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KUHN'S INCOMMENSURABILITY THESIS: GOOD EXAMPLES STILL TO BE FOUND**

Abstract

In *The Structure of Scientific Revolutions*, Thomas Kuhn famously argued that scientific revolutions consist in paradigm shifts in which the superseded and the new paradigms are incommensurable. My aim in this paper is to show that neither Kuhn's examples nor Yafeng Shan's recently proposed example adequately support this incommensurability thesis. Starting from the distinction between global and local incommensurability, I argue that, on the one hand, local incommensurability does not imply that paradigms are globally incommensurable, and, on the other, that it is likely that real support for Kuhn's thesis that "the proponents of competing paradigms practice their trades in different worlds" requires global incommensurabilities. Thus, I argue that the Kuhnian view is not capable of providing satisfactory evidence that those incommensurabilities ever occurred in the history of science.

Keywords: Kuhn, incommensurability, paradigm, exemplar

1. THE KUHNIAN VIEW OF SCIENTIFIC CHANGE

Thomas Kuhn (1970) famously argued that scientific revolutions consist in paradigm shifts in which the superseded and the new paradigms are in-

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commensurable, which means that they are methodologically, conceptually, and observationally disparate (cf. Oberheim 2018). This amounts to the claim that scientific revolutions cannot be explained within the rationalist model of scientific progress, according to which it is always possible to find out, in one way or another, why new paradigms are better than the superseded ones. Although Kuhn changed his accounts of incommensurability over the course of time (Kuhn 1983, Hoyningen-Huene 1990, Sankey 1993, 1998, Chen 1997),[1] his most controversial (and the most popular) thesis concerning incommensurability was probably the claim that scientists who work within different paradigms live in different worlds. Kuhn puts it as follows (1970: 150):

> In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds. . . . Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction.

Kuhn later replaced the perceptual account of incommensurability with a linguistic account (Chen 1997: 258), but the main idea concerning scientific practices in different paradigms remained the same. Namely, in his later writings, Kuhn understood incommensurability in a more specific way: first, in terms of untranslatability that stems from semantic shifts of the key notions shared by the two competing theories ("local incommensurability," cf. Kuhn 1983, Chen 1997, see also the next section), and later in terms of taxonomic changes to the effect that the proponents of different theories classify the same entities differently (Kuhn 1993, 2000a, Sankey 1998). Although the later Kuhn ceased to use the Gestalt switch analogy (which is indicated by the passage quoted above and elsewhere), his latest conception of taxonomic incommensurability might also be considered compatible with the idea that world changes occur due to scientific revolutions (Bird 2004b: 49).

However, I would like to stress at the very beginning that my focus in this paper will be primarily on the views of the earlier Kuhn, since I believe, like some contemporary interpreters (Bird 2002, 2004a, Shan 2018, Rowbottom 2011: section 2, van Fraassen 2002: lecture 3) that today those views look more plausible with respect to Kuhn's original aim to provide a realistic account of scientific change. As some authors have noticed (for more details, cf. Bird 2002, 2004a), in his later writings Kuhn moved from a naturalistic account of the nature of scientific change to an *a priori* one, thus departing from the mainstream views in philosophy (epistemology and the philosophy of language, in particular), on which he tried to build his philosophy of science.

[1] More recent discussions of the Kuhnian view of scientific change can be found in Kindi, Arabatzis 2012, Richards, Daston 2016, Mizrahi 2018.

Alexander Bird has called that move "Kuhn's wrong turning," arguing that, among other things, Kuhn's view has remained unsupported by good examples (see the next section for more details).[2]

Now, let us consider Kuhn's well-known example that illustrates his claim quoted above — the case of a stone swinging from a string. Here, Aristotelian physicists would say that the string impedes the stone's natural tendency to move downward, while Galileans would say that they see a pendulum (those who endorse the impetus theory would say that impetus was externally implanted to the stone; cf. Kuhn 1970: 118-119; 1990b: 303, Simmons 1994: 124). In all these cases, the interpretation of the same phenomenon depends on a previously adopted theory.

