (p. x). During *normal science*, research is "firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice" [41] (p. 10). These periods of normal science are preceded and succeeded by periods of *crisis*, when a number of competing paradigms are suggested and the very foundations of science are being questioned. A period of crisis usually culminates in a scientific revolution—a transition to a new paradigm and inception of a new period of normal science. Kuhn argues that each science, during its normal periods, is characterized by a single paradigm, for "without commitment to a paradigm there could be no normal science" [41] (p. 100). He writes:

> At any time the practitioners of a given specialty may recognize numerous classics, some of them—like the works of Ptolemy and Copernicus or Newton and Descartes—quite incompatible one with the other. But that same group, if it has a paradigm at all, can have only one. Unlike the community of artists—which can draw simultaneous inspiration from the works of, say, Rembrandt and Cézanne and which therefore studies both—the community of astronomers had no alternative to choosing between the competing models of scientific activity supplied by Copernicus and Ptolemy. [42] (p. 352).

While there is some debate whether Kuhn was actually a champion of paradigm-monism[13], he has often been read as one.

The idea of a single paradigm prevailing in each field of science at any given time struck Feyerabend as deeply problematic:

> The idea that here we had a period governed by an all embracing paradigm which absorbed the physicist's attention to the exclusion of everything else is seen to be a gross over-simplification. [45] (p. 253).

Feyerabend claims that paradigm-monism, if it ever were to actualize, would be harmful to the progress of science. In fact, says Feyerabend, "actual science is much closer to pluralism than one would expect when consulting the (usually monistic) historians" [46] (p. 111). Scientists should, and customarily do, "invent, and elaborate theories which are inconsistent with the accepted point of view" [46] (p. 105)[14]. The main purpose of this pluralistic proliferation of theories is to ensure "maximum testability of our knowledge" [46] (p. 105)[15]. Apparently, Feyerabend comes to this conclusion based on two assumptions—one drawn from Popper, and the other from Kuhn and Lakatos[16]. On the one hand, as Popper has shown, testability (i.e., empirical content) of a theory is essentially the set of all those propositions that formally contradict the laws of the theory. On the other hand, as Kuhn and Lakatos made clear, mature scientific theories/paradigms/research programmes are suspiciously good at avoiding experimental refutation: observed anomalies often remain shelved unless there is an *alternative* theory that attempts to explain the anomaly. Therefore, it is essential, says Feyerabend to proliferate alternative theories which will help reveal the deficiencies of our theories and, thus, increase their testability [46].

Now, we believe that a systematic application of the taxonomy of stances can introduce clarity and nuance to the debate and thus help to bridge the gap between Kuhn and Feyerabend. In particular,

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13 Watkins [43] argues that Kuhn held the 'paradigm monopoly thesis' though some have been critical of this interpretation cf. [44] (pp. 74–98).

14 This argument is repeated in [47]; [45] (pp. 251–254); and [30] (Sections 3–6).

15 See also [48].

16 See [49] for a discussion of this.

it makes clear what exactly went wrong in the debate and how the two classics came to talk past each other. First, let us appreciate that we are dealing not with one, but with *three different descriptive questions* concerning plurality vs. monism of paradigms:

- *Concerning acceptance*: is it possible to *accept* two or more competing theories at the same time?
- *Concerning use*: is it possible to *use* two or more competing theories in the same application?
- Concerning *pursuit*: is it possible to *pursue* two or more competing theories at the same time?

Importantly, answers to these questions do not necessitate each other: it is quite possible to answer "yes" while simultaneously answering "no" any other. If we apply our taxonomy, Kuhn can be understood as arguing that only a single paradigm can be *accepted* at any one time during normal science, while Feyerabend's criticism reveals that more than one theory/paradigm can be *pursued* at the same time and more than one theory/paradigm can be *used* in the same application. Unfortunately, having conflated the three stances, Kuhn and Feyerabend did not quite see that they were answering related but different questions.

Kuhn's notions of *paradigm* and *normal science* greatly contributed to this conflation. Both of these notions are so rich (or so vague, depending on who you ask) that they include elements of all three stances. First, Kuhn clearly implies that during a period of normal science, scientists *accept* their paradigm as the best available description of its domain. But acceptance is only one of the stances at play here. Consider, for instance, Kuhn's statement that "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" [41] (p. 153). This is, partially, a *promissory note*; it is a statement about the new paradigms *pursuit*-worthiness. He writes:

> The success of the paradigm ... is at the start largely a promise of success discoverable in selected and still incomplete examples. Normal science consists in the actualization of that promise ... [41] (pp. 23–24).

