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Structure and Scientific Controversies

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Abstract In this paper, I highlight the importance of models and social structure to Kuhn's conception of science, and then use these elements to sketch a Kuhnian classification of scientific controversies. I show that several important sorts of non-revolutionary scientific disagreements were both identified and analyzed in *Structure*. Ultimately, I contend that Kuhn's conception of science supports an approach to scientific controversies that has the potential to both reveal the importantly different sources of scientific disagreements and to provide useful resources for understanding their endurance and eventual termination. Several brief examples are used to suggest the power of a Kuhnian analysis and this analysis is contrasted with several more contemporary alternatives.

Keywords Kuhn · Scientific controversy · Models · Social structure of science

1 Introduction

Aristides Baltas (2000) once claimed that the study of scientific controversies was philosophically underdeveloped because of the work of Thomas Kuhn. Though Kuhn did bring attention to scientific disagreements, Baltas alleged that he did so only under the "all-embracing" notion of a paradigm change, which led to the philosophical community overlooking the "finer details" of such disagreements. Baltas is surely correct that much of the philosophical interest in, and attention to, Kuhn's *Structure of Scientific Revolutions* has focused on the alleged incommensurability of settled modes of inquiry and the transitions between them. Scientific revolutions, in spite of Kuhn's claims to the contrary, have often been understood as challenges to the rationality of science, or to its progress, and for that reason have received much philosophical scrutiny. To a certain extent, this focus on revolutions is the product of Kuhn's own emphasis on undermining what he perceived to be the received, cumulative image of scientific development. However, by focusing on the revolutionary, "non-cumulative episodes" in scientific development Kuhn drew attention away from what have turned out to be some of his more enduring insights into the nature of science. Indeed, while the continuing importance of the notion of incommensurability is, perhaps, philosophically controversial, few contemporary philosophers of science would contest the central role of both models and social structure to our contemporary understanding of both the nature of science and its development.[1] These ideas not only play crucial roles in *Structure*, but they continued to be important to Kuhn (and to the philosophical community more generally) long after he backed away from some of his more radical claims about revolutionary change.

It is in the characterization of 'normal science' and the paradigms around which scientific communities form that Kuhn first makes explicit the important role of both models (and/or exemplars) and the social structure of science. Not only do these ideas collectively constitute much of what is distinctive about Kuhn's conception of science in its cumulative mode, but they also play crucial roles in Kuhn's ongoing attempts to characterize revolutionary transitions

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[1] See, for example, Solomon (2001) on the social character of scientific rationality, and Giere (2004) on models in scientific representation.

between paradigms. In this paper, I will highlight the importance of these elements of Kuhn's thought to his conception of science, and then use these elements to sketch a Kuhnian classification of scientific controversies. Furthermore, I will show that, though Kuhn didn't emphasize them, several important sorts of non-revolutionary scientific disagreements were both identified and analyzed in *Structure*. Ultimately, I contend that Kuhn's conception of science supports an approach to scientific controversies that has the potential to both reveal the importantly different sources of scientific disagreements and provide useful resources for understanding their endurance and eventual termination. I will support this contention with several brief examples that suggest the power of a Kuhnian analysis. If, therefore, Baltas is right that Kuhn's focus on scientific revolutions has stunted philosophical work on scientific controversy, this is not because Kuhn's conception of science lacks the resources for an intelligent analysis of the kinds of scientific disagreement. Rather, it seems that philosophers have been too narrowly focused on Kuhn's alleged challenges to scientific rationality and progress, thereby loosing sight of what have turned out to be some of his most enduring insights.

2 Science is Essentially Social

In the second chapter of *Structure*, Kuhn makes the rather remarkable claim that though the investigators of physical optics before Newton "were scientists, the net result of their activity was something less than science" (Kuhn 1996, p. 13). That is, though these investigators crafted theories about optical phenomena, used those theories to design new experiments, and refined their theories in the face of those experimental results—they were *scientists*—their field was not yet a *science*, in the sense that it did not display "the pattern of development" characteristic of modern natural science. The missing ingredient, of course, was a particular social structure. In order to be a *science*, Kuhn claimed, the field had to achieve, "a paradigm that proved able to guide the whole group's research" (Kuhn 1996, p. 22).

A paradigm, in the sense relevant to the characterization of normal science, is "what the members of a scientific community share" (Kuhn 1977, p. 294) that facilitates the more esoteric and puzzle-solving orientation of a group of scientists able to take the foundations of their field for granted. By the Postscript, Kuhn refers to this "constellation of group commitments" (Kuhn 1996, pp. 181–182) as a disciplinary matrix and enumerates its elements. These include symbolic generalizations, instrumental commitments, quasi-metaphysical commitments, values, and exemplars or models. It is this last object of group commitment that was the original source of the term 'paradigm', meaning something like "concrete exemplar." At least three chapters of *Structure* are devoted to explaining what scientific investigation in the context of a shared disciplinary matrix is like, and why fields that have this particular social structure develop in a roughly cumulative way.