Kuhn also provided several examples in order to support his main theses about the nature of scientific revolutions. His favorite (and the most discussed) example was Einstein's special theory of relativity, which superseded Newtonian mechanics (Kuhn 1970: 101-102). This shift led to radical changes concerning our understanding of some of the most fundamental concepts in physics, such as space, time, and mass. Another example, frequently used in his book, was the replacement of phlogiston theory by Lavoisier's oxygen theory of combustion. Here, according to Kuhn, the paradigm shift occurred primarily due to Lavoisier's novel understanding of oxygen as an element rather than as a compound, which made already known information (that some metals gain weight when burned) relevant to rejecting the phlogiston theory.

Generally, Kuhn thought that pointing to good examples in the history of science is a virtue that any acceptable account of the nature of science and scientific change should have. In that respect, he criticized logical positivists and Popper, whose ideas about the nature of scientific change he found unrealistic (or at least less realistic than his own account). Kuhn (1970: 8-9) also departed from Reichenbach's famous distinction between the context of discovery and the context of justification (Reichenbach 1938) by claiming that they are deeply intertwined and therefore should not be seen as separate.

It might appear at first sight that Kuhn's incommensurability thesis is only of historical interest, since, as Bird has noticed, "there is no specific Kuhnian school in the philosophy of science" despite the fact that Kuhn is considered one of the most influential philosophers of science in the last third of the twentieth century (Bird 2002: 443-444). Yet there have been various attempts at making sense of the Kuhnian view of scientific change (one such attempt will be examined in due course). It is worth noting that some influ-

[2] It is also remarkable that, in his later writings, Kuhn stopped using the term "paradigm" and continued to speak about theories or lexical structures instead, cf. Kuhn 1982, 1990a, 1993, Sankey 1998: 10, Shan 2018: 6.

ential views today, such as van Fraassen's stance voluntarism (van Fraassen 2002), are in many respects akin to Kuhnian earlier views on paradigm shift. Some philosophers even think that van Fraassen's novelty consists in providing a supplement to paradigms — that is, in showing how different scientists can undertake different activities within the same paradigm (see Rowbottom 2011: 112, Prelević 2017), which amounts to the claim that stance voluntarism is pursued within a Kuhnian paradigm. These examples suggest that some forms of Kuhnianism with regard to the nature of scientific change are still present today.

Still, I argue in this paper that the Kuhnian view of scientific change is not supported by good examples. In view of that, my criticism is stronger than the one proposed recently by Moti Mizrahi (2015), who argues that there is no strong inductive support for Kuhn's incommensurability thesis. Mizrahi does not challenge the plausibility of Kuhn's favorite examples in physics, yet he argues that there are no good examples of incommensurability in other disciplines (in the life and social sciences). He refers to the anastomoses episode in the history of physiology in order to show that Kuhn's account does not work there. In the absence of strong inductive support (Mizrahi also argues that Kuhn's thesis cannot be proven deductively), the explanatory and predictive value of Kuhn's main thesis turns out to be negligible (Mizrahi 2015: 12). Providing good examples of incommensurability in other disciplines would presumably make the Kuhnian account more plausible with respect to inductive strength and one recently proposed example, the early development of genetics, will be discussed in section 3. Yet it will be shown that neither Kuhn's favorite examples nor the newly proposed example support the Kuhnian view of scientific change.

2. TYPES OF INCOMMENSURABILITY

As it was indicated in the previous section, Kuhn originally aimed to provide a realistic account of scientific change, which is supposed to be supported by appropriate examples that can be found in the history of science. However, my aim in this paper is to show that Kuhn did not provide good examples for his main thesis of the incommensurability of competing paradigms and that one recent attempt to do the same also fails to succeed. For example, Kuhn did not discuss in any detail Aristotle's theory of spontaneous generation (according to which living organisms could arise from nonliving matter), famously challenged by Francesco Redi's experiment (later improved by Louis

Pasteur; cf. Roll-Hansen 2018).[3] It is also worth mentioning here that Darwin's theory of evolution became dominant to a great extent due to corresponding breakthroughs in other disciplines, such as geology, paleontology, and — later — genetics (and molecular biology; Weinert 2009: Ch. 2).

