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Kuhnian revolutions revisited

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Abstract I re-examine Kuhn's account of scientific revolutions. I argue that the sorts of events Kuhn regards as scientific revolutions are a diverse lot, differing in significant ways. But, I also argue that Kuhn does provide us with a principled way to distinguish revolutionary changes from non-revolutionary changes in science. Scientific revolutions are those changes in science that (1) involve taxonomic changes, (2) are precipitated by disappointment with existing practices, and (3) cannot be resolved by appealing to shared standards. I argue that an important and often overlooked dimension of the Kuhnian account of scientific change is the shift in focus from theories to research communities. Failing to make this shift in perspective might lead one to think that when individual scientists change theories a scientific revolution has occurred. But, according to Kuhn, it is research communities that undergo revolutionary changes, not individual scientists. I show that the change in early modern astronomy is aptly characterized as a Kuhnian revolution.

Keywords Kuhn · Scientific revolution · Scientific change · Research communities · Taxonomic change

1 Introduction

Kuhn's legacy to philosophy of science is his theory of scientific change according to which the growth of scientific knowledge is punctuated by unsettling revolutionary episodes. Both historians and philosophers of science have raised concerns about the concept "scientific revolution." Some historians suggest that "the Scientific Revolution" of the early modern era was merely a rhetorical construction, and though it has figured prominently as an organizing idea in the discipline of history of science

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the idea has now lost its utility (see, for example, Dobbs, 2000; Shapin, 1996). And, many philosophers have sought to mitigate the threat posed by the very idea of revolutionary changes in science, since for many a revolutionary change seems to imply a non-rational change (see, for example, van Fraassen, 2002, Lecture 3).

My aim in this paper is to re-examine Kuhn's account of scientific revolutions. As some critics suggest, the sorts of events Kuhn regards as scientific revolutions are a diverse lot, differing in significant ways. Still, I argue that Kuhn does provide us with a principled way to distinguish revolutionary changes from non-revolutionary changes in science. I argue that an important and often overlooked dimension of the Kuhnian account of scientific change is the shift in focus from theories to research communities. Failing to make this shift in perspective might lead one to think that when individual scientists change theories a scientific revolution has occurred. But, according to Kuhn, it is research communities that undergo revolutionary changes, not individual scientists. Finally, I demonstrate how the concept of a Kuhnian revolution provides insight into the change that occurred in early modern astronomy.

In Sect. 2, I present four criticisms of Kuhn's distinction between normal and revolutionary scientific changes. In Sect. 3, I examine Kuhn's remarks about the nature of scientific revolutions and explain why he chose to call some changes in science "revolutions." In Sects. 4 and 5, I address the concerns of the critics.

2 Four criticisms of Kuhn's distinction

Let me begin by presenting four criticisms that have been raised against Kuhn's account of scientific revolutions. All four challenge the way Kuhn draws the distinction between normal and revolutionary science and thus lead us to question Kuhn's account of scientific revolutions.

First, Ernan McMullin (1993) argues that the various changes in science that Kuhn regards as revolutionary did not affect scientists and scientific practice to the same degree, and consequently are not aptly described as being of one kind. McMullin suggests that we need to distinguish between what he calls shallow, intermediate, and deep revolutions.

McMullin regards the discovery of X-rays as a typical shallow revolution (p. 59). As he explains, with this discovery "no fundamental change in theory occurred. No troublesome anomalies were noted in advance [and] there was no prior crisis to signal that a revolution might be at hand" (p. 59). Shallow revolutions, like this one, are quite circumscribed and leave much intact. More profound, McMullin argues, was the impact of "the replacement of phlogiston theory by the oxygen theory of combustion" (p. 60). But, even in this case "the epistemic principles governing the paradigm debate" were "left more or less unchanged" (p. 60). Consequently, McMullin regards it as an "intermediate revolution." McMullin grants that *some* revolutions are in fact *deep*. But these revolutions, he suggests, are quite rare, and when they have occurred they occur over a long period of time.[1] For example, according to McMullin, the Copernican revolution, "took a century and a half … to consummate" (p. 60).[2] Given

[1] McMullin claims that even "the two major revolutions in the physics of [the twentieth] century [are not aptly described as] deep [revolutions]" (1993, p. 61).