The feature of the development of normal science most relevant to this paper is that the disciplinary matrix is "an object for further articulation and specification under new or more stringent conditions" and not simply an "object for replication" (Kuhn 1996, p. 23). When initially achieved, the research consensus unifying the nascent field is typically imprecise and grounded in a few choice model applications. The "most important" work of normal scientists is to "resolve some of its [the paradigm's] residual ambiguities" and to permit "the solution of problems to which it had previously only drawn attention" (Kuhn 1996, p. 27). This is to say that the group consensus unifying a scientific field does not initially include interpretations of how to apply the paradigm in specific cases or how to solve novel problems. Scientists working within the same paradigm can be expected to disagree about such issues. Indeed, Kuhn characterizes the range of responses by idiosyncratic individual scientists as "the community's way of distributing risk and assuring the long-term success of its enterprise" (Kuhn 1996, p. 186). Eventually, most ambiguities are resolved and most problems "respond at last to normal practice" (Kuhn 1996, p. 81) resulting in an increasingly substantial and intricate disciplinary matrix.

The ever-expanding paradigm that forms the basis of a normal science tradition has the additional crucial feature, from the point of view of Kuhn's overall theory, that it reliably leads to its own demise. In spite of the fact that normal science does not "aim at novelties of fact or theory" (Kuhn 1996, p. 52), it regularly produces them. Anomalies, whether individually in the form of unexpected discoveries or collectively as signs of the break down in the problem solving capacity of a paradigm, eventually push scientific fields into crisis. This "extraordinary" phase of scientific development also depends crucially on the social structure of science. In the first place, individual scientists vary in their assessments of whether or not their fields are in crisis; this results in some of them being willing to try out "seemingly nonsensical" possibilities, while the majority stick to the procrustean task of fitting the offending anomalies into the current paradigm. Even among those committed to treating alleged "counterinstances" as puzzles for the current paradigm, Kuhn anticipates "a proliferation of divergent articulations" (Kuhn 1996, p. 27) as the anomaly continues to offer resistance. Most of the time, one of these divergent articulations will eventually provide a consensus solution to the puzzle. As a result it is "essential to science" that not all practitioners treat each anomaly as a reason to pursue revolutionary alternatives and that practitioners don't generally embrace "each new theory advanced by a colleague" (Kuhn 1996, p. 186). The range of attitudes of individual scientists ensure that, for the most part, normal science is able to keep up its cumulative march through the sea of anomalies facing any paradigm, while at the same time leaving room for its eventual overthrow.

When the possibility of a scientific revolution does arise, the social structure of science again plays a crucial role in deciding the future commitments of the scientific field. As Kuhn was keen to emphasize, debates between incommensurable alternative paradigms are settled by the scientific equivalent of "mass persuasion" (Kuhn 1996, p. 93). Mass persuasion must be resorted to because there is no "neutral algorithm" (Kuhn 1996, p. 200) that can be used to decisively measure which of the alternative possibilities is the best way forward. Instead this normative assessment is community property; the result of "the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists (Kuhn 1996, p. 200)." Some scientists are draw by their sense of the "appropriate or the aesthetic" (Kuhn 1996, p. 155) to begin to develop alternative ways forward in the face of a deepening crisis. If all goes well, they can gradually begin to solve some problems and encourage conversions to the new way forward. Many will resist, but, if the new paradigm is destined to triumph, gradually there will be "an increasing shift in the distribution of professional allegiances" until, at last, "only a few elderly holdouts remain" (Kuhn 1996, pp. 158–159).

The distinctive features of those fields that are now universally recognized to be sciences, according to Kuhn, include their social structures. The practitioners in these fields share a 'disciplinary matrix', which plays a crucial role in enabling these scientists to take for granted the foundations of their field and to concentrate on increasing the scope and precision of the paradigm. At the same time that there is a consensus on the disciplinary matrix, there is also, Kuhn alleged, a broad range of different opinions about both what is important to the field and how it might be pushed forward. This diversity plays a crucial role in the dynamics of scientific fields, both during their normal and extraordinary phases. The evolving and essential tension between the consensus and the differences in scientific communities is crucial to Kuhn's understanding of science, and that is part of why science is, according to him, essentially social. It might seem puzzling, however, how a community of scientists, which agrees about all of the elements of the disciplinary matrix, could support the diversity that Kuhn invokes. To understand how this can be so, we must turn to Kuhn's account of the cognitive content of science.

3 Locating the Content of Science

Kuhn's appeal to the firm set of commitments that unify a scientific field and his simultaneous invocation of the practitioners' diverse responses to anomalies or potential alternatives is supported by his account of the content of science. Contrary to what many philosophers at the time thought, and indeed what many may still think today, Kuhn held that linguistic formulations of scientific theories and scientific values were insufficient to capture either the evaluative or cognitive content of science. Though all scientists within a field might agree on some set of theoretical statements (such as Newton's Laws or the Schrodinger Equation) or on the importance of simple, unified theories; this is not sufficient to determine how they will apply those laws or values in concrete novel cases. It is not simply shared statements or abstractly formulated values that form the basis for the scientific consensus evident in periods of normal science; rather it is a shared stock of concrete exemplars that are the foundation of this consensus.

