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S.I.: SCIENTIFIC REALISM

Discarded theories: the role of changing interests

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Abstract I take another look at the history of science and offer some fresh insights into why the history of science is filled with discarded theories. I argue that the history of science is just as we should expect it to be, given the following two facts about science: (i) theories are always only partial representations of the world, and (ii) almost inevitably scientists will be led to investigate phenomena that the accepted theory is not fit to account for. Together these facts suggest that most scientific theories are apt to be discarded sometime, superseded by new theories that better serve scientists' *new* research interests. Consequently, it is reasonable to expect that many of the theories we currently accept, despite their many impressive successes, will be discarded sometime in the future. But I also argue that discarded theories are not always aptly characterized as a sign of failure or as a sign of some sort of shortcoming with science. Theories are discarded because scientists are making advances in their pursuit of knowledge. Thus, discarded theories are often a sign of the good health of science. Scientists are responding to their changing research interests.

Looking back at the history of science, one is struck by the fact that many very successful theories were ultimately rejected, to be replaced by alternative theories, theories which make significantly different assumptions about the nature of the world than the assumptions made in the theories they replace. Ptolemy's theory, in which the planets and the sun complete an orbit around the earth each day, was replaced by Copernicus' theory, in which the earth orbits the sun in a year and completes a rotation on its axis once a day. Descartes' theory, in which bodies are moved when they come in

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contact with other bodies and there is no action at a distance, was replaced by Newton's theory, in which there is a gravitational attraction even between bodies separated by great distances. These are just two of the most spectacular examples, but it appears that revolutionary changes of theory are ubiquitous in the history of science (see Laudan 1981; Putnam 1975; but also Hesse 1976; Stanford 2001, p. S9; and Worrall 1989). Even Karl Popper suggests that "in a time like ours…theories come and go like the buses in Piccadilly" (Popper 1952/2002, p. 125).

Theory change presents scientific realists with a serious challenge. The realist wants to maintain that our best scientific theories are approximately true, and that scientists are developing a picture of the world that is becoming increasingly more accurate.[1] But a change of theory often involves changes in the basic assumptions about the world. For example, in Copernicus' theory, the Earth is no longer a unique body at the center of the universe, but a planet, like Mars, Mercury and Jupiter.[2]

Reflection on the many once-successful but since-discarded theories in the history of science seems to suggest that today's best theories will meet a similar fate. If most past successful theories have since been discarded on the grounds that they are false, it seems likely that the theories we accept today, though successful, will also prove to be false in the future. Indeed, it seems like hubris on the part of scientists and philosophers of science to think that contemporary scientists have managed to do what their predecessors were unable to do, develop theories that are not only successful but also approximately true with respect to the claims they make about unobservable entities and processes.

Realists have attempted to address the challenge raised by the so-called "Pessimistic Induction" in a variety of ways.[3] The various realist responses to the Pessimistic Induction suggest three types of responses to the problem of discarded theories. First, some suggest that the problem has been exaggerated, and that as a matter of fact theories are not so frequently discarded. Consequently, there is little basis for constructing a strong inductive argument from the history of science supporting a pessimistic conclusion about contemporary theories. Hardin and Rosenberg (1982) seem to hold this view (see also Mizrahi 2013).[4] Second, some realists grant that many theories have been discarded in the past, but insist that these discarded theories have little bearing on what we can expect of today's best theories. We have entered a new age in science in which our best theories are likely to continue to be accepted, and will merely require refinements and extensions as scientists seek to account for hitherto unobserved phenomena.

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[1] Obviously different types of realists construe the success of science in different ways. Structural realists merely maintain that our knowledge of the structure of reality is increasing (see Worrall 1989).

[2] In his later writings, Thomas Kuhn suggests that changes of theory involve taxonomic or lexical changes. As a result, the various things that were grouped together in the theory that is discarded, may no longer be grouped together in the successor theory (see, especially, Kuhn 1991/2000, pp. 91–94).

[3] There is not just one Pessimistic Induction. Rather there are a variety of different arguments that are called "Pessimistic Inductions" discussed in the literature (see Wray 2015).