[2] Some historians of science have also raised this concern. For example, Dobbs (2000) expresses concern about the fact that many alleged scientific revolutions, the Copernican Revolution and *the*

the significant differences between the various scientific changes that Kuhn regards as revolutions, McMullin questions whether the various events constitute one *type* of event.[3]

Second, developing this line of reasoning further, McMullin (1993) argues that the various changes that Kuhn regards as revolutions are not different in *kind* from the various changes that occur during the phases that Kuhn describes as normal science. Rather, McMullin suggests that the various types of changes in science are more aptly construed as lying on a continuum. What we find in the historical record "is a spectrum of different levels of intractability, not just a sharp dichotomy between revolutions and puzzle solutions" (pp. 62–63). Further, McMullin argues that "decision between rival theories is an everyday affair in any active part of science" (p. 62). Hence, theory choice is not unique to scientific revolutions.[4]

Whereas McMullin's first criticism suggests that not all alleged revolutions affect science so profoundly, his second criticism suggests that revolutionary discoveries are not *categorically* different from normal scientific discoveries. Rather, revolutionary discoveries are those rare discoveries lying at one end of a continuum of discoveries, a continuum that includes even the most routine discoveries of normal science.

Third, Alexander Bird (2000) suggests that Kuhn's two categories, normal science and revolutionary science, are not exhaustive, and thus fail to provide us with the conceptual resources necessary to understand the variety of changes that do occur in science. Hence, "Kuhn's picture has no place for some classes of scientific discovery" (p. 50). In particular, Bird believes that the discovery of the structure of DNA "does not fit Kuhn's description of development—it originated in no crisis and required little or no revision of existing paradigms even though it brought into existence major new fields of research" (p. 60). Bird argues that significant discoveries that lead to the development of new practices and fields, but that do not alter existing paradigms, fit into neither of Kuhn's categories.

Footnote 2 continued
Scientific Revolution in particular, are such long drawn out affairs (p. 31). Clearly, she claims, events extending over such a long period of time hardly deserve to be called revolutions. Cohen (1985) also raises this concern. Cohen regards revolutions as essentially abrupt and significant changes.

Not everyone agrees that revolutions are necessarily abrupt events. Westfall (2000) believes that the key similarity between significant scientific changes and political revolutions that warrants treating the former as "revolutions" is the thoroughness of the changes, rather than the brevity of the events (p. 44). Westfall argues that the concept "revolution" still has an important role to play in advancing our understanding in the history of science. Incidentally, as Perez Zagorin explains, "in 1850, Alexis de Tocqueville, [noted that] … 'for sixty years we have been deceiving ourselves by imagining that we saw the end of the [French] Revolution' " (1973, p. 24).

3 McMullin is not the only one to raise this concern. See also Bird (2000, 50–54).

4 This concern has also been raised by others, including Bird (2000, 54–57) and Mayr (2004). Mayr believes that in biology "there is no clear-cut difference between revolutions and 'normal science' " (p. 165). Mayr grants that there have been some significant changes in biology, but he insists that "even the major revolutions do not necessarily represent sudden, drastic paradigm shifts" (p. 168). In fact, he claims that in biology "an earlier and subsequent paradigm may coexist for long periods … [and] they are not necessarily incommensurable" (p. 168).

Others have also sought to determine the extent to which Kuhn's model of change fits the biological sciences, given that Kuhn's background is in the physical sciences and his examples are drawn predominantly from physics, astronomy, and chemistry. For example, Greene (1971) investigates whether Kuhn's theory of scientific change aptly describes the process of change in *natural history*. Greene argues that, though "the Kuhnian paradigm of paradigms can be made to fit certain aspects of the development of natural history … its adequacy as a conceptual model for that development seems doubtful" (p. 23).

Fourth, Larry Laudan (1984) argues that *no changes* in science are aptly described as *revolutionary*. Laudan notes that, given Kuhn's holistic account of paradigms, a revolutionary change involves simultaneous changes in methods, goals, and theories. Laudan, though, claims that Kuhn's holism "leads to expectations that are confounded by the historical record" (p. 84). Laudan suggests that all changes in science are continuous enough with the traditions preceding them to make calling any of them "revolutions" inappropriate. The only reason one would be led to believe otherwise, he claims, is if one fails to look at the process of change in sufficient detail.