When training in a particular field, according to Kuhn, students absorb the 'tacit knowledge' required of full-fledged members of the scientific community by working through an increasingly complex set of model applications. Initially these exemplars will be standard pedagogical examples, but eventually, as the future scientist's training becomes more specialized, it will include some of the "technical problem solutions found in the periodical literature" (Kuhn 1996, p. 187). These exemplars show the scientist "how their job is to be done" (Kuhn 1996, p. 187) and provide a stock of solved puzzles on which they can model their own contributions to normal science. Presumably, all of the features of the disciplinary matrix are picked up, at least in part, through this sort of exposure to concrete examples. Abstractly formulated laws can, of course, be presented in a lecture or a textbook, but their real content, Kuhn repeatedly claims (in most detail in Kuhn 1977, pp. 293–319) is learned by engagement with concrete model applications. Similarly, whatever abstract values members of a scientific community are prepared to endorse, such as problem-solving ability or simplicity, are also provided with normative content by the concrete examples held to display them. What counts as a neatly solved problem, a fruitful explanation, or a beautiful theory is not obvious on its face and cannot be defined in some collectively acceptable, abstract manner. Instead, these assessments have content for a particular scientist as a result of engagements throughout scientific training with concrete examples held to demonstrate these virtues. This shared stock of exemplars, and the cognitive and normative content that it underwrites, is, Kuhn contends, sufficient to explain the consensus judgments of scientists within a particular field of normal science.

Though they share a stock of exemplars, the members of a scientific community need not, and in fact rarely do, share an "interpretation or rationalization" (Kuhn 1996, p. 44) of their paradigms. That is, they have not abstracted, nor could they be expected to, a set of rules for interpreting or applying their approach in particular cases. Attempts to formulate the consensus belief of a scientific community in a set of linguistic claims are typically rewarded with "continual and deep frustration" (Kuhn 1996, p. 44) because some members of the community will reject any imaginable phrasing. As a result, Kuhn contends, the coherence of a research tradition rests not on implicit rules for how to go on, but instead on family resemblances with, and analogies to, the shared corpus of established achievements. So long as the scientific community agrees about their set of model applications, there is no need to attempt to formulate abstract principles characterizing their consensus practice. Instead, it is principally in times of crisis or revolution that scientists engage in debates about how to interpret or rationalize their community practice.

One virtue of this account of scientific coherence is that it leaves room for substantial disagreements within a particular scientific tradition and not just between distinct traditions. The members of a linguistic community, to extend a Kuhnian analogy, can communicate successfully, for the most part, most of the time, using kind terms like "game" in spite of the fact that there are no explicit rules guiding this community practice. Nonetheless, disagreements about particularly ambiguous or fringe cases can and do arise. A speaker who disagreed about central cases would not be a member of the linguistic community; however, it is not only possible, but to be expected, that community members would occasionally disagree about whether or not some particular practice was a game. Similarly, being a member of a research tradition entails a general and broad based agreement with other members about the appropriateness of particular practices, or problem solutions, within the field. Still, there are particular circumstances in which this consensus will break down giving rise to disagreements, and potentially controversies, within scientific research traditions. These sorts of expected intra-community disagreements supply the diversity that Kuhn requires for his social explanation of scientific development in the context of normal science.

Furthermore, the move from normal science into extraordinary science can be understood as the loss of faith, by some members of the community, in the shared stock of exemplars underwriting the (diminishing) group consensus (Kuhn 1996, pp. 47–49). In such circumstances, some of the problem solutions or practices once held as models for how to go on are in doubt and this manifests itself in debates over "legitimate methods, problems, and standards of solution" (Kuhn 1996, p. 48). Depending on the availability of incommensurable alternatives, these might be either revolutionary debates or debates about how to bend or modify the current paradigm in order to alleviate the crisis. The difference between these debates would come down to whether the scientists engaging in them were, metaphorically speaking, members of different "linguistic communities" or members of the same community advocating different modifications of a dysfunctional linguistic practice. Kuhn's continued emphasis on incommensurability makes it seem as if there should be a bright line between these possibilities, but it is not necessary for the purposes of this paper to decide whether he is right about that.[2] Instead it is enough to emphasize that Kuhn not only has room for non-revolutionary debates about paradigm modification, but also requires them for his social account of scientific development.

4 Non-revolutionary Scientific Controversies

Kuhn made repeated and well-known attempts to characterize the sorts of debates that occur between proponents of competing paradigms. At stake in such debates is the future course of a scientific field; furthermore, these debates cannot be straightforwardly resolved, at least according to Kuhn, by any sort of appeal to commonly accepted standards of evidence or methodology.[3] As a result, such debates can involve scientific controversies, where a 'scientific controversy' is (Freudenthal 2000), "a persistent antagonistic discussion over a disagreement concerning a substantial scientific issue that is not resolvable by standard means of the discipline involved." Since paradigm shifts, and the revolutionary controversies that surround them, have garnered the bulk of philosophical attention since the publication of *Structure*, I will not focus on them here. Instead, I want to start with the observation that there are many scientific controversies (episodes in the history of science that fit Freudenthal's definition) that cannot be comfortably regarded as 'revolutionary controversies' in Kuhn's sense. In a typical case, several scientists who are unequivocally members of the same normal science tradition (and thus share a paradigm) have a persistent disagreement over a scientific issue that resists, at least for a significant amount of time, resolution by standard means. The prevalence of such non-revolutionary controversies is what gives credence to Baltas' charge that by shifting the focus to revolutions, Kuhn's work had stifled the study of

[2] Kuhn describes the evolution of his position on these matters at (Kuhn 2000, pp. 56–57).