[4] There are alternative strategies for reducing the inductive base upon which the Pessimistic Induction rests. Lange (2002), for example, suggests that our concern should not be with the number of theories that have been discarded in the past. Rather, he suggests that some specific fields may have a higher turnover rate than other fields, and we do not want to judge the latter fields on the basis of the failures in the former fields.

This seems to be the view of Fahrbach (2011), Devitt (2011), and Boyd (1985). They believe that because of the advances that scientists have made in methodology over the years scientists are now likely developing theories that are approximately true. Third, some realists grant that many theories have been discarded in the past but note that there is a pattern in the history of science, a pattern that suggests scientists are converging on the truth. With each successive change of theory in a field, scientists are closer to the truth than they were before. Psillos (1999), Kitcher (1993), and Harker (2013) defend this sort of view.[5]

Realists who grant that many theories have been discarded in the past seem to assume that theories are discarded because scientists have managed to develop better theories, *ones closer to the truth*, or *ones that better represent reality*. That is, each change of theory in a field is thought to be a step closer to (i) the truth or (ii) a more accurate representation of reality. Scientists have a fixed target set by nature, and each generation of scientists gets us closer to the target (see, for example, Bird 2000, Chap. 6). A theory is discarded when it is discovered that it missed the target. The realist thus offers a narrative of triumph, despite the history of science with its many discarded theories. This narrative of triumph is what I want to challenge in this paper.[6]

My aim is to take another look at the history of science and to offer some fresh insights into why the history of science is filled with discarded theories. I argue that the history of science is just as we should expect it to be, given the following two facts about science: (i) theories are always only partial representations of the world, and (ii) almost inevitably scientists will be led to investigate phenomena that the accepted theory is not fit to account for. Together these facts suggest that most scientific theories are apt to be discarded sometime, superseded by new theories that better serve scientists' *new* research interests. Consequently, it is reasonable to expect that many of the theories we currently accept, despite their many impressive successes, will be discarded sometime in the future.[7] Though the realist can grant that changes of theory are apt to continue into the future, what she cannot admit is that the changes of theory introduce radical new conceptions of the world. I aim to show that new research interests will sometimes require radical changes of a kind that cannot be reconciled with most types of scientific realism.

But I also argue that discarded theories are not always aptly characterized as a sign of failure or as a sign of some sort of shortcoming with science. Theories are sometimes

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[5] Poincaré (1913/2001) discusses a version of this view in *The Value of Science*. He refers to it as "the scientific conception." According to the scientific conception "every law is only a statement, imperfect and provisional, but it must one day be replaced by another, a superior law, of which it is only a crude image" (339).

[6] Poincaré also suggests that some gains are never lost (see 1905/2001, pp. 122–123). Specifically Poincaré claims that even through an episode of radical theory change "the differential equations are always true, they may always be integrated by the same methods, and the results of this integration still preserve their value" (pp. 122–123). Kuhn also believes that some gains are preserved through radical theory change. "Laws…to the extent that they are purely empirical, enter science as net additions to knowledge and are never thereafter entirely displaced" (Kuhn 1976/1977, p. 19). Theories, Kuhn believes, are a different matter (see 1976/1977, p. 19).

[7] The principal type of success that figures in the discussions of the Pessimistic Induction is predictive success. Realists tend to give predictions of novel phenomena extra weight or consideration. See, for example, Musgrave (1988) and Leplin (1997).

discarded because scientists are making advances in their pursuit of knowledge. In making such advances they are changing their research interests. Thus, discarded theories are often a sign of the good health of science. Scientists are responding to their changing research interests. Importantly, I am not claiming that changes in research interests are the only causes of theory change.

1 Why do theories fail?

I want to consider the extent to which we can explain the history of science without appealing to the realists' assumption that each new theory in a field typically is a step closer to the truth or a more accurate representation of reality than the theory that preceded it. Part of my motivation for this project stems from a concern about the realists' appeal to the notion of relative closeness to the truth, a notion connected to the notion of approximate truth.