If so, then it is hard to believe that Kuhn provided a complete reconstruction of the history of science, which would cover all episodes of scientific change.[4] Still, one might say that Kuhn's primary intention was not to offer a complete reconstruction of the history of science but rather to provide a *more realistic* account of major revolutions than the alternative views proposed in his time.[5] However, I am going to argue that he did not succeed in it either.

In order to do that, I draw a common distinction between global and local incommensurability,[6] according to which the latter is relative to already fixed

[3] Although Pasteur's results are sometimes understood as being misinterpreted by the proponents of the rationalist picture of scientific change (cf. Farley, Geison 1974, Geison 1995, Strick 2002), Kuhn himself remained silent on that episode, which is slightly surprising, given that, on the one hand, he had been teaching in James Conant's course in the history of science for many years, while, on the other hand, Pasteur's work on fermentation and spontaneous generation were Conant's favorite examples (Roll-Hansen 2018: 2). However, Roll-Hansen argues that despite the fact that some external factors were present in the debate between Pasteur and Pouchet (and later on between Pasteur and Bastian) over the validity of the theory of spontaneous generation, there are good reasons to believe that the whole debate was primarily focused on the outcomes of experiments rather than on definite refutations of competing theories.

[4] I do not claim that Kuhn really aimed to provide a complete reconstruction of the history of science (although Kuhn himself held that Kuhn 1970 was a history written for philosophical purposes; cf. Kuhn's discussion with Baltas, Gavroglu, and Kindi in Kuhn 2000b, Bird 2002: 445-446; 2004a, see also the next footnote). Rather, I claim that Kuhn's account would not have accomplished this goal, had he had it.

[5] In a sense, the following passages from Kuhn support this interpretation: "we shall deal repeatedly with major turning points in scientific development with the names of Copernicus, Newton, Lavoisier, and Einstein" (Kuhn 1970: 6-7); "a few readers of this book concluded that my concern is primarily or exclusively with major revolutions such as those associated with Copernicus, Newton, Darwin, and Einstein" (Kuhn 1970: 180-181).

[6] That there are at least two senses of incommensurability is not a new idea (cf. Newton-Smith 1981, Hoyningen-Huene 1990, Sankey 1993, Chen 1997). For instance, the distinction between local and global incommensurability can be found in Simmons 1994. However, Simmons does not determine in his paper whether Kuhn provided satisfactory examples for the incommensurability thesis or not. Also, it has been mentioned in the previous section that (the later) Kuhn used the phrase "local incommensurability" in a narrower sense, meaning by that that there is no common language into which two competing theories can be translated without remainder (Kuhn 1982: 670). Likewise, Michael Friedman's (2001) and Nathaniel Goldberg's (2009) interpretations of Kuhnian incommensurability are mainly focused on the translatability problem. Unlike Kuhn's, Friedman's, and Goldberg's usages, local incommensurability is understood in this paper in a broader sense, as will become clear in due course.