To summarize, critics have raised four challenges to the distinction Kuhn draws between normal and revolutionary science: (1) the changes that Kuhn regards as revolutionary changes are a diverse range of phenomena; (2) the two types of changes, normal and revolutionary, are not *categorically* different; (3) the two categories, normal and revolutionary, are not exhaustive; and, finally, (4) the class of revolutionary changes is an empty class.

Motivating Kuhn's critics is a concern to show that science is not influenced by irrational or non-rational factors to the extent that Kuhn implies (see Laudan, 1984, pp. 70–71). If truly revolutionary scientific changes are either *extremely rare*, or *never* occur, or can be shown to be similar in important respects to normal scientific changes, then science is shielded from the influence of non-rational factors.

3 What did Kuhn mean by "revolutionary change"?

In this section, I want to examine what Kuhn meant by the term "scientific revolution." Kuhn's choice of terms was very deliberate, and understanding what he meant will help us evaluate and address the critics' concerns.

In *Structure*, Kuhn defined a scientific revolution as a "non-cumulative developmental [episode] in which an older paradigm is replaced in whole or in part by an incompatible new one" (1996, p. 92). This characterization of scientific revolution led to a variety of misunderstandings. As Margaret Masterman (1970) notes, in *Structure* Kuhn employs the term "paradigm" in multiple ways. The ambiguity associated with the term "paradigm" led to multiple interpretations of Kuhn's view of scientific revolutions, thus creating misunderstandings.

As Kuhn refined his view in light of criticism, he developed an alternative characterization of scientific revolutions, one that makes no reference to paradigms. Ultimately, he characterizes scientific revolutions as scientific changes involving taxonomic changes (see Kuhn, 2000). An example will illustrate what he has in mind. Whereas Ptolemaic astronomers used the term "planet" to denote wandering stars, that is, those "stars" that are subject to apparent retrograde motion, Copernicus used the term "planet" to denote satellites of the sun. This is no small change. After all, whereas Ptolemaic astronomers did not consider the earth to be a planet, Copernican astronomers did conceive of the earth as a planet. According to Kuhn, a revolution always involves the *replacement* of one lexicon or taxonomy by another. Because a research community can only change taxonomies if a lexicon or taxonomy is already widely accept, revolutions can only occur in *mature* fields (see especially Kuhn, 1996, Chapter VII).

Despite revising the definition of "scientific revolution," Kuhn did not change his views about which episodes in the history of science were to count as revolutions. In offering a new definition, his aim was to provide clarity to his view in *Structure*. As we will see, a clearly articulated notion of scientific revolution cannot be reconciled with the full list of examples Kuhn cites.

Although the notion of paradigm is no longer central to Kuhn's account of scientific revolutions, paradigms still have an important place in his mature account of scientific change. The positivists, Kuhn's adversaries in *Structure*, implied that all scientific knowledge is embodied in theories whose content is expressible in sentences. Kuhn, though, insists that some scientific knowledge is embodied in concrete scientific accomplishments that serve as exemplars for solving hitherto unsolved problems. Kepler's model of the orbit of Mars presented in *The New Astronomy* is a typical paradigm. It provided astronomers with a template for modeling the motion of the other planets. Even after Kuhn conceptualized revolutions as taxonomic changes, he still believed such exemplars play a crucial role in science and science education.

In calling particular episodes in the history of science *revolutions*, Kuhn sought to draw attention to similarities between a type of change in science and a type of political change, political revolutions. In *Structure,* he identifies two key similarities that warrant the comparison. First, he notes that preceding both a political revolution and a scientific revolution is a "growing sense … that existing institutions have ceased adequately to meet the problems posed by an environment that they have in part created" (1996, p. 92). This "sense of malfunction that can lead to crisis is prerequisite to revolution" (p. 92). Hence, just as a political community will not overthrow the existing institutions until they believe that these institutions are failing them, a scientific community will not seek to replace its taxonomy until they believe that the taxonomy is unfit to solve the problems that concern them.[5] The reason scientists are reluctant to alter their taxonomy should be clear. As Kuhn explains, "as in manufacture so in science—retooling is an extravagance reserved for the occasion that demands it" (1996, p. 76).[6]

The second noteworthy similarity between political revolutions and these significant scientific changes is that neither sort of change is sanctioned by the existing institutions and norms. As Kuhn explains, "political revolutions aim to change political institutions in ways that those institutions themselves prohibit" (1996, p. 93). Similarly, revolutionary scientific changes alter the existing institutions and norms in ways that the currently widely accepted theory, norms, and standards prohibit.

