Can Kuhn's Taxonomic Incommensurability Be an Image of Science?

Abstract

I criticize Kuhn's (1962/1970) taxonomic incommensurability thesis as follows. (i) His argument for it is neither deductively sound nor inductively correct. (ii) It clashes with his account of scientific development that employs evolutionary theory. (iii) Even if two successive paradigms are taxonomically incommensurable, they have some overlapping theoretical claims, as selectivists point out. (iv) Since scientific revolutions were rare in the recent past, as historical optimists observe, they will also be rare in the future. Where scientific revolution is rare, taxonomic incommensurability is rare, and taxonomic commensurability is common. For these reasons, taxonomic commensurability rather than incommensurability should be advanced as an image of science.

Keywords

Evolutionary Theory, Historical Optimism, Incommensurability, Selectivism

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1. Overview

Thomas Kuhn (1962/1970, 149-150) famously claims that competing paradigms are taxonomically incommensurable. I call Kuhn's claim 'the taxonomic incommensurability thesis (TI),' following Moti Mizrahi (2015, 362). It appears that taxonomic incommensurability can be advanced as an image of science, when combined with Kuhn's view that scientific development consists of cycles of normal science and revolutionary science. (TI) and the account of scientific development jointly imply that scientific revolutions will occur in the future as they did in the past, and as a result, present paradigms will be displaced by taxonomically incommensurable new ones. Thus, taxonomic incommensurability is a perennial phenomenon in science.

This paper aims to show that taxonomic incommensurability cannot be advanced as an image of science. I proceed as follows. In Section 2, I delineate what a deductively sound argument and an inductively correct argument for (TI) would look like. I also argue that it is impossible to construct a deductively sound argument for (TI), and that it is difficult, although possible, to construct an inductively correct argument for (TI). In Section 3, I argue that Kuhn's argument for (TI) is neither deductively sound nor inductively correct. In Section 4, I argue that (TI) clashes with Kuhn's contention that science evolves in the way that organisms do. In Section 5, I show that the taxonomic incommensurability of successive paradigms does not mean that an old paradigm is thrown out *in toto*. In Section 6, I argue that scientific revolutions will be rare in the future, as they have been in the recent past, and hence that taxonomic incommensurability will rarely arise in the future. In Section 7, I anticipate and reply to some possible objections.

2. Deductively Sound Argument and Inductively Correct Argument

Mizrahi (2015, 363-368) raises and answers the question: Can there be a deductively sound argument for (TI)? I wish to raise and answer a related question: If there is a deductively sound argument for (TI), what would it look like?

Let me use an analogy to answer this question. Suppose there are some balls in an urn. You believe that all of them are red. How would you go about constructing a deductively sound argument for your belief? The answer to this question is simple and straightforward. You pull out all the balls from the urn and then check each ball for its color. If there are ten balls and all of them are red, you can construct a deductively sound argument as follow:

There are ten balls in the urn.
Ball$_1$, ball$_2$, ----, and ball$_{10}$ are red.
∴ All the balls in the urn are red.

In this argument, the conclusion necessarily follows from the premises, and the premises are true. So the argument is deductively sound.

This example illustrates how we can go about constructing a deductively sound argument for (TI). Suppose that there are one hundred pairs of competing paradigms in science, that you check each pair for taxonomic incommensurability, and that all of them are taxonomically incommensurable. You can, then, construct a deductively sound argument for (TI) as follows:

There are one hundred pairs of competing paradigms in science.
Pair$_1$, pair$_2$, ---, and pair$_{100}$ are taxonomically incommensurable.
∴ All pairs of competing paradigms are taxonomically incommensurable.

Can we construct such an argument for (TI)? My answer is no. Constructing such an argument would require us to enumerate all the pairs of rival paradigms in science, and to check each pair for taxonomic incommensurability. The set of all the pairs includes not only past, but also future, paradigms. But how can we be certain about the number and the contents of future rival paradigms? Set this problem aside. There is another. As Marc Lange (2002, 283) points out, it is difficult to individuate and count scientific theories. It follows that it is also difficult to individuate and count paradigms. Kuhn (1962/1970) does not enumerate past paradigms. He does not even say how many paradigms there were in the history of science. He only says that “the examples could be multiplied *ad nauseam*” (1962/1970, 136). It is not surprising why he only says so, given that it is difficult to individuate and recognize paradigms.