criteria (i.e., relative to empirical evidence, meanings of theoretical terms, non-empirical virtues, adopted sorts of explanations, research priorities, etc.), while the former presupposes that competing paradigms are incommensurable in all respects (observationally, conceptually, and methodologically).[7] Such a difference can be illustrated by the cases of underdetermination of scientific theories (cf. Duhem 1906, Carrier 2011), in which competing and conceptually different theories (e.g., Copernicus' heliocentrism and Tychonic geoheliocentrism before Galileo's discovery of the phases of Venus and the moons of Jupiter) are capable of explaining the same empirical data. This amounts to the claim that those theories are incommensurable with respect to empirical observation.[8] However, those theories are not *globally* incommensurable, because they could be (as they were) compared with respect to certain non-empirical virtues, such as simplicity, coherence with the background knowledge, testability, fruitfulness, and the like. Now, scientists might disagree with respect to those non-empirical virtues as well (cf., e.g., Newton-Smith 1981: section 9.8). For instance, conventionalists typically prefer simplicity over coherence with the background knowledge, while inductivists do the opposite. But such disagreements are resolvable (at least in principle) by using other criteria (i.e., by establishing research priorities, further philosophical considerations, etc.).

Incommensurability with respect to the meanings of theoretical terms (such as "mass," "space," "time," "phlogiston," etc.) has been examined at length by William Newton-Smith (1981: Ch. 7), who has argued that the examples to which Kuhn frequently appealed in his later writings just showed that competing paradigms might ascribe different meanings to the same theoretical terms, without proving that such equivocations entail their incommensurability. Newton-Smith also argues that widely accepted referential theory of meaning (the so-called new theory of reference) could easily deal with Kuhnian examples. Namely, Kuhn (like neo-positivists) endorsed a holistic theory of meaning, according to which the meaning of a theoretical term depends on the role it plays in a theory, which implies that changes in the theory amount to a corresponding change in the term's meaning. This licenses a view that incommensurability occurs when there is no common language into which new and superseded theories could be fully translated (Kuhn 1990b: 299). One undesirable consequence of such a view (cf. Newton-Smith 1981: 110, 150, 158) is that it predicts that, for example, Einstein's theory and

[7] In effect, that is the original sense of the notion of incommensurability that the earlier Kuhn endorsed.

[8] This means that empirical observation cannot be used here as a criterion for comparing two competing theories or paradigms.

Newton's theory do not contradict each other, but they rather speak about different things. This amounts to the claim that these two paradigms are not in competition, which departs from how scientific community typically understands the difference among them.

Relatedly, it is worth considering to what extent Kuhn's theory of scientific change is compatible with the mainstream views in the philosophy of language, such as Fregeanism (Frege 1960) and referentialism (Kripke 1972, Putnam 1975), of which the former states that sense determines reference, while the later denies that claim and typically holds that the reference of a natural kind term (or some other rigid designator) is fixed by initial baptism. Although Kuhn's views as such need not be considered incompatible either with Fregeanism or with referentialism (Read, Sharrock 2002, Bird 2004b),[9] it is still hard to find convincing examples that would support his incommensurability thesis. It is highly unlikely that new paradigms, as Kuhnian account predicts, refer to different kinds of entities than the superseded ones — that is, that changes in meaning produce corresponding shifts in reference. Rather, it is (at least *prima facie*) more reasonable to say that competing paradigms either try to explain the same reality or that a new paradigm changes ontological commitments (Bird 2004b: 46-48). For instance, in the case of Newtonian mass and Einsteinian mass, to which Kuhn frequently appealed, it seems more plausible to say either that the former does not exist at all (in that respect, Newtonian mass would be like phlogiston) or that both Newtonian "mass" and Einsteinian "mass" refer to the same quantity, but Newton had some false beliefs about its nature (in that case, Newtonian mass would be like Ptolemy's understanding of "Mars"; cf. Bird 2004b: 46-48). Bird (2004b: 47) notices that both options sharply depart from Kuhnian incommensurability, according to which both Newtonian mass and Einsteinian mass exist in the world due to meaning changes of the term "mass." Ultimately, it does not seem that Kuhn's later views of incommensurability (local incommensurability and taxonomic incommensurability), under the assumption that they are compatible with the mainstream views in the philosophy of language mentioned above, are supported by convincing examples.