Successive theories in a scientific field are almost invariably *described* by realists as closer to the truth than the theories they replaced. But the notion of relative closeness to the truth turns out to be quite challenging to define or operationalize. Popper's (1972/1979) conception of verisimilitude or relative closeness to the truth was shown to be deeply flawed. Roughly, Popper claimed that one theory is closer to the truth than another theory if either (i) it has more truths than the other theory and no more falsehoods, or (ii) it has fewer falsehoods than the other theory, and as many truths as the other (see Popper 1963/2002, p. 316). In the mid-1970s Tichý (1974) and Miller (1974) independently proved that you cannot increase the true content of a partially true theory, that is, a theory that is partly false, by adding true claims without also increasing the false content of the theory. Miller asks us to consider two theories, A and B. According to Miller, "if [Theory] B exceeds [Theory] A in content then either it exceeds it also in falsity content or they are both true" (see Miller 1974, p. 168). By introducing more true claims to a partially false theory one inevitably introduces more false claims as well. Consequently, Popper's criteria will not enable us to determine which theory, A or B, is closer to the truth. Thus, measuring relative closeness to the truth proves to be more intractable than Popper and other realists assume. For this reason alone it is worth entertaining alternative explanations for the many discarded theories in the history of science.[8]

In the remainder of this section, I aim to re-examine the nature of scientific theories, and their relationship to scientists' interests. It is worth emphasizing that I am not concerned here with either (i) social and political interests or (ii) the interests of

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[8] I have benefited from Stathis Psillos' (1999, p. 263) analysis of Tichý's and Miller's papers. Larry Laudan has also objected to the realists' appeal to the notion of approximate truth (see Laudan 1984, pp. 30, 31). Laudan notes that "few…have defined what it means for a statement or theory to be 'approximately true"' (Laudan 1984, p. 30). Incidentally, some realists recognize the difficulties with operationalizing the notion of relative closeness to the truth, but insist that we can rely on a common sense understanding of what "relative closeness to the truth" means in judgments of competing theories (see, for example, Psillos 1999, pp. 276–279; and Chalmers 2013, pp. 260–264). Attempts to revive the notion of increasing verisimilitude in an effort to explain scientific progress continue (see, for example, Niiniluoto 1999, 2014). Darrell Rowbottom, though, argues that "central aspects of scientific progress do not involve science's theories increasing in verisimilitude" (Rowbottom 2015, p. 104). Rowbottom claims that even false beliefs can promote progress.

individual scientists in advancing their careers. I recognize that such interests influence scientists and science. But these are not my concern here. Rather my focus will be on the research interests that determine what sorts of issues a scientist investigates. No doubt these latter interests can be affected by those other sorts of interests. But I want to set aside consideration of the other types of interests here.

Just to be clear on the sort of interests that will concern me here, consider the following examples. At one point in the history of astronomy, astronomers were concerned with the question of whether or not planets were self-illuminating. At one time this question was regarded as an interesting and important scientific question (see Goldstein 1996, pp. 4 and 7). But it is no longer an issue of concern for astronomers. It has been answered. Similarly, even in Kepler's day a serious astronomer might tackle the problem of explaining why there are six planets and only six. This is no longer regarded as a genuine scientific problem though it certainly was seen as one by Kepler. What I want to do is examine the effects that changes in these sorts of interests can have on scientific theories.

Here is my argument in outline: I argue that every theory is only ever a partial representation of the world. Thus, every theory leads scientists to disregard some features of the world. I also argue that scientists' interests determine which features they disregard in their theories. I then argue that as scientists realize their research goals, their interests will change. Consequently, a theory that effectively served the interests of scientists at one time is apt to seem inadequate at some later time, when scientists have different research interests. At this later time, the theory is vulnerable to being discarded and replaced by a new theory that better serves current research interests.

1.1 Theories and interests

I will begin by examining the nature of theories, for this provides the key to understanding why theories have been discarded in the past and are apt to continue to be discarded indefinitely into the future. Much of what I will say here is neither new nor contentious. But I think that many philosophers have not thought through the implications of these claims.

Theories are partial representations of the world. They focus on and account for some features of the world but not others. The partial nature of theories is, in part, a consequence of the fact that theories often embody abstractions. When scientists introduce abstractions into their theories they disregard aspects of the real world (see Chakravartty 2007, p. 221).[9] When scientists work with theories that embody abstractions they are knowingly working with partial representations of the world. My concern here is not with the fact that such theories misrepresent the world (if in fact they do), but with the fact that such theories provide only a partial representation of the world.