[3] Such competing paradigms are not incomparable, but there is no unit of measure, "in terms of which both can be measured directly and exactly" (Kuhn 2000, p. 189).

scientific controversies. Of course, if what I have argued in the last two sections is correct, not only did Kuhn attribute an essential role to non-revolutionary disagreements between scientists, he also provided an account of the content of science designed to explain the sources of such disagreements.

Though Kuhn did not explicitly offer a principle for classifying such non-revolutionary disagreements, he did both abstractly characterize and provide examples of some of these disagreements in *Structure*. As might be expected because of Kuhn' emphasis on the essentially social character of science, the forms of these disagreements vary with the overall (social) state of the fields in which they occur. More explicitly, while disagreements in the context of two incommensurable alternatives take one form (revolutionary debates involving a lot of circular reasoning, etc.), the disagreements that arise in both the context of normal science and extraordinary science are likewise distinct. Furthermore, Kuhn's account of the content of science, both in terms of the disciplinary matrix and the concrete exemplars that give it content, provide useful tools for characterizing these differences in form.

The first kind of non-revolutionary disagreement that Kuhn discusses at some length in *Structure* is between scientists advocating alternative articulations of their shared paradigm in order to accommodate some newly recognized phenomenon. Presumably, such scientists might acknowledge both the same set of concrete exemplars and the existence of the novel phenomenon, but then disagree about how those exemplars bear upon it. It is possible to imagine a variety of different forms such a disagreement might take. The scientists might disagree about whether any of their recognized exemplars are similar to the new case, with one party concluding that the novel phenomenon falls within the scope of the paradigm, and others disagreeing. Or they might disagree about which of the recognized exemplars should be used as a model approach to the new phenomenon. Or even if they agree about the appropriate model, its application might be ambiguous, leading to different extrapolations into the novel case. The actual case that Kuhn discusses seems to fall most naturally into the second form, where different scientists recognize the novel phenomenon to be 'similar' to different exemplars from their shared stock of recognized results.

Kuhn cites his paper, "The Caloric Theory of Adiabatic Expansion" (1958) to support his claim that often "a paradigm developed for one set of phenomena is ambiguous in its application to other closely related ones" (1996, p. 29). The Caloric Theory, he continues, was built on model applications to the cases of heat transfer accompanying mixture and changes of phase. However, there are lots of other ways that a substance might gain or loose heat, including adiabatic expansion (where work done by the gas, without exchanging heat with its environment, results in a lowering of its temperature). Though we now see such cases as obvious evidence that it is energy, not heat or caloric, which is conserved, the scientists working within the Caloric paradigm found ways to assimilate this phenomenon to their accepted models. One option, actually advocated by Dalton, was to attribute a heat capacity to the void, and then to see expansion as a case of mixing with the void (which would take up some of the caloric resulting in a decrease in temperature). Others (including Laplace and Poisson) reasoned that just as heat capacity might differ according to phase, so too might it vary with pressure. On decreasing the pressure of a gas, therefore, one increases the amount of bound caloric resulting in less free caloric experienced as an increase in temperature. The various and competing articulations of the Caloric Theory designed to accommodate adiabatic expansion and related phenomena where worked out and contested, both experimentally and theoretically, over the course of 50 years. All of this work, Kuhn claims, "arose from the caloric theory as paradigm; and all exploited it in the design of experiments and in the interpretation of results" (1996, p. 29). The disagreements of these caloricists were intra-paradigmatic. But neither theory nor experiment offered an unambiguous way forward; these scientists were moving into "a region where neither was entirely secure." As a result, "the selection and evaluation of empirical tests was as much a matter of taste and judgment as the selection and evaluation of theory" (Kuhn 1958, p. 140). This suggests that such disagreements, at least initially, would not have been resolvable by the standard methods available to caloricists at the time; that is, these disagreements would support scientific controversy.