Examples of different types of incommensurability listed above corroborate the claim that sometimes top-down and bottom-up changes occur in science. An example of a top-down change is Einstein's theory of relativity,

[9] As is well known, Kuhn criticized Putnam's Twin Earth thought experiment (Kuhn 1990b, 2000a), but today it is a prevailing view that this criticism relies upon several misunderstandings (Bird 2004b: section 6). Bird has also pointed out that Kuhn could have simply rejected scientific realism, on which Putnam's theory relied, but in that case his critique of the new theory of reference would turn out to be question-begging.

which was, according to Einstein himself (in his famous lecture on geometry and experience that was held in Berlin in 1921; cf. Einstein 2008: 19), made possible by breakthroughs in mathematical logic ("axiomatics"), which made room for the axiomatization of non-Euclidean geometry used in spelling out his theory. As examples of bottom-up changes one may consider various attempts to transform logic in view of the rise of indeterministic interpretations of quantum mechanics (e.g., in quantum logic; cf. Birkhoff, von Neumann 1936).[10] These examples of bottom-up and top-down changes do not corroborate the claim that global incommensurability between competing paradigms ever occurred in the history of science.

Now, let us turn again to Kuhn's favorite example — the special theory of relativity. As it was stressed above, Kuhn thought that Einstein's revolution led to radical changes concerning our understanding of some of the most fundamental concepts in physics, such as space and time. However, it is well known that Einstein was aware of the fact that he could choose whether to adopt a more complicated geometry (non-Eucledian spacetime geometry) or to adopt a more complicated physical theory, as well as that he, as a physicist, preferred simpler physics over simpler (Euclidian) geometry (Howard 2005: 38). In view of the last fact, simplicity can be understood (as usual) as a rational criterion that led physicists to abandon the Newtonian paradigm. This gives rise to the claim that the incommensurability between Einsteinian and Newtonian paradigms was at best a case of local rather than global incommensurability. If so, then Kuhn's main example does not prove what Kuhn claimed it does.

All in all, it seems that local incommensurability does not corroborate Kuhn's claim (quoted in section 1) that "the proponents of competing paradigms practice their trades in different worlds" (Kuhn 1970: 150). To justify this claim, a stronger conception of incommensurability is needed.

3. GOOD EXAMPLES STILL TO BE FOUND

Now, let us turn to a recently proposed interpretation of the Kuhnian view of scientific change: the "exemplar-based approach" proposed by Bird (2002) and Yafeng Shan (2018), according to which Kuhn's main novelty consists in emphasizing the role of exemplars in characterizing paradigms (Kuhn 1970: 187, Rowbottom 2011: 112). In view of that, paradigm shifts can be under-

[10] Jan Łukasiewicz (1968) famously argued that the universal validity of the principle of bivalence depends on whether determinism is true or not.

stood primarily as episodes in which new exemplars had been introduced. Here, exemplars are understood as essential constituents of disciplinary matrices, which suggest new puzzles, new approaches to solving these puzzles, and provide criteria of evaluation for the proposed solutions (Bird 2002: 447, Shan 2018: 5, Rowbottom 2011: 115). The exemplar-based approach is aimed to breathe new life into Kuhn's conception of incommensurability.

Shan's interpretation seeks to "show how the exemplar-based approach as a case of reviving Kuhn's legacy is helpful to contemporary philosophy of science" (Shan 2018: 14).[11] He uses the early development of genetics (Mendelian genetics) as a case study in order to support Kuhn's central idea. According to Shan (2018: section 5), Mendel's novelty in the study of inheritance consists primarily in introducing new research problems, conceptualization, and providing corresponding solutions. Namely, Mendel introduced a new exemplary practice by examining the development of pea hybrids in their progeny and applied statistical analysis in the study of heredity in order to explain the dominance and recessiveness of traits, something that made sense within that analysis (Shan 2018: 19). Other botanists, such as Hugo de Vries and Carl Correns, introduced new exemplary practices, respectively, by applying Mendel's insights to the study of pangenesis and trying to find out to what extent his ideas are verifiable.