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[9] Chakravartty (2007) provides a clear account of the difference between abstractions and idealizations. "An abstract theory is one that results when only some of the potentially many relevant factors present in a target system are taken into account" (Chakravartty 2007, p. 221). On the other hand, "an idealized theory is one that results when one or more factors is simplified… so as to represent a system in a way it could not be" (221). My concern will be with abstractions as they make our theories partial, accounted for some features of the world but not others.

So my argument is not motivated by the concern that theories are false in virtue of being partial representations.

Indeed, the fact that theories are partial representations is not news. Many philosophers have long been aware of this. William James, for example, noted that "as the sciences have developed farther…investigators have become accustomed to the notion that no theory is absolutely a transcript of reality, but that any one of them may from some point of view be useful" (James 1907/1949, pp. 56, 57). Similarly, Ernst Mach argued that "a theory…always puts in the place of a fact something *different*, something more simple, which is qualified to represent it in some *certain* aspect, but for the very reason that it is different does not represent it in other aspects" (1892, p. 201).

Theories are partial representations because scientists are selective about what features of the world they attend to (see Poincaré 1913/2001, pp. 182–185; Mach 1892, p. 201; Cartwright 1983; Giere 1988, pp. 78–80; and Longino 2001, for example). There is just too much information available to scientist. In order to make any sense of experience, scientists, and people in general for that matter, must be selective about what features they attend to (see Hempel 1966, p. 13; Popper 1957/2002, p. 61). But unlike a layperson, a scientist is more deliberate and reflective about what features in the world she attends to. Whereas the layperson may often selectively attend to features uncritically, maybe even as a consequence of the evolutionary history of our species, the scientist consciously decides to take note of some variables, and to disregard others.[10] Clearly, guiding the scientist in her choices are theoretical assumptions about what the world is like. Carl Hempel vividly illustrates the selective nature of data collection in his account of Ignaz Semmelweis' attempt to determine why the women in one ward of the Vienna General Hospital were prone to a higher death rate during childbirth than the women in other wards. Each hypothesis Semmelweis considered led him to collect a different body of data, and to attend to different variables (see Hempel 1966, pp. 3–6). Consider the data Semmelweis collected when he tested the hypothesis that medical students were bringing contaminants from the autopsies they conducted before their clinical work in the affected ward. Clearly Semmelweis was concerned with different data than the data he considered when he was testing the hypothesis that the women were dying of fright from the presence of a priest attending to the dying.

There are obvious benefits to working with theories that are partial representations of the world. By selectively attending to some features of the world and disregarding other features, scientists are able to avoid being overwhelmed by information. This puts them in a better position to detect patterns that might otherwise be difficult to detect. And by employing abstractions scientists can work with theories that are more tractable. For example, calculating the positions of planets is made far simpler by disregarding the effects of the gravitational attraction of neighboring planets, and by "treating the planets as point masses or homogenous spheres" (see Chalmers 2013, p. 223).

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[10] Popper discusses the evolutionary basis of the way animals divide their environments. A hungry animal discerns between food and non-food, an animal being pursued by a predator discerns between hiding places and escapes routes (see Popper 1957/2002, p. 61). Popper's examples are drawn from D. Katz's *Animals and Men*. Clearly, the layperson is more like an animal than a scientist in this respect.

Importantly, I am not claiming that the only reason scientists introduce abstractions is to make a problem more tractable. But it is clearly one important reason.

Indeed, it is because scientists work with theories that are merely partial representations that they are as effective as they are in realizing their research goals. This is a key point in Thomas Kuhn's *Structure of Scientific Revolutions* (1962/2012; see especially Chaps. 3 and 4). Though paradigm-guided research leads scientists to be myopic, this paradigm-induced myopia is generally an epistemic asset, focusing the attention of scientists on only those features of the world that really matter. In fact, Kuhn recognized that this paradigm-induced myopia was both an epistemic asset and an epistemic impediment. It is part of the "essential tension" that characterizes science. When scientists are working in a well functioning research tradition, uncritically working with the accepted theory, paradigm-induced myopia can help scientists realize their research goals. They are determined to make nature fit into the conceptual boxes supplied by the accepted theory. But when persistent anomalies become intractable and a new theory is needed, paradigm-induced myopia can be a serious impediment to scientific progress. It can prevent them from seeing things that are relevant to accounting for the otherwise intractable anomalies.