The second sort of intra-paradigmatic disagreements that Kuhn discusses occur during extraordinary science. Extraordinary science develops when a sizeable fraction of the members of a scientific field begin to recognize persistent and significant breakdowns in the problem solving ability of their paradigm. Often, a particular class of anomalies becomes the focus of extraordinary research. Though there are a variety of reasons that these crisis-inducing anomalies become significant, once they are recognized the field becomes more and more focused on their resolution. Initial attempts to solve the problem will be closely modeled on the established results of the paradigm. But, if none of the standard paths of articulation are able to provide solutions, the practitioners are forced to stray farther and farther from their guiding models. Eventually, "even formerly standard solutions of solved problems are called in question" (Kuhn 1996, p. 83); the basis of the cognitive content of the paradigm is in dispute. Kuhn refers to this situation as the "blurring of the paradigm" (Kuhn 1996, p. 83). Again, it is easy to imagine a variety of more specific forms that disagreements about how to bend the paradigm might take. As a first pass, these disagreements might be sorted according to which elements of the formerly accepted disciplinary matrix are in dispute. Some scientists may abandon or reinterpret one symbolic generalization, while others contest the acceptability of particular standard solutions. Since, according to Kuhn's account, there need be no explicit interpretation or rationalization of the concrete exemplars that guide the paradigm, it is not likely that there would be any consensus about how to modify or reinterpret these exemplars in the face of the recognition that something has gone wrong. Instead, each scientist would deny or modify principles (or exemplars) that they regard as less central in order to preserve what, in their assessment, forms the core content of the paradigm.

Kuhn develops three extended examples of scientific crisis in *Structure*. All three of these examples are cases where the crisis eventually results in a revolution, but Kuhn is clear that crises and revolutions are not necessarily concomitant. Some crises will end with the significant anomaly eventually being accommodated within the current paradigm; others will end with the anomaly eventually being written off as too difficult. Only when there is an incommensurable alternative that manages to convince the scientific community that it offers a better future for the field will there be a revolution, and even in such cases the transition may take a long time. This means that according to Kuhn, disagreements about how to bend paradigms should occur in many contexts where they cannot, or should not, be assimilated to revolutionary debates.

The example that Kuhn develops most in *Structure* is the response of Phlogiston theorists to the experimental results that eventually threw their paradigm into crisis. New gases discovered after 1750 and the fact that metals gain weight upon roasting collectively, according to Kuhn, pushed the Phlogiston theory into crisis. Since the phlogiston theory held that, roughly, there was only one kind of gas–air–that could be saturated with various other things (such as water or phlogiston), the discovery of many distinct gases put pressure on the theory. To find room for all the newly recognized gases, various principles and spirits had to be dissolved in the air to different degrees. This led to "almost as many versions of the phlogiston theory as there were pneumatic chemists" (Kuhn 1996, p. 71). Similarly, because the phlogiston theory held that metals were more complex than their calxes, the fact that many metals were observed to gain weight on being converted to their calx posed problems. These observations "did not result in a rejection of the phlogiston theory" (Kuhn 1996, p. 71). Instead distinct modifications of the theory were taken up by different practitioners attempting to bend the theory to fit these new results. Some worked out the thought that, "phlogiston had negative weight" and others the possibility that "something else entered the roasted body as phlogiston left it" (Kuhn 1996, p. 71). In this context, it was "harder and harder to know what the phlogiston theory was" and the field returned to something like a pre-paradigm state, where members of distinct schools compete with each other for the future of the field. These sorts of disagreements among members of the same, though degenerating, paradigm would not be amenable to straightforward resolution and so they might also support scientific controversies.

The sorts of non-revolutionary disagreements mentioned by Kuhn are plausibly not amenable to straightforward resolution because the issue in dispute is not decided by the recognized common content of the shared paradigm. In the case of *controversies of articulation*, like applying the caloric theory to adiabatic expansion, the dispute centers on the ambiguous assimilation of a new phenomenon to core results. On the other hand, in the case of *controversies of reinterpretation*, like modifying the phlogiston theory to fit the proliferation of gases, the dispute centers on what elements of the core must be surrendered to keep the paradigm afloat. In neither case can the (current) core of established model applications decide the issue; but that does not mean that the controversy cannot be resolved, or that it can only be resolved by some analog of mass persuasion. Because disputants in these cases are members of the same (perhaps bending) paradigm, there is at least the potential for appealing to some "common measure" in order to resolve (or dissolve) the controversy.

Teasing out how Kuhn thought that closure or resolution might occur in controversies of these sorts is complicated by the fact that both of these controversies were eventually rendered irrelevant by subsequent revolutions. In the case of applying the caloric theory to adiabatic phenomena, Kuhn suggests that eventually caloricists were able to reject Dalton's proposed articulation in favor of the approach advocated by Poisson, even though–from the point of view of subsequent paradigms–there are good grounds for rejecting Poisson's approach as well (Kuhn 1958, pp. 138–140). Experiments designed by Guy-Lussac in order to test the Daltonian hypothesis evidently convinced most caloricists that Dalton's articulation was inappropriate. The eventual triumph of Lavoisier's system makes it unclear what a resolution to the dispute about reinterpretations of the phlogiston theory would look like. Certainly, only some of the proposed reinterpretations of the phlogiston theory survived to compete, and eventually lose out to Lavoisier's new chemistry. Some of these longer surviving approaches probably fit better with other accepted science. For instance, reinterpreting calcination as an exchange reaction (rather than a simple addition of phlogiston) is easier to reconcile with Newtonian mechanics than holding that phlogiston has negative weight. Others demonstrated themselves capable of accounting for more novel phenomena, such as Priestley's modified phlogiston theory, which found room for completely dephlogisticated air (oxygen). Because of his revolutionary focus, Kuhn does not have much to say about how either of these sorts of controversies are closed or resolved, when they are closed or resolved at all.