Shan also recalls, without going into details, questions raised by Darwin for the first time in biology, such as "How will the struggle for existence . . . act in regard to variation?" and the like (Shan 2018: 11, cf. Darwin 1859: 80). Examples of that sort can be found in Kuhn's book as well. For instance, Kuhn pointed out that the adherents of the corpuscular theory of light in the eighteenth century, who worked within the Newtonian paradigm, tried to find evidence of the pressure exerted by light particles impinging on solid bodies, which was not considered an enterprise worth undertaking by the proponents of the early wave theory (Kuhn 1970: 12).

Shan's example mentioned above slightly departs from Kuhn's original characterization of paradigms, according to which "normal science" occupies a relatively long period of time, since it depicts Mendel's immediate successors (for example, de Vries and Correns) as those who introduced new exemplary practices and paradigms thereof (cf. Shan 2018: 21) so that the origin of genetics is characterized as a chain of exemplary practices, rather than pursuing different activities within the same paradigm. Nonetheless, given that this exemplar-based approach is aimed to reconcile the Kuhnian view of sci-

[11] Shan even thinks (in accordance with what is said in the previous section) that only such an interpretation could save the day, since he admits that other interpretations of Kuhnian incommensurability are not supported by good historical examples (Shan 2018: 10).

entific change with everyday scientific practice in one way or another, it should be taken seriously.[12]

However, it does not seem that Shan's understanding of the early development of genetics is in accordance with how prominent geneticists understood that phenomenon. For example, Ronald Fisher wrote in his famous book:

> It is a remarkable fact that had any thinker in the middle of the nineteenth century undertaken, as a piece of abstract and theoretical analysis, the task of constructing particulate theory of inheritance, he would have been led, on the basis of a few very simple assumptions, to produce a system identical with the modern scheme of Mendelian or factorial inheritance. (Fisher 1930: 7)

This suggests that those thinkers were capable of finding a satisfactory account practically from the armchair (Dawkins 2004: 68) without experiencing any paradigm shift, contrary to what Shan's version of the Kuhnian account would predict. Fisher was well aware of the fact that Darwin had not been far away from endorsing the particulate theory of inheritance (relating to this, Fisher quoted Darwin's correspondence with Wallace; cf. Darwin 2008), while recently Richard Dawkins (2004: 68-70) has presented further textual and historical evidence for such a view.

Namely, Darwin felt the need to explain inheritance within his theory of evolution. As is well known, Fleeming Jenkin (1867) in his review of *The Origin of Species* criticized Darwin's theory by claiming that it is incompatible with the blending theory of inheritance (superseded by the particulate theory) — according to which progeny inherits the average values of the parents' characteristics — to which Darwin appealed at that time. Jenkin noticed that, on the one hand, natural selection is a much slower process than blending, while, on the other hand, blending tends to decrease variation that makes natural selection possible. The upshot is that variation would disappear long before natural selection had started to work, which would prevent the very occurrence of natural selection. Darwin became aware of the problems that the blending theory poses to the theory of evolution and consequently searched for a solution that had striking similarities with Mendelian particulate theory. He even performed a Mendelian-style experiment on sweet peas: in his 1866

[12] It is worth recalling Mizrahi's view, mentioned in section 1, according to which there is no strong inductive support for Kuhn's incommensurability thesis. Now, if the arguments presented in this and the previous section are correct — i.e., if they show that Kuhn's main examples in physics do not work — then, by the same token, his incommensurability thesis would remain unsupported by induction: *even if* we take it for granted that Shan's example of Mendelian genetics is convincing, Kuhn's account would remain supported by evidence in one domain only. However, I go one step further and argue that Shan's purported example is unconvincing either.