There is a second feature of theories that relates to their partial nature. Scientific inquiry is interest-driven. Theories are developed with specific research problems and goals in mind. And the research problems that concern a scientist will determine which features she takes account of and which features she disregards. That is, the specific features that a theory accounts for, as well as the specific features that a theory disregards or brackets, are determined by the research interests of scientists.

For example, in early modern Europe astronomers sought to account for the motion of the planets and stars as observed from Earth. In developing their models astronomers sought to account for certain features, like the direction of a planet's motion, including its periodic stations and retrograde motion, the period of a planet's cycle through the fixed stars, and the relative brightness of a planet in the course of its orbit. But they disregarded other features. For example, they made no attempt to account for the apparent color of the planets. And for a long time, astronomers also made no attempt to take account of the mass of planets and stars. These features were deemed irrelevant to their research goals. The mass of planets was regarded as irrelevant to a large extent because astronomers, unlike natural philosophers, were not concerned with causes. Rather, their interests and efforts were primarily directed toward developing accurate planetary models, that is, models that enabled them to predict the location of planets, conjunctions, eclipses, and other such phenomena (see Westman 1975).

Given a different set of research interests, scientists would be led to account for different features than those they accounted for. For example, when Newton sought to develop a physical theory that unified terrestrial and celestial mechanics, the mass of the celestial bodies became a relevant feature in his planetary models.

Before moving on, I want to underscore my main point. My point is that theories are limited in what they represent. Their limitations are what make them valuable. But the partial nature of theories can become an impediment when research interests change. And this provides a key to understanding why the history of science is a history of once-successful but now discarded theories.

We need to resist the temptation to think that scientists are only choosing to work with theories embodying abstractions because they do not yet have the true theory. My argument above is meant to suggest that abstractions play an indispensible and constructive role in science. They make doing the job of science tractable. And they facilitate the aims of science, by providing the means by which scientists are effectively able to make accurate predictions, and manipulate the world in predictable ways. Thus, it is not profitable or insightful to think of a future science where abstractions will play no role in theorizing. No such future is in store for us.

1.2 Changing interests

The fact that a scientific theory is a partial representation of the world need have no negative impact on the course of scientific research. Any specific abstraction that is built into a theory *may* never pose a problem for scientists. After all, the scientists who work with the theory may only apply it to phenomena that are largely unaffected by the features of the world that are not accounted for by the theory.

But sometimes scientists find themselves studying phenomena that will be misrepresented by a theory, phenomena that are not easily accounted for given the conceptual resources supplied by the accepted theory. In these cases, the fact that a theory is only a partial representation may become a concern. But generally in such situations a scientist's first impulse is not to discard a long-accepted theory. Doing so is costly. And generally scientists will want an alternative theory to replace the theory they are discarding. Most research, after all, cannot be conducted effectively without the aid of some theory or other. Whatever else theories might be, they are aids to research. Despite Kuhn's fame for emphasizing the role of theory change in science, even he recognized that a scientist's first impulse is to find a way to solve a research problem using the resources of the long-accepted theory. Kuhn notes that "retooling is an extravagance to be reserved for the occasion that demands it" (Kuhn 1962/2012, p. 76). The decision to discard a long-accepted theory is not to be taken lightly. Other things being equal, there are strong incentives to continue to work with the long accepted theory.

Sometimes scientists will be able to salvage the long accepted theory, accounting for the anomalous phenomena by adjusting various parameters in their models, or changing what features are abstracted in the theory they are working with. A modified version of the accepted theory may thus take the place of the older theory. Continuity may be restored. But sometimes it is not possible for scientists to merely augment the accepted theory to accommodate a new discovery. The long accepted theory will prove to be a significant impediment to advancing scientists' research goals. In such cases, scientists may be led to discard the theory they have been working with.