Mizrahi (2015, 368-373) raises and answers the question: Can there be an inductively correct argument for (TI)? I wish to raise and answer a related question: If there is an inductively correct argument for (TI), how can it be constructed?

My answer to this question is that some pairs of competing paradigms should be randomly selected from the set of all the pairs of competing paradigms. If not random, the fallacy of biased statistics would occur. In addition, the number of selected pairs should be large enough. If not, the fallacy of hasty generalization would arise. Only if these two conditions were met, could the sample be representative of the general population of the pairs of successive paradigms and the inference from the sample to the population be inductively correct. It would be possible, although difficult, to construct such an argument, once we set aside the problem of selecting future paradigms and the problem of individuating and recognizing paradigms.

In this section, I delineated what a deductively sound argument and an inductively correct argument for (TI) would look like, and whether and how they can be constructed. In the next section, I show that Kuhn's (1962/1970; 2000) argument for (TI) is neither deductively sound nor inductively correct.

3. Kuhn's Argument

Kuhn (1962/1970) offers two examples to argue for (TI). One example concerns Newtonian and Einsteinian mechanics. Under Newtonian and Einsteinian mechanics, space is unaffected and affected by the presence of matter, respectively, so the meaning of 'space' changed (Kuhn, 1962/1970, 149). His other example concern Ptolemaic and Copernican astronomy. Under Ptolemaic and Copernican astronomy, the Earth does not move and moves, respectively, so the meaning of 'the Earth' changed (1962/1970, 149).

Kuhn (2000) offers three examples to argue for (TI). The first example involves the concept of motion in Aristotelian and Newtonian mechanics. In Aristotelian mechanics, 'motion' refers to change in general, whereas in Newtonian mechanics it refers only to a change of positions (2000, 17). The second example involves the historical episode that the concept of a cell changed as a result of the replacement of the contact theory of a battery with the chemical theory of battery (2000, 20-24). The third example involves Max Planck's replacement of the terms 'energy element' and 'resonator' with the new terms 'energy quantum' and 'oscillator' (2000, 24-28).

In total, Kuhn (1962/1970; 2000) uses five examples to argue for (TI). He does not claim that they are exhaustive, so his argument for (TI) is not deductively sound. In addition, he neither claims that the five examples are randomly chosen from the population of successive paradigms, nor that the number of the examples is large enough. So his argument for (TI) is not inductively correct. In a nutshell, he has offered neither a deductively sound argument nor an inductively correct argument for the position that all pairs of competing paradigms are taxonomically incommensurable.

Mizrahi is on the right track, when he says that "it is a mistake to generalize from a few selected examples that competing theories in general are taxonomically incommensurable" (2015, 368). Kuhn's argument for (TI) shows, at best, that five pairs of consecutive paradigms are taxonomically incommensurable. It must be independently argued that other pairs of successive paradigms in science, such as the pair of the caloric and the kinetic paradigm and the pair of the ether and the electromagnetic paradigms, are also taxonomically incommensurable.

It might be, however, that Kuhn does not go into the details of other pairs of competing paradigms in science to save space. So I will grant, for the sake of argument, that all paradigms before the early 20th century were taxonomically incommensurable with their successors. Even so, I argue in the following sections, it is problematic to advance taxonomic incommensurability as an image of science.

4. Evolutionary Theory

Kuhn contends that science does not move toward a goal, just as organisms do not evolve toward a goal, and that the analogy between the evolution of science and that of organisms is “very nearly perfect” (1962/1970, 172). Thus, it is wrong to think that science moves toward truths. First, note that this claim about the evolution of science and that of organisms presupposes that evolutionary theory is true. After all, if evolutionary theory is false, then so would be his claim about the evolution of science (Park 2016a, 3-4).