letter to Wallace (Darwin 2008: 137-138), Darwin says that he had "crossed the Painted Lady and Purple sweet peas, which are very differently colored varieties, and got, even out of the same pod, both varieties perfect but not intermediate." Besides, although the blending theory has some intuitive appeal, a little reflection shows, as Dawkins has pointed out, that it is implausible independently of whether one accepts the theory of evolution or not, given that one of its unpalatable consequences is that male and female human parents would produce intermediate hermaphrodites, which is at odds with the fact that they produce either males or females (Dawkins 2004: 68). It also wrongly predicts that we should be as indistinguishable as clones since it presupposes that variation disappears over time (Dawkins 2004: 67). Bearing this in mind, one might come (by using the method of elimination) to an embryonic version of the particulate theory of inheritance or at least to an understanding of why such a theory should be preferred to the blending theory.

If so, then one need not experience a paradigm shift in order to find out that the particulate theory is superior to the blending theory of inheritance. Rather, it is more appropriate to say that one theory (or, in the case of Darwin, one of its underlying assumptions) was refuted by reflection and argumentation. In view of the last fact, it is unlikely that Shan's purported example supports Kuhn's incommensurability thesis.

Now, one might say that my criticism of Shan's example is a case of cherry-picking since it relies upon a view of a famous geneticist (Fisher), which need not be considered reliable evidence, given that scientists themselves tend to be unreliable narrators of earlier developments of the field they are part of. It would be more convincing, according to this objection, to counter Shan's view with the work of actual historians of biology and genetics. As for Darwin's Mendelian-style experiment on sweet peas mentioned above, one might argue that appealing to it would lead to the precursor fallacy, in which the precursor phenomenon is considered the same as the phenomenon itself.[13]

In response to the above worries, I would like to stress that in this dialectical context the burden of proof is on those who try to show that the Kuhnian account of scientific change is supported by good examples, which means that they have to offer a *clear* example that supports their view.[14] In that respect, my criticism of Shan's alleged example of incommensurability might amount

[13] I would like to thank an anonymous reader for voicing these concerns. The precursor fallacy has been described in Sandler 1989: 118.

[14] Especially since Shan claims that Kuhn's definition of exemplar is not well articulated, because, among other things, "no detailed historical example of an exemplar is illustrated" (Shan 2018: 10).

to the claim that this example is far from being clear rather than to a proof that the example fails. Thus, raising a mere suspicion that Fisher was not a reliable narrator would boil down, in its present form, to an argument from ignorance.

Furthermore, it is noticeable that Shan (2018: 22) builds his Kuhnian account of scientific change on historical reconstructions like the one proposed by Iris and Lauren Sandler, according to which it is best to understand the merits of Mendel's work (and to explain why it was ignored until 1900) by taking into account new research problems introduced by Mendel himself (cf., e.g., Sandler, Sandler 1985, 1986). Namely, the authors have noticed that, on the one hand, "during the whole of the second half of the nineteenth century, biology as a discipline was principally concerned with the problems of evolution that had been raised by Mendel's contemporary, Darwin" (Sandler, Sandler 1986: 754), while, on the other hand, Darwin himself understood inheritance merely as a form of growth, which, according to them, implies that the prevailing view at that time was that genetics and embryology (in today's sense) should not be seen as separate, contrary to how they were to be considered later on. For this reason, Sandler and Sandler hold that it is not surprising that Mendel's result, which was only about the transmission of inherited traits, was not considered a complete theory by his contemporaries (Sandler, Sandler 1986: 755).

Still, it is hard to believe that just for the above reasons Darwin would not accept Mendel's result (and that he would be willing to cling to the blending theory of inheritance), had he been informed about it. His Mendelian-style experiment on sweet peas as well as his concerns about the blending theory suggest the opposite. Even if Darwin held for some reason that a proper theory of inheritance should take the process of growth into account, it does not follow that he would not be ready to incorporate Mendel's result into his own theory of evolution. For it is one thing to say that a theory is incomplete and quite another to consider it incommensurable with, or not better than, an alternative theory. If so, then it is highly unlikely that (in a hypothetical situation) Darwin would need to experience a paradigm shift to be able to recognize the merits of Mendel's work and its importance to his own theory of evolution. Besides, given that Darwin performed his experiment on sweet peas at about the same time when Mendel's work was published, the abovementioned criticism that my appealing to Darwin's experiment on sweet peas leads to the precursor fallacy remains unwarranted.