Even in the normal course of research scientists will sometimes consider discarding a long-accepted theory. It is worth examining how this sort of situation arises. As Kuhn notes the typical scientist's career is spent applying or extending an accepted theory (see Kuhn 1962/2012, Chaps. 3 and 4). Typically scientists work with the conceptual resources supplied by a theory in an attempt to solve hitherto unsolved research problems, problems that are in a sense suggested by the accepted theory. This is the sort of work that science education trains scientists for (see Kuhn 1962/2012, p. 47). The working assumption is that the accepted theory is adequate to the task at hand. The challenging part of research is figuring out how the accepted theory can be applied to the specific phenomena one encounters (see Kuhn 1962/2012, p. 36).

In the pursuit of this goal, in their efforts to fit nature into the conceptual boxes supplied by the accepted theory, two sorts of problems are apt to arise that may lead scientists to consider abandoning a long accepted theory. First, scientists may encounter hitherto undetected phenomena that seem irreconcilable with the long accepted theory. X-rays, Neandertal remains, and novas each raised challenges for a long accepted theory when they were first discovered. Indeed, we are still making striking discoveries about Neandertals and their relationship to our own species as scientists analyze and compare the DNA of Neandertal remains and the DNA of modern humans. For example, we now have evidence that the early ancestors of Asians and Europeans, but not Africans, interbred with Neandertals. This discovery significantly changes our understanding of our species and its relationship to Neandertals and other hominids (see Vernot and Akey 2014; Sankararaman et al. 2012; Green et al. 2010).

Second, in the course of conducting research, after solving a series of research problems, inadvertently a research community may be led to raise new research questions that were unthinkable earlier. In these latter sorts of cases, the research community will have inadvertently developed new research interests. And the research community may find itself confronting research problems that cannot be addressed adequately with the conceptual resources of the accepted theory. In such a situation, the research community may be led to discard the long-accepted theory. For example, it was in the course of extending classical mechanics in an effort to solve a hitherto unsolved problem that Planck inadvertently contributed to the downfall of the accepted theory (see Kuhn 1987/2000, pp. 25–28). What to Planck was intended as an expedient way to model black-body radiation ultimately led other physicists to discover problems with Newtonian mechanics (Kuhn 1987/2000, p. 27). In his work on the black-body problem Planck assumed that radiation was not distributed continuously, merely as a means to make his research problem mathematically tractable. But as he worked on the black-body problem he thought radiation could in fact be distributed continuously (see Kuhn 1987/2000, p. 27). Inadvertently, Planck's research led to the downfall of classical mechanics.

A cautionary remark is in order. I am not claiming that the research interests of scientists change completely from one theory to its successor. Not even Kuhn, despite his reputation, believed this. My point is that provided there is some significant shift in research interests in a research community a theory that seemed adequate before the change may come to seem unacceptable after. As scientists direct their attention to different research problems they change their research interests. Thus, quite extensive continuity in research interests does not pose a threat to the view I am defending. But even against a backdrop of extensive continuity, a long accepted theory may prove inadequate for the problems that now concern scientists.

In summary, what I have been suggesting here is that in the course of pursuing their evolving research interests, scientists may be led to discover the inadequacies of a long-accepted and hitherto empirically successful theory. Sometimes the critical evaluation of the long-accepted theory is related to the specific abstractions that figure in the theory. The features of the world that were disregarded earlier are now regarded as salient and cannot be disregarded any longer, given the research interests of the scientists working in the field. The change in status of the theory, from harmless partial representation to an impediment to research, is sometimes a consequence of the emergence of new research interests. Not all changes of research interests will be smooth. Sometimes changes in research interests will lead scientists to believe that the long-accepted theory is grossly inadequate, indeed, false. And this could be very disconcerting to the scientists who have been working with the theory for some time. Nothing I have said above denies this possibility. But even when scientists do not respond this way, changes in research interests can lead to a change of theory.

Let me briefly address a criticism I anticipate. Earlier, I suggested that the replacement of a long accepted theory by a new theory is not to be regarded as a failure. But above I describe the replaced theory as inadequate. These claims are not inconsistent. What makes the old theory inadequate on the account I am presenting here is that researchers' interests have changed. So the shortcomings of the rejected theory are a function of the fact that scientists have changed the standards by which they are evaluating theories. But such changes of standards are inevitable, as scientists change their research interests. Scientists who experience radical changes of theory, though, are almost inevitably going to regard the discarded theory as a failure. After all, they will be appealing to new standards, standards that have evolved with the new alternative theory that has taken the place of the discarded theory. It should not surprise us that by these standards the old theory appears deficient.