Recall, however, that according to Kuhn, scientific development consists of cycles of normal and revolutionary science. This account of scientific development and (TI) jointly imply that evolutionary theory will be superseded by a taxonomically incommensurable new theory, and hence that Kuhn’s account of scientific development will also be superseded by a taxonomically incommensurable new account of scientific development. So, for example, what is meant by ‘paradigm’ under Kuhn’s account of scientific development will be different from what is meant by ‘paradigm’ under the new account. Just as the word ‘planet’ picks out different objects under the Ptolemaic and Copernican paradigms, ‘paradigm’ will pick out different segments of science under Kuhn’s account of scientific development and its successor. Or perhaps according to the new concept of paradigm, competing paradigms will be commensurable, and a scientific revolution will be completed when a group of scientists persuades another group of scientists through rational argumentations. Or it might be that future philosophers will use different terms, as Imre Lakatos (1970) does, to give an account of scientific development.

Kuhn might reply that evolutionary theory is an exception to his account of scientific development and (TI), i.e., evolutionary theory will not be replaced with a taxonomically incommensurable new theory. So his account of scientific development and (TI) will also not be replaced by a new account of scientific development. In other words, evolutionary theory and his account of scientific development will remain stable unlike our other best theories, such as the kinetic theory and the special theory of relativity.

It is not clear, however, how plausible this move is. What is so special about evolutionary theory that sets it apart from our other best theories? What is the reason for thinking that evolutionary theory will not be superseded by a taxonomically incommensurable alternative, while our other best theories will be? Without convincing answers to these questions, it is merely *ad hoc* to say that evolutionary theory is an exception to Kuhn's account of scientific development and (TI).

Kuhn might reply that he does not need evolutionary theory *in toto* to defend his account of science (Mizrahi, personal communication). His account of scientific development only requires the observational claim of evolutionary theory that organisms do not evolve toward a goal. Scientific claims can be divided into theoretical and observational claims. Antirealists can avail themselves of observational claims, but not theoretical claims, to develop an account of scientific development. So it is not necessarily incoherent to appeal to evolutionary theory to give an account of scientific development which discredits the theoretical claims of science.

This possible reply from Kuhn, however, faces the following two objections. First, the claim that organisms do not evolve toward a goal is predicated on the claim that organisms have not evolved toward a goal since the beginning of life about four billion years ago. It is controversial whether such a claim is observational or theoretical. Although it can be classified as observational by fiat, it cannot be directly confirmed in the way an observational claim that a cat is on the mat is directly confirmed. We can only infer to it from other observational claims. It is also not clear whether it can be better confirmed than theoretical claims, such as the claim that the motion of molecules is responsible for heat.

Second, appealing to Gestalt psychology, Kuhn (1962/1970, 111-135) advances the famous claims about observation, viz., observation is theory-laden, scientists of competing paradigms live in different worlds, and observational data cannot serve as neutral arbiters between competing paradigms. I set aside the issue of whether Gestalt psychology is supported by theory-laden or theory-neutral data. I instead pursue the issue of whether we can trust or not the observational claim that organisms do not evolve toward a goal. If Kuhn is right that observation is theory-laden, that observational claim is also contaminated by evolutionary theory. Hence, it will not be endorsed by future scientists working under a different paradigm. Philosophers of science who will invoke the alternative paradigm will also disagree with Kuhn about whether science moves toward a goal or not. The philosophers and Kuhn, if alive, will live in different worlds, and observations about science will not be able to serve as a neutral arbiter between them. A moral here is that it is self-defeating for Kuhn to invoke a scientific theory to give an account of science which discredits scientific claims, theoretical and observational.

5. Selectivism

Concerning Newton’s second law of motion, Kuhn claims that the “concepts of force and mass deployed in that law differed from those in use before the law was introduced” (2000, 15). Based on this observation, he claims that when “referential changes of this sort accompany change of law or theory, scientific development cannot be quite cumulative” (2000, 15). In other words, he claims that scientific development cannot be cumulative due to taxonomic incommensurability. So proponents of (TI) might be tempted to think that a taxonomically new paradigm ousts an old paradigm *in toto.*

A closer look into the history of science, however, reveals that it is wrong to think so. As John Worrall (1989), Philip Kitcher (1993, 140-149), Jarrett Leplin (1997), and Stathis Psillos (1999, Chapters 5 and 6) point out, when theories were ousted, not all theoretical claims of an old theory were thrown out; some theoretical claims were carried over to the new theory. Their position is called selective realism or selectivism. The idea is that we should be selective about which theoretical claims are worthy of our belief and which are not.