Not only do the scientists involved in a revolution not acknowledge the legitimacy of existing standards, the competing parties involved "acknowledge no supra-institutional framework for the adjudication of revolutionary difference" (p. 93). Consequently,

[5] A number of social scientists attribute a significant causal role to the sense of disappointment that precedes political revolutions. See, for example, Davies (1962) and Gurr (1970). This emphasis on the role of rising disappointments as the cause of political revolutions can be traced back to de Tocqueville (see Zagorin, 1973, p. 41). Significantly, Gurr argues that it is neither deprivation in itself nor a specifiable degree of deprivation that leads to a political revolution (1970, p. 83). History provides us with many examples of people deprived of many things who do not revolt. The deprivation that seems to agitate people enough to revolt is a *relative* deprivation. That is, a precondition for a political revolution and political violence in general is a perceived discrepancy between people's expectations and their capabilities for satisfying them. As Gurr explains, people's "point of reference [in determining their own sense of deprivation] may be [their] own past condition, an abstract ideal, or the standards articulated by a leader as well as a 'reference group' " (p. 25). Hence, in principle, one could have an *objective* improvement in one's conditions and still feel a sense of deprivation.

[6] Kuhn employs a variety of metaphors in *Structure*, including paradigm, religious conversion, political revolution, gestalt switch, this manufacturing metaphor, as well as various biological metaphors. Though his metaphors have led to interesting insights for those studying science, at times his mixing of metaphors makes it difficult to get clear what exactly he is saying about science.

each party appeals to its own standards to justify their behavior. As a result, the competing parties involved in a revolution inevitably talk past each other. Moreover, any attempt to justify one's choice is apt to be circular (p. 94). Both those defending the status quo and those seeking to initiate a radical change will justify their choice by appealing to standards and norms their opponents do not accept. Indeed, it is this feature of scientific revolutions that has led philosophers to think that Kuhn believes that the resolution of revolutions is irrational (see especially Lakatos, 1972, p. 93, and Laudan, 1984).

In summary, according to Kuhn, scientific revolutions are those changes in science that (1) involve taxonomic changes, (2) are precipitated by disappointment with existing practices, and (3) cannot be resolved by appeal to shared standards. It is worth stressing that these features are features that affect a research community, not individual scientists. An individual's decision to adopt a theory different from the one she accepted in the past does not qualify as a revolution. Hence, when Tycho Brahe accepted a new theory of planetary motion that embodied a lexicon or taxonomy at odds with the then-widely accepted Ptolemaic taxonomy, a revolution in astronomy did not occur. The relevant locus of taxonomic change is the research community. A revolution occurs only when a research community changes taxonomies.

This point about the locus of change in science is easy to overlook. In fact, Kuhn admits that in his early discussion of scientific change, he often confused the attributes and experiences of individual scientists with the attributes and experiences of the research community. For example, he occasionally carelessly implies that both individuals and research communities undergo a gestalt shift when they accept a new theory (see Kuhn, 2000, p. 88). Clearly, as Kuhn recognizes, only individuals would have such an experience. Moreover, a gestalt shift is not a revolution (see Barker et al., 2003, p. 220 and Nersessian, 2003, p. 185).

Further, Kuhn uses the term "revolution" as a term of success only. Given his definition, there is no such thing as a failed scientific revolution. Though the expression or elaboration of an alternative theory may seem to indicate the potential for a revolution, and its subsequent suppression may seem like an apt candidate for a *failed* scientific revolution, the sorts of events that Kuhn would call revolutions require the overthrow of one theory by another. A revolution occurs when one lexicon or taxonomy is replaced by another incompatible lexicon or taxonomy.

We are now in a position to identify the necessary and sufficient conditions for a Kuhnian revolution. For a scientific revolution to occur, (i) a research community must make a taxonomic change, (ii) the change must undermine the shared standards of the research community, and (iii) there must be widespread disappointment with existing practices. Each of these conditions is a necessary condition for a revolution. But, none on its own is a sufficient condition. It is only when the three conditions occur together that a scientific revolution occurs. After all, some taxonomic changes can be made without undermining the shared standards of a research community. For example, the addition of a new term designating a newly discovered animal species may be accommodated by merely extending the existing biological taxonomy, by adding an additional branch. Such an addition is not apt to affect the prevailing standards of evaluation. But, when a set of concepts is reorganized, changing the relations between the concepts such that particular things that were regarded as belonging to the same class no longer belong to the same class, evaluating the competing theories becomes problematic. As we will see below, in Sect. 5, competing taxonomies direct scientists' attention to different problems.