It is easy to imagine and articulate, however, ways that such controversies could evolve using the accounts of both the social structure and content of science supplied by Kuhn. Instead of ending in a definitive experiment, for example, *controversies of articulation* might be resolved or closed in a variety of other ways. For instance, the paradigm might reach a pragmatic compromise by allowing that one articulation works for certain problem situations while the other works better in a different subset of cases,[4] or aspects of the proposed articulations might be combined into one coherent compromise approach. As Kuhn suggests in other contexts, the controversy might be simply unresolvable given the state of the science and dropped or suspended. *Controversies of reinterpretation* might be thought of, again following Kuhn's suggestion, as resolved or closed by mechanisms analogous to the process of adopting the original paradigm in a particular field. For example, one reinterpretation might have a startling explanatory or experimental success, thereby distinguishing itself from its competitors. Distinct subfields, each reinterpreting the original paradigm differently, might be spawned leading to the permanent fragmentation of the original consensus in the face of the crisis. There are many other possibilities besides. Baltas is surely right that philosophical accounts of non-revolutionary controversies have suffered because of all the attention focused on revolutions, but equally clearly Kuhn provides (as yet poorly exploited) resources for thinking about such disputes and their eventual termination. In what follows, I will try to sketch how a Kuhnian inspired analysis might be used to shed light on a couple of non-revolutionary controversies and then contrast this perspective with other available general accounts of scientific controversy.

5 Example: The Controversy over Sexual Selection

Charles Darwin and Alfred Russel Wallace are now generally credited as the co-discoverers of evolution by natural selection. Not only were they close friends, but they were also allies in trying to bring the biological community around to their new approach to evolutionary change. In spite of their advocacy of the same basic approach, "they disagreed on a number of important issues … the main disagreement was about sexual selection" (Guyon 2010, p. 140). The dispute between Darwin and Wallace about sexual selection was carried on discretely while Darwin was alive, but eventually after his death Wallace accused Darwin of having "weakened the spirit of Darwinianism" through his advocacy of sexual selection by female choice (Guyon 2010, p. 141). Given that Darwin and Wallace worked together to establish a new approach to evolutionary biology, it seems that their controversy over sexual selection cannot be understood as a revolutionary controversy; it therefore provides a useful test case for a Kuhnian inspired approach to non-revolutionary scientific controversy.

At issue between Darwin and Wallace were cases like the male bird-of-paradise, which has elaborate plumage that is not in any obvious way advantageous in the "battle for life" (Guyon 2010, p. 140). Darwin saw such cases as analogous to breeders who select, "'fancy' varieties in domestic species" (Veuille 2010, p. 150). He hypothesized that the females of the species exercised a choice, based on some aesthetic sense, for the more beautifully endowed males. The chosen males would reproduce more successfully, even though they were not better equipped for survival, thereby promoting the development of the trait in the population. Though it is not entirely clear what Darwin thought about the relationship between sexual selection by female choice and natural selection, he is clear that these are distinct mechanisms. Wallace seems to have thought that attributing aesthetic senses to female birds was unacceptably anthropomorphic–Darwin's analogy was inappropriate. Instead, Wallace tried to assimilate cases like the bird-of-paradise to more standard explanations invoking only natural selection (Guyon 2010, pp. 140–141). Wallace's attempts were not all that convincing, and so "the two scientists never came to an agreement on sexual selection" (Guyon 2010, p. 141). Indeed the issue of sexual selection was largely avoided for many years after Darwin's death and still "the mechanisms governing the evolution of sexual choice in animals remain largely unresolved" (Veuille 2010, p. 145).

This enduring dispute between Darwin and Wallace seems like a natural example of what I previously referred to as a *controversy of articulation*. The dispute arises out of the difficulties encountered by these scientists in assimilating a class of phenomena—dramatic sexual dimorphism—that they both suppose their approach should explain. They pursue different strategies in relating these phenomena to the core of consensus results acknowledged by both. Whereas Darwin, perhaps inspired by a youth spent as a pigeon fancier, sees a novel mechanism and a new analogy to artificial selection, Wallace sees more elaborate consequences of a species adapting to its

[4] Perhaps a case like the competing models of the laser described in (Cartwright 1983) could be thought of in these terms.

environment. It is important that these differences arise only against a strong background of agreement. For the most part, even when explaining other forms of sexual selection (such as selection by male combat), these scientists are in agreement; it is only in cases that are understandably difficult to assimilate (such as prima facia non-adaptive traits) that their approaches come apart.