Selectivism applies to the very examples that Kuhn uses to argue for (TI). Consider the transition from the Ptolemaic theory to the Copernican theory. Some theoretical claims of the Ptolemaic theory were retained in the Copernican theory, such as the claim that the orbit of Mars falls inside that of Jupiter, and the claim that the orbit of Jupiter falls inside that of Saturn. Consider also the transition from Newtonian mechanics to Einsteinian mechanics. As Michael Friedman (2001, 63) observes, some theoretical claims of Newtonian mechanics are enshrined in Einsteinian mechanics. For example, Euclidean geometry and the law of inertia were carried over from classical mechanics to the special theory of relativity.

Selectivists would agree with Kuhn that the Sun is classified as a planet under the Ptolemaic theory, whereas it is classified as a star under the Copernican theory, and that $m=F/a$ in Newtonian mechanics, whereas, $m=E/c^2$ in Einsteinian mechanics. They would point out, however, that the Ptolemaic theory and Newtonian mechanics shared some theoretical assumptions with the Copernican theory and Einsteinian mechanics, respectively.

Thus, the rival paradigms had some overlapping assumptions about unobservables, although the paradigms were taxonomically incommensurable.

Psillos draws two interesting conclusions from the fact that past and present theories have some overlapping assumptions about unobservables. First, he concludes that past theories were not completely false but approximately true (1999, 113). Second, he concludes that past theoretical terms like ‘phlogiston’ and ‘ether’ *approximately* refer to the referents of present theoretical terms like ‘oxygen’ and ‘electromagnetic field’ (1999, 294). On this account, reference admits of degrees; it is not an all-or-nothing affair. His theory of reference strengthens his contention that past theories were approximately true and weakens (TI) which presupposes that reference is an all-or-nothing affair.

How do scientific antirealists criticize selectivism? P. Kyle Stanford (2015, 876) claims that there is only a terminological dispute between realists and antirealists. Realists affirm, while antirealists deny, that the preserved theoretical claims of past theories are rich enough to attribute ‘approximate truths’ to past theories. Stanford’s observation indicates that not all theoretical constituents of past theories were abandoned. Thus, realists and antirealists alike would reject that since rival paradigms are taxonomically incommensurable, they have no overlapping theoretical assumptions, or that a new paradigm will oust all the theoretical assumptions of an old paradigm.

6. Historical Optimism

Scientific realists have developed various theoretical resources to defuse the pessimistic induction that since past theories were abandoned, present theories will also be abandoned. Selectivism is one of them. Another was developed by Ludwig Fahrbach (2011a, 148), Seungbae Park (2011, 79), and Mizrahi (2013, 3220). They distinguish between distant and recent past theories. Distant past theories include the Ptolemaic theory, the humoral theory, the phlogiston theory, the caloric theory, and the ether theory. These theories were all abandoned before the early 20th century. Recent past theories include the germ theory, the oxygen theory, the kinetic theory, and the special theory of relativity. They were accepted in the 20th century and have not yet been rejected. Since they are still accepted in the early 21st century, they can also be regarded as present theories. The set of recent past theories is far larger than that of the distant past theories. Fahrbach, for instance, observes that “at least 95% of all scientific work ever done has been done since 1915, and at least 80% of all scientific work ever done has been done since 1960” (2011a, 148). Fahrbach, Park, and Mizrahi’s observation of the history of science is named as historical optimism (Park 2016b, 3). Historical optimism rebuts the premise of the pessimistic induction that all (or most) past theories were rejected.

Historical optimism also rebuts (TI). Even if all distant past paradigms were taxonomically incommensurable with their successors, viz., with recent past paradigms, it is still problematic to say that most past paradigms were taxonomically incommensurable with their successors. Most past paradigms were recent past paradigms, and recent past paradigms have no successors yet. If we randomly select some paradigms from the general population of past paradigms, most of the selected paradigms would be recent past paradigms. Mizrahi (2013, 3219-3220) has already carried out such a random sampling, selecting forty theories out of one hundred twenty four past theories. It turns out that twenty nine of them were still accepted theories, six were abandoned theories, and five were debated theories. Thus, to argue that most past paradigms were taxonomically incommensurable with their successors on the basis of some examples of distant past paradigms is to commit the fallacy of biased statistics. In sum, historical optimism forestalls any attempt to construct an inductively correct argument for (TI).