Widespread disappointment with existing practices is also necessary in every scientific revolution. Unless there is such disappointment, there would be no *reason* to change taxonomies. Scientists are not whimsical. Indeed, were a research community to change taxonomies without there being widespread disappointment, science would differ little from the fashion industry where changing *tastes* are sufficient to cause radical change.

Confusions about the meaning of "revolution" are not unique to Kuhn or philosophers and historians of science. There is no consensus amongst social scientists on what a *political revolution* is. In the early 1970s Perez Zagorin examined a variety of definitions of "political revolution" in various theories of political revolution developed in the social sciences and history. Zagorin found them all wanting in one way or another, and concluded that "after this review of theories of revolution, the main conclusion to be drawn is that the subject is in a lively but disorderly state" (1973, p. 52).[7] An examination of some of the most influential work on political revolution published since the publication of Zagorin's paper reveals that little has changed. The term "revolution" continues to be used in a variety of ways (compare, for example, Gurr, 1970 with Skocpol, 1979 or Tilly et al., 1975). Some treat political revolutions as a sub-class of political violence, others as a sub-class of social revolutions. In an effort to advance our understanding of political revolutions, Zagorin urges social scientists and historians to reserve "the term ['revolution'] for a single, reasonably well marked out class of events" (p. 27). Philosophers and historians of science need to show similar restraint if the concept of "scientific revolution" is to aid us in understanding the dynamics of scientific change.

4 Kuhnian replies

In this section, I want to begin the task of developing and defending a Kuhnian account of scientific revolution. I will examine the first three concerns presented in Sect. 2, above. Addressing these concerns will provide me with an opportunity to clarify Kuhn's view, as well as make concessions to the critics in my efforts to develop an adequate account of scientific revolutions.

First, McMullin is correct to claim that the sorts of changes that Kuhn identifies as revolutions are a mixed lot. Some of Kuhn's examples of revolutions do not have the requisite necessary features. For example, the discovery of X-rays is not aptly described as a scientific revolution. Even though this discovery opened up new and unanticipated areas of research, it did not lead to the replacement of one taxonomy by another incommensurable taxonomy (see Chen and Barker, 2000).[8] Rather, the

7 Kramnick (1972) also examines the competing definitions and explanations of revolution in the then-recent scholarship. He reaches a similar conclusion to that reached by Zagorin. As Kramnick (1972) explains, "as diverse as is the literature defining revolution, there is an even wider assortment of explanations" (p. 35). He distinguishes between four broad types of explanations: political, economic, sociological and psychological. The psychological, he suggests, are the least plausible, and all four types of explanations seem to rightly presuppose a political dimension. Kramnick also discusses Kuhn's theory of revolution in an effort to determine what insight it might provide into the nature of political revolutions.

8 Chen and Barker (2000) note that some taxonomic changes can be accommodated without violating the "no-overlap principle" (p. S214). In such cases, the new and old taxonomies are not incommensurable. Like those critics of Kuhn who seek to mitigate the affects of non-rational factors, Chen and Barker aim to show that all conceptual changes in science can be rationally defended (p. S221).

discovery was accommodated by creating a new field, a field devoted to the study of hitherto unnoticed phenomena (see Wray, 2005). Even though the discovery of X-rays had important implications for neighboring fields, it did not require the replacement of the taxonomies employed in neighboring fields. X-rays could be added to the inventory of possible entities by merely adding on to or extending the accepted taxonomy. Since the discovery of X-rays did not lead to the development of a new taxonomy incommensurable with the old taxonomy, it is a mistake to count it amongst the class of revolutions.

The discovery of X-rays is not the only example of an alleged revolutionary discovery identified by Kuhn that really does not warrant the name. Elsewhere, I have compiled a list of 25 alleged revolutionary discoveries identified by Kuhn in *Structure* (see Wray, 2003). The discovery of Uranus is also the sort of discovery that merely required the extension of an existing taxonomy. There was no need to replace the existing taxonomy with a new *incommensurable* one. Hence, the critics' first concern is legitimate. The sorts of changes that Kuhn has grouped together under the label "revolution" are not all the same type. Those scientific changes that deserve to be called revolutions involve the replacement of one lexicon or taxonomy by another.