2 Whose interests?

So far in my account of discarded theories I have referred to changing research interests as if it were obvious whose interests I was talking about. It is worth clarifying whose changing interests are responsible for the many discarded theories in the history of science.

Elsewhere I have argued that scientific specialty communities are not agents. I have in mind here groups like endocrinologists, herpetologists, and inorganic chemists. These groups are not capable of having beliefs, intentions or interests (see Wray 2007). More precisely, a scientific specialty does not have interests that are irreducibly the interests of the specialty. Consequently, we cannot expect a scientific specialty, taken as an irreducible whole, to change its interests. Individual scientists, though, do have interests. They choose to conduct research on one topic rather another. And research teams, groups of scientists who pursue research projects collaboratively, are also aptly described as having interests. Research teams may involve two scientists in either the same or different fields, or larger groups of scientists, ranging from three to several hundred in number. Research teams, no matter what their size, must make choices about what research problems they will address.

But an individual scientist or a research team cannot cause a theory to be discarded merely by changing their own research interests, nor even by deciding to no longer work with the long accepted theory.[11] When a theory is discarded in the sense relevant to our concerns here, it is no longer accepted in the research community as a whole.

The problem of explaining how a theory comes to be discarded by a research community was raised by some of Kuhn's critics against his account of scientific change. These critics claimed that given Kuhn's account of scientific change the accepted theory seems to have such a grip on the scientists working with it that it becomes inconceivable how the community could ever break free from the theory (see, for example, Laudan 1984, Chap. 4). Kuhn *seems* to suggest that scientists are incapable of seeing outside the accepted paradigm.

Typically research interests change in a research community when hitherto outstanding problems are solved to the satisfaction of the members of the community. Then researchers turn their attention to other problems. In doing so, the research community changes its interests. The change of interests in a research specialty, though, is not a coordinated affair. Research communities do not operate by consensus conferences. They do not convene meetings to determine what research problems they will address next. Rather, individual scientists and research teams will be compelled to address different research questions, and thus will be moved by different interests. Somehow, though, research communities generally manage to stay relatively focused on a circumscribed set of problems such that the community as a whole retains its cohesiveness and identity, even through episodes of theory change.

There are two factors that tend to ensure that a research community persists through a change of theory, and that a new consensus generally emerges. First, the cohesion of the research community through a change of theory is secured, in part, by the fact that an individual scientist's own interests are affected and constrained by the interests of her peers and colleagues. If an individual scientist addresses problems that do not engage her colleagues, she will find herself marginalized or ignored. As David Hull notes, science is structured such that generally the individual scientist's interests line up with the interests of science, the institution (see Hull 2001, Chap. 5). Hull's aim was to explain why scientists tend to uphold scientific standards, and generally resist fudging data and other deceitful practices detrimental to science. This tendency for scientists to pursue research interests that are of interest to their peers is just one more manifestation of this happy coincidence. A scientist may find herself without an audience for her research if her research interests depart too far from the interests of the rest of the research community. The individual scientists and research teams working in a particular field are thus constrained in choosing what to investigate. Their own research interests must align, to some extent, with the interests of the rest of the research community.

The second factor that explains why a research specialty is unlikely to fragment and why a new consensus is likely to emerge after a theory is discarded is that scientists working in a specialty are not faced with endless choices when consensus breaks down and the field is in crisis. Rather, they may have only two or three alternative theories to

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[11] Perhaps the exception here is those enormous research teams that virtually employ most of the scientists working in a field. When such a research team changes its interests, the field as a whole changes its interests; the field and the team are co-extensive. This, though, is probably rare, and may only happen in certain areas of physics.

choose from when they are looking for a new theory to guide them in their research.[12] Consequently, the fact that a new consensus emerges is no mystery. As the various competing theories are developed and revised in light of criticism from proponents of the competing theories, one theory is likely to emerge as the superior theory (see Wray 2011, Chap. 9). Then it is no mystery at all why most of the scientists working in the field adopt that theory.