Relatedly, it is worth mentioning that some philosophers of science think that it is not quite clear when exactly genetics was born. On the one hand, if by "genetics" (the word coined by William Bateson) we mean "the new science of heredity based on Mendel's laws," then we should agree that it was institutionally established at the beginning of the twentieth century (Gayon 2016: 226). In that respect, it does not make sense to claim that any paradigm shift occurred here, because there was no previous paradigm that had been superseded by the Mendelian one. On the other hand, if by genetics we simply mean the study of heredity in general, then we should hold that it can be traced back to ancient times, since it is well known that Aristotle criticized Hippocrates's earlier version of pangenesis (Sturtevant 2001: Ch. 1, Olby 1966). In that case, the main question that arises here is, again, whether Mendel's contemporaries, such as Darwin, would have experienced a Kuhnian paradigm shift had they been informed about his research. It is hard to believe so for the reasons presented above.

In addition, it might be stressed that Shan's solution puts the cart before the horse, since the main task here is to learn what motivates scientists to raise new questions that had not been addressed before, or, even more importantly, what makes raising those questions legitimate, rather than to find out which solutions to those questions are appropriate. Namely, Kuhnian exemplars are typically understood as sets of "contextually well-defined research problems and the corresponding solutions" (Shan 2018: 11), which suggests that problem-defining (Shan understands it as the "practice of defining and redefining the research problems," Shan 2018: 13) is an essential part of an exemplar. Yet Shan's proposal has remained silent on why scientists pose new questions as well as on whether there are any criteria for distinguishing questions that are worth addressing from those that are not. This requires an explanation, since there are many examples in philosophy and science (and in argumentation in general) in which some questions have been proclaimed *wrongheaded* (rightly or wrongly).

For example, the debate between the adherents of Newtonian corpuscular theory of light and the Huygens wave theory was framed by the assumption that light travels through luminiferous aether (for a historical survey, cf., e.g., Darrigol 2012). Some well-known experiments, such as Thomas Young's double-slit experiment and Léon Foucault's rotating mirror experiment, had been purported to prove the latter theory. However, the assumption shared by both theories turned out to be false in light of the further development of physics (the Michelson—Morley interferometric experiment and special relativity, in particular), in which the existence of the luminiferous aether was denied. Likewise, eliminativists in philosophy of mind, such as Daniel Dennett (1991) and Paul and Patricia Churchland (Churchland 1996, Churchland 1986), typically claim that phenomenal consciousness will be explained away in a future physical theory, which means that the questions concerning the problem of the explanatory gap (Levine 1983) between the physical and the mental or the hard problem of consciousness (Chalmers 1996) are, according to them, wrongheaded. Whether a question is wrongheaded or not depends on a hidden assumption involved in it, and it is likely that there are independent grounds that enable us to establish if the very assumption is true or not (for example, if the Michelson–Morley experiment is correct, then the luminiferous aether does not exist, and so on). So the mere fact that sometimes different paradigms address different issues does not license the view that they are globally incommensurable. Thus, the incommensurability with respect to exemplars, illustrated by Mendelian genetics, should not be understood as a case of global incommensurability that supports Kuhnian views of paradigm shifts. The same holds, *mutatis mutandis*, for other examples mentioned in this section.

If these considerations are correct, then the Kuhnian account of scientific change remains unsupported by evidence: the real support to Kuhn's thesis that "the proponents of competing paradigms practice their trades in different worlds" requires *global* incommensurabilities, but, unfortunately, neither Kuhn nor recent interpreters have provided convincing evidence that those incommensurabilities ever occurred in the history of science. Good examples are still to be found.

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