Let us now consider McMullin's second criticism, revolutionary scientific changes and normal scientific changes are not categorically different. As McMullin notes, the sorts of problems that lead to many discoveries, normal and revolutionary, may appear intractable at first. What distinguishes the problems that give rise to revolutionary discoveries from the problems that one encounters during periods of normal science is that the former are generally *more* intractable. Further, no matter how fundamental a change is in science there is always some degree of consensus amongst the relevant practitioners. What distinguishes periods of revolutionary science from periods of normal science is the extent of consensus in the research community.

In suggesting that the differences between revolutionary changes and normal changes are merely differences of degrees, McMullin implies that there is no *principled* way to distinguish the two classes of events. On this point, he is mistaken. In normal science, scientists agree about the standards by which a contribution is to be evaluated, whereas in revolutionary science the parties involved do not agree about the standards by which their competing claims should be judged. Consequently, revolutionary changes are resolved in a manner that resembles the resolution of political revolutions.[9]

Let us now consider Bird's concern, that Kuhn's taxonomy of scientific changes is not exhaustive. According to Bird, normal science and revolutionary science do not account for all of the types of changes in science. Bird is certainly correct about this. But, the Kuhnian account of scientific change provides us with greater resources to account for the range of changes in science than Bird claims. Kuhn's account of the developmental cycle of scientific change explicitly recognizes at least two additional types of changes, paradigm-creating changes and pre-paradigm discoveries.

Consider *paradigm-creating* changes. A discovery that leads to the creation of the first paradigm in a field is neither a revolutionary change nor a normal change. Such a discovery neither applies a widely accepted taxonomy nor aims to replace a widely accepted taxonomy. The discovery of DNA seems to fit this description. The discovery

[9] Discoveries made during periods of normal science *can* be significant, for "normal" does not imply insignificant. Boyle's Law is a typical normal scientific discovery. This discovery was certainly *significant*, but that does not make it revolutionary. The evaluation of Boyle's great discovery was unequivocal.

of X-rays also fits this description. Unlike the taxonomic change that occurred when Descartes' mechanistic physical theory *replaced* Aristotle's physical theory, the discovery of X-rays did not involve the replacement of one theory by another.

Kuhn also recognizes a fourth class of scientific discoveries, discoveries that occur during the pre-paradigm stage of a field. For example, Kuhn cites a variety of discoveries made by those who studied electrical phenomena before a research community was formed with a widely accepted taxonomy (see Kuhn, 1996, pp. 13–14). Such discoveries are also instances of neither revolutionary science nor normal science.

5 Are there really scientific revolutions?

In this section, I argue that there really are Kuhnian revolutions in science. Specifically, I argue that the best way to explain the change that took place in astronomy in the 16th and 17th centuries is to invoke the concept of a Kuhnian revolution.

A number of historians and philosophers of science question whether this episode in the history of science is aptly described as a Kuhnian revolution. Cohen (1985), for example, suggests that if there was a revolution in 16th century astronomy it involved the *changes in practice* introduced by Tycho Brahe. As far as he is concerned, the 16th century witnessed no change in theory. And, Michael Heidelberger (1980) suggests that "*with the emergence of Copernicus' theory, no paradigm-shift occurs but rather a coalescing of two traditional paradigms*" (p. 277). Hence, as far as Heidelberger is concerned, the change that occurred is not aptly described as an instance of theory replacement.

I believe that neither Cohen nor Heidelberger are correct for Copernicus' discovery involved a significant taxonomic change. The meaning of key terms in astronomy were altered, most notably "planet." Whereas Ptolemaic astronomers regarded "planets" as wandering stars and did not count the earth as a planet, Copernican's grouped the Earth with Mercury, Venus, Mars, Jupiter and Saturn, and regarded planets as satellites of the sun. Copernicus also introduced a new type of phenomenon into astronomy, satellites of planets. In Ptolemy's theory, no celestial body orbits another body that simultaneously orbits a third body. Given Copernicus' theory, though, the moon orbits the earth as the earth orbits the sun. Hence, the changes that occurred in astronomy in the 16th and 17th centuries involved significant taxonomic changes. And such changes could not be made by simply extending the then-widely-accepted Ptolemaic taxonomy. Hence, the change in astronomy involved more than a change in practices or a coalescing of two traditional paradigms.