Historical optimism goes hand in hand with Kuhn's examples of paradigms. It is a tricky business to individuate and recognize paradigms, as we noted earlier, but Kuhn uses the following examples of paradigms throughout his book (1962/1970):

Kuhn's Examples of Paradigms
Ptolemaic astronomy (ibid, 10), Copernican astronomy (ibid, 10)

phlogiston chemistry (ibid, 2), the oxygen theory of combustion (ibid, 56)

Aristotelian dynamics (ibid, 10), the scholastic impetus paradigm (ibid, 120), Newtonian dynamics (ibid, 10), Einsteinian dynamics (ibid, 110), quantum mechanics (ibid, 49)

the Franklinian paradigm of electricity (ibid, 18)

catastrophism (ibid, 48), uniformitarianism (ibid, 48)

Newton's corpuscular optics (ibid, 12), Young and Fresnel's wave optics (ibid, 12), Maxwell’s electromagnetic theory (ibid, 58), Einstein’s corpuscular optics (ibid, 12), the quantum optics (ibid, 12)

caloric thermodynamics (ibid, 2, 29), statistical mechanics (ibid, 48)

pre-Darwinian evolutionary theories (ibid, 171), Darwin’s theory of evolution (ibid, 20, 151, 171)

affinity theory, Dalton’s atomic theory (ibid, 131)

Kuhn does not use other examples of paradigms in his book (1962/1970). Compare Kuhn’s list of paradigms with Stanford’s (2006, 19-20) list of transitions from past to present theories:

Stanford’s List

from elemental to early corpuscularian chemistry to Stahl’s phlogiston theory to Lavoisier’s oxygen chemistry to Daltonian atomic and contemporary chemistry

from various versions of preformationism to epigenetic theories of embryology

from the caloric theory of heat to later and ultimately contemporary thermodynamic theories

from effluvial theories of electricity and magnetism to theories of the electromagnetic ether and contemporary electromagnetism

from humoral imbalance to miasmatic to contagion and ultimately germ theories of disease

from eighteenth century corpuscular theories of light to nineteenth century wave theories to the contemporary quantum mechanical conception

from Darwin’s pangenesis theory of inheritance to Weismann’s germ-plasm theory to

> Mendelian and then contemporary molecular genetics
>
> from Cuvier's theory of functionally integrated and necessarily static biological species and from Lamarck's autogenesis to Darwin's evolutionary theory

An interesting common feature emerges between Kuhn's and Stanford's examples. Their examples of past theories are of ones that were accepted and rejected before the early 20th century; none of them were accepted after the early 20th century. This common feature accords well with historical optimism that recent past theories have been relatively stable.

Utilizing historical optimism, Park (2016b, 10) constructs a pessimistic induction against pessimists. The pessimists of the early 20th century, such as Poincaré (1905/1952, 160) and Ernst Mach (1911, 17), predicted that scientific revolutions would occur and as a result, that the then present theories, viz., the aforementioned recent past theories, would be overturned. Their prediction has not accorded with the history of science. Since the pessimists of the early 20th century were proved to be wrong about most of their present theories, the pessimists of the early 21st century will also be wrong about most of their present theories. This pessimistic induction over pessimists entails that most present paradigms will not be ousted by taxonomically incommensurable new ones, that most future paradigms will be similar to most present paradigms, and that taxonomic incommensurability will rarely arise in the future, as it has rarely arisen in the recent past.

Wray would accept the pessimistic induction over pessimists, given that he says that "only the fate of our most recently developed theories are relevant to determining what we can expect of today's best theories" (2015, 63). In other words, if we want to know whether present theories will be surpassed by alternatives or not, we should investigate the recent past theories of the 20th century, and not the distant past theories from before the 20th century. Wray is right on this account. Present theories are more similar to recent than to distant past theories. So if we have to choose between distant and recent past theories in order to assess the fate of present theories, we should look into recent past theories rather than distant past theories. Since most recent past theories have been stable, most present theories will also be stable.