Following Kuhn's analysis of scientific content, we might expect to find differences between these scientists in the interpretation, or rationalization, of the common, core results of evolution by natural selection. These differences show up at the fringes, or in the difficult cases, where the stock of accepted results does not speak clearly about how to go on. Interestingly, Jean Guyon (2010) does just this in his paper on this controversy, concluding that the controversy reveals the differences in Darwin and Wallace's conceptions of natural selection. Whereas Wallace had an "environmentalist conception of natural selection", where evolutionary change was the result of changing environmental conditions, Darwin had a conception of natural selection that rested on competitive success and not necessarily on adaptive advantage. Furthermore, Guyon suggests, these two different conceptions of natural selection are both still alive in contemporary biology, and sexual selection is still a test case for them (Guyon 2010, pp. 142–143). From Kuhn's point of view, the continued diversity of Darwinism is not only to be expected, but is also an essential ingredient in evolutionary biology's continued development as a science.

6 Example: Aristotelian Cosmology after the Telescope

Kuhn is clear that the Copernican Revolution is a case where a scientific field goes through a prolonged crisis before eventually undergoing revolution (Kuhn 1996, p. 86), and so it provides a useful window on extraordinary science. In particular, the prolonged crisis in Aristotelian cosmology should provide concrete examples of *controversies of reinterpretation*, where scientists disagree about how to modify the commitments of their paradigm in order to accommodate crisis-provoking results. Though Kuhn does discuss the Copernican Revolution in *Structure*, and he invokes it in support of his claims about extraordinary science, he does not devote much effort to describing the extraordinary science that took place within it. Fortunately, Kuhn's first book, *The Copernican Revolution* (Kuhn 1957), provides some of this detail. Chapter 6, "The Assimilation of Copernican Astronomy," is devoted to describing the gradual process by which heliocentrism deriving from Copernicus eventually took over the astronomical and cosmological communities. Kuhn is careful to acknowledge both that there was much resistance to this assimilation and that this resistance could not be simply dismissed as irrational. Still, eventually, the force of the accumulating arguments for heliocentrism made the transformation "inevitable."

One set of arguments that Kuhn discusses in some detail (Kuhn 1957, pp. 220–228) is based on the observations made by Galileo using the telescope beginning in 1609. Galileo saw a lot of things using his telescope, including craters on the moon, comets, sunspots, moons around Jupiter, and the phases of Venus. All of these newly observed phenomena could be used as the basis of arguments for heliocentrism, but as Kuhn remarks, "[t]hough the telescope argued much, it proved nothing." The most rational of Galileo's opponents "agreed that the phenomena were in the sky, but denied that they proved Galileo's contentions" (Kuhn 1957, p. 226). Though Kuhn doesn't describe these opponents in any detail, they would presumably be adherents to Aristotelian cosmology who thought that they could accommodate Galileo's observations by modifying their current system, rather than rejecting it in favor of heliocentrism. Furthermore, based on Kuhn's descriptions of extraordinary science in *Structure*, one expects that some fragment of the astronomical (or cosmological) community would adopt this strategy. Members of this conservative faction would differ in the features of Aristotelianism that they were willing to surrender to accommodate these new observations; different scientists would interpret what had gone wrong with their paradigm differently and make adjustments that reflect their assessments of the core commitments of Aristotelian cosmology.

Interestingly, recent work on the Copernican Revolution has brought out some details about this conservative faction. Roger Ariew describes the work of Jacques du Chevreul who (Ariew 2009, p. 296):

> "managed to accept the observations made by Galileo … with the assistance of the telescope, but did not regard these phenomena as evidence for either the Copernican or the Tychonic system. He accepted Galileo's observations from more or less within the framework of Aristotelian cosmology."

Accommodating these new observations required du Chevreul to make "significant modifications" (Ariew 2009 p. 297) to his Aristotelianism, however. Instead of occupying their own heaven, according to du Chevreul, Venus and Mercury rotate around the Sun, "any other arrangement would require the interpenetration of orbs, causing a vacuum–and this is impossible in nature" (Ariew 2009, p. 295). The status of Venus and Mercury in the Aristotelian system underwent, as a result of du Chevreul's modifications, a dramatic change; but de Chevreul was able to maintain other core tenets of his position including, for instance, the impossibility of the vacuum. Not surprisingly, given Kuhn's account, Ariew also describes other members of this same faction who interpret what has gone wrong with Aristotelian cosmology differently. He summarizes: "[a]ll of them could be said to use Aristotelian principles they deemed more fundamental to deny Aristotelian tenets that they regarded as secondary" (Ariew 2009, p. 297).

Whereas Ariew sees the response of the conservative faction to Galileo's telescopic observations as counterexamples to Kuhn's account of scientific change, it is not necessary to interpret it that way. It is true that these scientists, "made changes that went well beyond what could be described as the articulation of the Aristotelian paradigm or exemplar" (Ariew 2009, p. 297). It seems plausible, at least based on this article, to regard these changes as reinterpretations of the paradigm in the face of persistent and significant anomalies. If they are so regarded, then these disagreements appear to be a classic case of the 'bending' of a paradigm and the subsequent return to something like the "pre-paradigm" state. That is, enduring disagreements between these Aristotelians would be *controversies of reinterpretation*. Following the development and eventual termination of these debates might, then, supply some of the missing details about how this class of Kuhnian controversy evolves and eventually ends.