Those who reject the notion of Kuhnian revolutions believe that there are always shared standards rich enough to resolve disputes in science. A careful look at the change in early modern astronomy, though, will show that the *shared* standards were not rich enough to resolve the dispute. Indeed, it is easy to understand why some may be led to think that early modern astronomers *did* agree on standards. No matter what their theoretical allegiances were, astronomers agreed that an acceptable theory should accurately predict the location of stars and planets, as well as the dates of eclipses. Because accuracy in predictions can be quantified, it seems that early modern astronomers did in fact accept a common standard. Indeed, if the dispute in astronomy *could* be resolved by appeal to quantitative measures alone, then the comparison with political revolutions would be inappropriate. From a quantitative point of view the competing theories were equally strong. The two theories were equally prone to error with respect to predicting the locations of the moon and the planets, and the margins of error of the two theories were also comparable. As Owen Gingerich (1975) explains, "in the Regiomontanus and Stoeffler ephemeredes [which were based on Ptolemy's theory], the error in longitude for Mars is sometimes as large as 5°. However, in 1625, the Copernican errors for Mars reached nearly 5°" (p. 86). The ranges of errors were even greater with respect to the theories' abilities to predict the location of Mercury (Gingerich, 1971). Victor Thoren (1967) also notes that "the Ptolemaic and Copernican theories frequently differed by over 1/2° in predicting the longitude of the moon, and it was common knowledge that the moon was rarely to be found in the place assigned to it by either theory" (p. 21).[10] Thus, the shared quantitative standards underdetermined theory choice (see also Heidelberger, 1980, p. 274).

Despite the fact that the theories were roughly comparable from a quantitative point of view, astronomers generally did not suspend judgment and await the arrival of further quantitative data. Rather, they took sides in the dispute. The fact that some astronomers were persuaded to accept the Copernican theory, while others were led to accept the Ptolemaic theory strongly suggests that *qualitative* considerations had a significant impact in the resolution of the dispute.

Qualitatively, the theories were different. Each theory could address different problems, and neither theory addressed all problems. Copernicans could explain why Venus and Mercury do not depart far from the sun, and later, why Venus exhibits phases like the Moon. Ptolemaic astronomers, on the other hand, had a theory that fit better with a literal interpretation of Scripture. Moreover, the Ptolemaic theory fit better with the then-accepted terrestrial physics. Further, Ptolemaic astronomers were able to cite the fact that stellar parallax could not be detected, a phenomenon that seemed to be implied by Copernicus' theory, but not by their own theory.

Early modern astronomers did not agree about what problems an acceptable theory *should* address. It is in this respect that they disagreed about the standards by which a theory should be judged. Early advocates of the Copernican theory regarded the strengths of the Copernican theory as more significant than the strengths of the Ptolemaic theory. In contrast, those who remained loyal to the Ptolemaic theory regarded the strengths of that theory as more significant than the strengths of the Copernican theory. Further, there was no agreed upon way to order and weigh the considerations in favor of each theory (see Kuhn, 1977). Hence, the dispute in early modern astronomy was resolved, to a large extent, without the aid of shared standards.

Given the protracted nature of the taxonomic change that occurred in early modern astronomy, some critics find it odd to call the event a "revolution." Critics who emphasize the fact that the change was drawn out assume that if the *individuals* involved experienced the change as continuous with the preceding tradition, then there is little basis for calling the event a revolution. This line of reasoning, I believe, is mistaken and betrays a misunderstanding of the nature of Kuhnian revolutions.

According to the Kuhnian account of scientific change, the *locus* of scientific change is the research community. That is, it is a research community that undergoes a revolutionary change, not an individual scientist. Individual scientists do adopt new taxonomies, but such an event does not constitute a scientific revolution. Brahe's decision to abandon the Ptolemaic theory for his own new theory may have required a radical

[10] Gingerich (1973) provides graphs comparing the accuracy of both the Copernican and Ptolemaic theories with respect to their predictions for the locations of Mercury, Venus, the Sun, Mars, Jupiter and Saturn over a number of years (p. 54).

shift in the way *he* saw the world, but there was no revolution in astronomy until a new taxonomy came to be widely accepted in the research community. Indeed, one of the most radical aspects of Kuhn's account of scientific change is the focus on research communities rather than theories. Theories still matter. But, the community is more fundamental.