Thus, the dominant paradigm gives scientists a certain assurance that there exist solutions to their puzzles—it contains a promise of fruitful research [41] (pp. 35–42); [50] (p. 322). It allows scientists to avoid distracting debates about the fundamentals of their paradigm and instead focus on *pursuing* different solutions to their puzzles. Finally, accepted paradigms also shape the technological practice of the time, i.e., theory *use*, through producing new instruments and becoming tools for engineering; when switching to a new paradigm, "scientists [often] adopt new instruments" [41] (p. 111). It is not surprising that some readers of Kuhn ended up misunderstanding his point about paradigm-monism; there is sufficient equivocation is Kuhn's language to render such misunderstandings virtually inevitable.

Kuhn, however, was not the only one at fault there, since, as we have shown in the previous section, Feyerabend was also wont to conflate different stances. However, he takes a step towards distinguishing between *acceptance*, *pursuit*, and *use* in his discussion of a special subset of theories—myths[17]. Myths, according to Feyerabend, cannot be *pursued* as they do not offer any puzzles to solve. This is due to the fact that myths are accepted not merely as the *best available* but as *absolutely true* descriptions of their domains:

> A myth can very well stand on its own feet. It *can give explanations*, it can reply to criticism, it can give a satisfactory account even of events which prima facie seem to refute it. It can do this *because it is absolutely true* ... [52] (p. 64).

It is this "attitude of complete and unhesitating acceptance" [52] (p. 61) towards myths that makes it impossible to *pursue* them further. Yet, myths can be very useful: the main function of the myths in

[17] In addition to scientific theories, Feyerabend's concept of *theory* also includes political ideas, religious systems, and myths [51] (p. 219, fn. 3); [46] (p. 105, fn. 5).

science, in Feyerabend's view, is that they are often *useful* for providing alternative viewpoints and thus maximizing criticism. Thus, Feyerabend came close to distinguishing between different stances. However, he never explicitly articulated this and, consequently, conflated different stances. His debate with Kuhn concerning paradigm-monism is a case in point.

A much clearer picture transpires once we look at the debate through the lenses of the taxonomy of stances. Kuhn can be interpreted as saying that during periods of normal science, the same paradigm that is *accepted* is also often being *pursued* and *used*. This, however, does not mean that only the theories within the accepted paradigm can be used in practice or pursued. For instance, he says that "Ptolemaic astronomy is still widely used today as an engineering approximation" ([41], 68) despite the fact that it is no longer accepted as the best available description of its domain. He customarily speaks of theories that "ceased to yield research problems at all and instead become tools for engineering" [41] (p. 79). Thus, it is clear that he would not object to Feyerabend's idea of pluralism of *used* theories. Similarly, Kuhn would have to agree that, at least at times, scientists simultaneously *pursue* multiple conflicting paradigms; this is especially apparent during the period of crisis. If pressed, he would also likely agree that there might be scientists pursuing alternative paradigms even during the periods of normal science; while he seemed to characterize the practice of pursuing more than one paradigm "wasteful of time and talent" [53] (p. 236), at the very least, he would have to accept the existence of that practice and, thus, eventually agree with Feyerabend that many competing theories/paradigms are often simultaneously pursued[18]. So as far as theory *pursuit* and theory *use* are concerned, Kuhn had more in common with Feyerabend than he would be prepared to admit.

What appears to be contentious is whether one can *accept* two mutually inconsistent paradigms/theories at the same time. As far as we can tell, Kuhn's position is that one cannot accept two or more conflicting paradigms at the same time. That is the gist of his claim that "the community of astronomers had no alternative to choosing between the competing models of scientific activity supplied by Copernicus and Ptolemy" [42] (p. 352). It is in this specific sense that Kuhn can be properly characterized as a paradigm-monist. In contrast, Feyerabend seems to think that incompatible paradigms can and have often been simultaneously *accepted*. For instance, he points out that towards the end of the 19th century there were three different paradigms in physics: "*the mechanical point of view* which found expression in astronomy, in the kinetic theory, in the various attempts to devise mechanical models for the explanation of electric phenomena . . . ; the point of view connected with the slow development of an independent and *non-mechanical theory of heat* which finally turned out to be inconsistent with mechanism; the point of view implicit in Maxwell's *electrodynamics* which was developed, and freed from its mechanical concomitants by Hertz" [45] (252–253). However, a closer reading of Feyerabend is required to extract his exact position on this issue.