7 Contrast with Contemporary Accounts

Because it is often assumed that the only sorts of controversies Kuhn had room for were *revolutionary controversies*, contemporary philosophical approaches to scientific controversy often develop themselves in opposition to Kuhn. Interestingly, however, when working out their approaches to scientific controversy, such philosophers often end up invoke ideas very similar to those crucial to Kuhn's own broader understanding of scientific disagreements.

Aristides Baltas, for example, develops a classification of scientific controversies based on the idea that scientific controversies arise when "disagreeing scientists do not share background 'assumptions'" (Baltas 2000 p. 44). He ends up distinguishing three major types of scientific controversy. Very roughly, the kinds of controversy that Baltas identifies line up with the broad types of controversy identified in this paper. There are some differences between these accounts, however. The most obvious difference is in their understanding of scientific content: whereas Baltas divides the content of science into a "set of overtly formulated premises" and an "amorphous plethora" of background assumptions (Baltas 2000, p. 41), Kuhn thinks about the content of science in terms of exemplars and analogy. A more significant difference arises from Kuhn's emphasis on the social epistemology of science. For Kuhn, the potential for scientific controversy arises in a natural way from his understanding of the essentially social development of science (and his theory of content). As we have seen, for Kuhn, a tension between consensus and disagreement plays an important role in how science develops in all of its major phases. These distinct phases give rise to different sorts of disagreements, which in turn manifest as different kinds of controversy. Thus the Kuhnian inspired classification of scientific controversies described in this paper is integrated with a larger story about scientific development, and the role of social dynamics in that development, in a way that Baltas' is not. Of course having an account of scientific controversy integrated with a larger story about scientific development can be either good or bad, depending on the quality of that larger story. This is not the place to evaluate the merits of Kuhn's account of science and its development, but it is worth pointing out that Kuhn has room for a more realistic account of scientific consensus and disagreement than he is often given credit for.

Other philosophers have taken up the social dimension evident in Kuhn's thought, emphasizing the important role that scientific disagreements play in scientific progress. Philip Kitcher (2000), for instance, understands scientific controversies to be debates about proposals to modify consensus practice in order to bring it into line with some individual's practice. It can be a good thing, from the point of view of scientific development, that "cognitive variation" is kept alive by the "complex forces of human motivation" (Kitcher 2000, p. 27) until it becomes clear which, or whether, such proposals will lead to a more unified and consistent consensus practice. Controversies arise and are sustained by the fact that different scientists weigh the competing demands of consistency and unification differently; they are resolved when all but one of the "escape trees are blocked" (Kitcher 2000, p. 31) either by inconsistencies or explanatory loses. Much of this seems roughly compatible with Kuhn's claims about the important roles of diversity in scientific development. Differences show up, yet again, in the language used to talk about scientific content; Kitcher prefers more and less unified explanatory schemata and the cases they can be extended to cover to Kuhn's values, exemplars, and analogies. Apart from the fact that Kuhn emphasizes the importance, and ambiguity, of a whole range of epistemic virtues important to the evaluation of scientific approaches, I am not sure that Kitcher's understanding of scientific controversy is importantly different from what I identified as a Kuhnian controversy of articulation. If this is right, Kitcher's account could supply some of the detail missing from the sketch presented in this paper of Kuhnian controversies of articulation (for instance, by using escape trees to talk about why Dalton's account of adiabatic expansion lost out). Furthermore, the other classes of controversy identified by Kuhn (controversies of interpretation and revolutionary controversies) might indicate types of scientific disagreement that are not comfortably accommodated by Kitcher's approach[5]. Roughly speaking, for Kitcher, Kuhnian normal science—with its rich social dimension—is all there is, all the time.

8 Conclusion

I hope to have demonstrated the importance of two relatively underappreciated aspects of Kuhn's thought–the social element in the epistemology of science and the role of models or exemplars in underwriting scientific content. These ideas were essential to the account of science and its development presented in *Structure*, and they have, in a testimony to Kuhn's prescience, only become more important to philosophers of science over the last 50 years. Additionally, these ideas are combined in *Structure* to provide a much more sophisticated account of scientific disagreement than Kuhn is often given credit for. Although he is not overt about it, Kuhn identifies several sorts of scientific disagreement beyond the revolutionary debates that he discusses so extensively. These disagreements differ according to the overall social state of the scientific fields in which they arise. Furthermore, Kuhn's account of scientific content provides useful resources for exploring the nature of these disputes, the reasons for their endurance, and the potential avenues for their termination. The two examples considered show that at least some non-revolutionary scientific controversies can be usefully thought about in these Kuhnian terms. Lastly, alternative contemporary accounts of scientific controversy have developed these Kuhnian themes, trying to classify scientific controversies through an analysis of scientific content and, in some cases, emphasizing the social dimension of scientific development. A Kuhnian account of scientific controversy promises to interact fruitfully with these contemporary accounts, and perhaps, because of its connection to a grander narrative about the development of science, to offer a useful alternative point of view on scientific disagreement and its role in scientific progress.

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[5] In endnote 11 (Kitcher 2000, p. 36), Kitcher explains that the rival individual practices that supply the basis for scientific controversies need to be understood as "rival developments of a shared consensus practice."