7. Objections and Replies

How might Kuhn respond to the aforesaid pessimistic induction over pessimists? He might argue that scientists today are doing normal science. For example, physicists today are fleshing out the general theory of relativity. They dogmatically stick to it, even if they encounter anomalies to it. It is therefore not surprising that there have been no scientific revolutions in the recent past. Revolutionary science, however, follows normal science by the very definition of the term ‘normal science.’ So, Kuhn might argue, scientific revolutions will occur, and present paradigms will be displaced by taxonomically incommensurable new ones.

Proponents of the pessimistic induction over pessimists, however, would object that it begs the question to apply the term ‘normal science’ to what scientists are doing these days. Of course, they are fleshing out present paradigms. But applying the term ‘normal science’ to current scientific activities implies that scientific revolutions will occur, since ‘normal science’ is defined as that which is followed by revolutionary science. So we need a new term that is neutral as to whether present paradigms will be superseded by new paradigms or not. I propose that we use ‘ordinary science’ instead of ‘normal science’ to describe what scientists are doing these days. To say that scientists are doing ordinary science means that they are fleshing out existing paradigms, but that does not mean either that scientific revolutions will occur or that they will not occur.

Kuhn might now argue that recent past paradigms will be ousted by future paradigms, as distant past paradigms were ousted by recent past paradigms. Hence, recent past paradigms will be taxonomically incommensurable with future paradigms, as distant past paradigms were taxonomically incommensurable with recent past paradigms.

Is this induction tenable? Many philosophers have brought up two important differences between distant and recent past theories. First, recent past theories are far more successful than distant past theories, as pointed out by Jarrett Leplin (1997, 141), Gerald Doppelt, (2007, 111; 2014), Juha Saatsi (2009, 358), Michael Devitt (2011, 292), Fahrbach (2011b, 1290), Park (2011, 80), and Mizrahi (2013). Second, scientists developed recent past theories with a view to overcoming problems that had beset distant past theories, as pointed out by Leplin (1997, 144) and Alexander Bird (2007, 108). For example, the general theory of relativity was proposed to explain the perihelion motion of Mercury, which was an anomaly to Newtonian mechanics. For these reasons, it is one thing that scientific revolutions occurred in the distant past, and it is another that they will also occur in the future. An argument that addresses these differences is required to assert that scientific revolutions will occur as they did in the distant past.

Kuhn might raise another objection to the pessimistic induction over pessimists. The fact that recent past theories have lasted for about a hundred years does not indicate that they are true, for it usually takes more than a hundred years for a scientific revolution to occur. As Wray (2015, 64) observes, the Ptolemaic theory lasted for about 1,200 years, before it was superseded by the Copernican theory in the mid-16th century. So in about 1,000 years, all present paradigms will be superseded by new paradigms.

As Wray (2015, 64) also observes, however, four scientific revolutions occurred in less than 120 years over the nature of light. There were four transitions from Newton's particle theory, to Fresnel's wave theory, to Maxwell's electromagnetic theory, to Einstein's particle theory, and to the quantum theory of light. If we take this as a typical scientific revolution, it takes only about thirty years for a scientific revolution to occur. On that reckoning, the best explanation of why recent past theories have been stable for about a hundred years is that they are true.

Moreover, as Fahrbach (2011a) observes, the body of scientific knowledge grows exponentially, and as Devitt (2011, 292) observes, present science uses more advanced technologies. Thus, present scientists have better means to discover anomalies to their existing paradigms than past scientists had. So enduring the tribunal of experience for a hundred years in the 20th century has a higher epistemic value than withstanding the tribunal of experience for a hundred years, say, in the 12th century. It does not prove very much that a theory lasted for 1,200 years before the 16th century, because scientific knowledge was slim, it was growing slowly, and past scientists did not have advanced technologies to test their theories.