Regardless of Feyerabend's actual position, the important takeaway of our discussion is that a proper taxonomy of stances would have undoubtedly clarified that the central question at issue was that of the simultaneous *acceptance* of more than one paradigm and would have helped Kuhn and Feyerabend better understand each other.

5. A Few Lessons

In this paper, we have provided a taxonomy of stances that can be taken towards a theory and shown how their application in debates during the historical turn would have aided in their clarification and resolution. Before concluding, we wish to outline a few lessons that can be drawn from the previous discussion.

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[18] It is important to keep in mind that here we are not discussing the normative question of whether it is *advisable* or *permissible* to pursue more than one paradigm at a time. Kuhn would famously object against the practice of pursuing multiple paradigms, by calling it wasteful and not conducive to progress [53] (pp. 229–231); [41] (p. 162). On this normative issue, he and Feyerabend held opposing views.

First, there are different *processes* of scientific change depending on which of the three stances we are focusing on. The history of transitions from one set of *accepted* theories to another set will not always coincide with the history of transitions from one set of *used* theories to another, or from one set of *pursued* theories to another. Regardless of whether we, as historians, are attempting to make sense of a certain historical episode or whether we, as philosophers, are trying to understand the mechanism of changes in theories, we have to first and foremost clarify changes in which of these three stances we are interested.

Second, the respective *mechanisms* of change would likely be different. It is safe to say that the requirements that a theory should satisfy to become *accepted* as the best available description of its domain would not be the same as the requirements that the same theory should satisfy to be *used* in practical applications. It is likely that there will be a third set of requirements that a theory should satisfy to be considered *pursuit-worthy*. Consider, for instance, the requirement of confirmed novel predictions that is employed nowadays in some situations when evaluating the *acceptability* of a theory: if a physical theory attempts to introduce a new type of entity or a new type of relation, it is often expected to have confirmed novel predictions (for a detailed reconstruction of the requirement, see [24] (pp. 145–150). In contrast, when we evaluate the *usefulness* of a theory, we do not seem to be particularly concerned about the presence or absence of any novel predictions: in fact, we may find a theory useful for a variety of different reasons depending on what we are trying to accomplish. An engineer will probably use a theory if its equations are easy to calculate and if the resulting predictions are sufficiently accurate for the project at hand. An educator may find a theory useful from the didactic perspective when, say, contrasting the theory with its competitors. A psychic will find an astrological theory useful for making money, etc. Finally, we seem to be even more lenient when it comes to evaluating the *pursuit-worthiness* of a theory. Whatever our actual methods for theory acceptance, use, and pursuit may be, it is important to keep in mind that to the best of our knowledge all of these three types of methods are *changeable*—different communities and different time-periods will likely have their distinct methods of evaluation for theory *acceptance*, *use*, and *pursuit*.

Third, all three stances allow for the pluralism about which Feyerabend was so adamant. Historically, we often see multiple mutually inconsistent theories simultaneously *used* by the same community even in the same application. Take Hasok Chang's favorite example of GPS technology which simultaneously makes use of mutually inconsistent theories; it uses classical mechanics to put satellites in orbit around the earth and uses time correction formulas from special and general relativity and atomic clocks built on principles from quantum theory [54] (p. 266). Similarly, two mutually inconsistent theories can be simultaneously *pursued* by the same community. In fact, sciences do this all the time, by making it possible for different researchers to work on elaborating very different—often drastically opposing—ideas. Finally, the history of science seems to suggest that it might even be possible to simultaneously *accept* two mutually inconsistent theories. For instance, nowadays we accept both general relativity and quantum physics as the best available descriptions of their respective domains despite the fact that the two theories seem to be mutually inconsistent, especially when attempting to describe objects that are both very small and very massive (e.g., singularities within black holes). This apparent tolerance towards inconsistencies is probably due to the fact that neither of these theories is taken to be a perfect description of their respective domains[19].

There will, undoubtedly, be other important lessons to be drawn from the application of the taxonomy of stances. At this stage, it is clear that changes in different stances pose different historiographical and philosophical questions and, therefore, they should not be conflated.

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[19] For a detailed discussion, see [24] (pp. 152–164).

6. Concluding Remarks

We think that the recent revival of interest in the historical turn reminds us of all kinds of interesting questions that we have forgotten about. It also forces us to look closer at the philosophies of those participating in the historical turn and introduce some nuance and sophistication to the caricatures with which these philosophers are often landed. However, as commentators, we must remain vigilante and watchful for mistakes that were made. We believe that our taxonomy of stances provides us with an effective tool for analyzing debates about scientific change and can greatly clarify the tasks we have in front of ourselves.

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