When we look at the change in early modern astronomy from the perspective of the research community, we can see the revolutionary nature of the change. The illusion of continuity is a function of scale. Revolutions are macro-level phenomena and appreciating the process requires a macro-level perspective. Provided one is willing to adopt the macro-level perspective on scientific change, Kuhn's political metaphor is apt and the Copernican revolution is a quintessential scientific revolution.

This brings us to a final consideration that critics cite in their efforts to show that the change in early modern astronomy was not a Kuhnian revolution. It is often noted that when Copernicus developed his theory, astronomy was *not* in a state of crisis (see Heidelberger, 1980, p. 275). If this is true, then one of the necessary conditions for a Kuhnian revolution is not fulfilled. But, as Copernicus correctly notes, there was no consensus amongst his contemporaries. As Copernicus explains, "in setting up the solar and lunar movements and those of the other five wandering stars, [mathematicians] do not employ the same principles, assumptions, or demonstrations for the revolutions and apparent movements … some make use of homocentric circles only, others of eccentric circles and epicycles" (1543/1995, p. 5).

And, once the Copernican research program gained adherents discontent began to grow. Astronomers became aware of the fact that Copernicus' theory could answer questions that the Ptolemaic theory could not. For example, Copernicus' theory provides an explanation for why Venus and Mercury do not stray far from the sun (see Copernicus, 1995, pp. 19–21). Indeed, Ptolemaic astronomers were aware of the facts. They knew the maximum angles of elongation of the orbits of Venus and Mercury. And, they even had a solution to the problem. But, their solution to the problem was ad hoc. They merely *stipulated* that the center of the epicycles of both Venus and Mercury always remain on a straight line running from the sun to the earth. Indeed, this stipulation solves the problem, ensuring that the theory agreed reasonably well with appearances. But, once compared with the Copernican solution, the Ptolemaic solution struck some as inadequate. Thus, Copernicus' proposal sowed the seeds of discontent for some Ptolemaic astronomers. Moreover, the discontent increased over time. After Galileo announced his discovery that Venus exhibits phases like the moon, even more Ptolemaic astronomers became dissatisfied.

It is worth highlighting the similarity between the situation in early modern astronomy and the dynamics of political revolutions. Political revolutions do not occur at the first expression of an alternative incommensurable political ideal. Hence, the mere expression of an alternative view should not be identified as the revolution. In fact, there may not even be widespread discontent when an alternative is presented. For a political revolution to occur, the alternative view needs to gain adherents. Similarly, a revolution did not occur in astronomy just because Copernicus provided an alternative theory. His theory, though, did begin to breed discontent amongst astronomers which then led others to develop the theory in ways that increased the discontent. In fact, the revolution did not occur until a *much-altered* Copernican taxonomy replaced the Ptolemaic taxonomy.

In examining the nature of the change in early modern astronomy I have addressed the final concern of the critics, the concern that "scientific revolution" is an empty class. Clearly, the changes in astronomy ushered in by Copernicus and his followers are an instance of a Kuhnian revolution.

6 Concluding remarks

I began this paper presenting four concerns that have been raised against Kuhn's account of scientific revolutions. Kuhn's critics are correct in claiming that his examples of revolutions are a heterogeneous lot and do not all warrant being classified as revolutions. However, I have argued that the critics' remaining concerns are ungrounded. Kuhn does not believe that all scientific changes are instances of either normal science or revolutionary science. Further, Kuhn defines "revolution" in a manner that makes it clear that revolutionary changes are categorically distinct from normal scientific changes. The Copernican Revolution, I have argued, is a paradigmatic case of a Kuhnian revolution. Hence, we need not be concerned about the claim that "Kuhnian revolutions" is an empty class.

I have argued that an important and often overlooked dimension of the Kuhnian account of scientific change is the shift in focus from theories to research communities. Failing to make this shift in perspective might lead one to think that when individual scientists change theories a scientific revolution has occurred. Research communities undergo revolutionary changes, not individual scientists. Scientific revolutions are those changes in science that involve taxonomic changes, precipitated by disappointment with existing practices, that cannot be resolved by appealing to shared standards.

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