8. Conclusion

I criticized (TI) as follows: Kuhn's argument for (TI) is neither deductively sound nor inductively correct. (TI) clashes with his account of scientific development which invokes evolutionary theory. Even if two successive paradigms were taxonomically incommensurable, they have some overlapping theoretical claims, as selectivists point out. Since scientific revolutions were rare in the recent past, as historical optimists observe, they will also be rare in the future. Where scientific revolution is rare, taxonomic incommensurability is rare, and taxonomic commensurability is common. For these reasons, taxonomic commensurability rather than incommensurability should be advanced as an image of science.

References

Bird, Alexander. 2009. "What Is Scientific Progress?" *Noûs* 41: 64–89.

Devitt, Michael. 2011. "Are Unconceived Alternatives a Problem for Scientific Realism?" *Journal for General Philosophy of Science* 42: 285–293.

Doppelt, Gerald. 2007. "Reconstructing Scientific Realism to Rebut the Pessimistic Meta-induction." *Philosophy of Science* 74: 96–118.

Doppelt, Gerald. 2014. "Best Theory Scientific Realism." *European Journal for Philosophy of Science* 4: 271–291.

Fahrbach, Ludwig. 2011a. "How the Growth of Science Ends Theory Change." *Synthese* 180: 139–155.

Fahrbach, Ludwig. 2011b. "Theory Change and Degrees of Success." *Philosophy of Science* 78: 1283–1292.

Friedman, Michael. 2001. *Dynamics of Reason*. Chicago: University of Chicago Press.

Kitcher, Philip. 1993. *The Advancement of Science: Science without Legend, Objectivity without Illusions*. New York: Oxford University Press.

Kuhn, Thomas. 1962/1970 *The Structure of Scientific Revolutions*. 2nd ed. The University of Chicago Press.

Kuhn, Thomas. 2000. *The Road since Structure*. edited by James Conant and John Haugeland. Chicago: University of Chicago Press.

Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes." In *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Allen Musgrave, 91–196. New York: Cambridge University Press.

Lange, Marc. 2002. “Baseball, Pessimistic Inductions and the Turnover Fallacy.” *Analysis* 62: 2881–2885.

Leplin, Jarrett. 1997. *A Novel Defense of Scientific Realism.* New York: Oxford University Press.

Mach, Ernst. 1911. *History and Root of the Principle of the Conservation of Energy*. translated by Philip E.B. Jourdain. Chicago: Open Court Publishing Company.

Mizrahi, Moti. 2013. “The Pessimistic Induction: A Bad Argument Gone Too Far.” *Synthese* 190: 3209–3226.

Mizrahi, Moti. 2015. “Kuhn’s Incommensurability Thesis: What’s the Argument?” *Social Epistemology* 29: 361–378.

Park, Seungbae. 2011. “A Confutation of the Pessimistic Induction.” *Journal for General Philosophy of Science* 42: 75–84.

Park, Seungbae. 2016a. “Problems with Using Evolutionary Theory in Philosophy.” *Axiomathes* doi:10.1007/s10516-016-9308-0.

Park, Seungbae. 2016b. “Why Should We Be Pessimistic about Antirealists and Pessimists?” *Foundations of Science* doi:10.1007/s10699-016-9490-y.

Poincaré, Henri. 1905/1952. *Science and Hypothesis*. New York: Dover.

Psillos, Stathis. 1999. *Scientific Realism: How Science Tracks Truth.* New York: Routledge.

Putnam, Hilary. 1975. *Mathematics, Matter and Method (Philosophical Papers, vo. 1)*, Cambridge: Cambridge University Press.

Saatsi, Juha. 2009. “Grasping at Realist Straws.” Review Symposium, *Metascience* 18: 355–362.

Stanford, P. Kyle. 2006. *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*. Oxford: Oxford University Press.

Stanford, P. Kyle. 2015. “Catastrophism, Uniformitarianism, and a Scientific Realism Debate That Makes a Difference.” *Philosophy of Science* 82: 867–878.

Worrall, John. 1989. “Structural Realism: The Best of Both Worlds.” *Dialectica* 43: 99–124.

Wray, K. Brad. 2015. “Pessimistic Inductions: Four Varieties.” *International Studies in the Philosophy of Science* 29: 61–73.