3 Truth and interests

In this section, I want to briefly address a criticism that I anticipate. I can imagine a determined realist suggesting that nothing I have argued for above is incompatible with the realists' conception of the aim of science as the discovery of theoretical truth. Even when scientists' interests change they are still concerned with getting at the truth. So the failure of a theory to measure up to the truth is what really explains why it was discarded, the realist critic claims.

Caution is in order here. In the literature on the realism/anti-realism debate there is a lot of ambiguity in discussions of the aims of science (see, for example, the exchange between Rosen (1994) and Van Fraassen 1994; and Rowbottom 2014, for the diagnosis). Bas van Fraassen notes, for example, that the aims of *science* and the aims of *scientists* need not be the same (see Van Fraassen 1994, p. 181). He claims that the aims of science are concerned with the criterion of success in science, whereas the aims of scientists are related to individual scientists' motives (see Van Fraassen 1994, p. 182). Van Fraassen insists that the aims of *scientists* are irrelevant to understanding what divides scientific realists and scientific anti-realists. Rowbottom (2014) argues that the ambiguities surrounding the notion of "the aims of science" have created so much confusion in the realism/anti-realism debate that it is best to abandon any discussion of the aims of science. I think it is wise to heed his advice here.

What I have sought to show above is that we can explain the fact that scientists have been led to discard theories that were long regarded as successful without invoking the notion of truth. On the account of discarded theories I have presented here, scientists are not always discarding theories because they discover that their theories are not true. Rather, sometimes they are led to discard a theory when their research interests shift to such an extent that the long accepted theory proves to be inadequate. The inadequacy of the long accepted theory is a function of the fact that it no longer serves the current research interests of the research community. Hence, the theory is not necessarily being discarded because it is false. In fact, I suggested above that scientists are often deliberately and knowingly working with theories that are only partial representations of the world. A partial representation need not be false. But in virtue of its being

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[12] The Copernican Revolution in astronomy is exceptional in this respect. There were, as many know, three well developed alternative theories competing for the allegiance of European astronomers around 1600: the Copernican theory, the late Renaissance version of the Ptolemaic theory, and Tycho Brahe's theory. There, were, in addition, other competitors, including a version of Brahe's theory that included the Earth rotating on its axis daily, and the so-called "Egyptian theory," which was Earth-centered, but had Mercury and Venus, but not the other planets, orbiting the Sun as the Sun orbits the Earth. More often, scientists are faced with a choice between just two competing theories.

partial, it is bound to not account for some variables which may later prove to be of some consequence.[13]

It seems that realists and anti-realists disagree about the relationship between truth and interests. Given that scientists cannot possibly expect to pursue all truths, some realists seem to think that scientists' interests merely serve to select which truths they pursue. This seems to be Philip Kitcher's view (see Kitcher 1993, p. 94). Realists seem to think that the choice to not pursue other truths is inconsequential from an epistemic point of view. The view I am defending, on the other hand, is that interests play a more pronounced role in science, and the choices scientists make to not account for some variables in their theories can be quite consequential, at least in the long run. Scientists' interests determine which truths they seek, that is, which variables they account for. But scientists' interests also determine which truths and variables can be neglected or disregarded. Scientists create theories that are partial when they employ abstractions. But scientists introduce abstractions because they serve their epistemic interests, focusing their attention on only those qualities that matter, given their current research interests.[14] And clearly their current assumptions about reality will affect which qualities scientists choose to attend to. The account I present seems to fit the actual practice of science better than the realist's account does.

It is worth repeating that I am not claiming that non-scientific interests or broader social interests do not shape scientists' research interests. No doubt they do. But the broader interests can only direct scientists so much. Scientists still need to determine what variables they will study or account for, and these choices will most often be affected by their conjectures about what sorts of factors are causally relevant. In this respect, Kuhn was correct to claim that in mature fields scientists are shielded from the influence of broader social factors. The audience of their research is, first and foremost, other scientists in their specialty, that is, their peers.[15]

4 Unconceived alternatives and interests

The account of discarded theories I have presented here offers some new insight into an issue Stanford (2001, 2006) has drawn attention to, the existence of unconceived

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[13] One might think that I am presenting a false dilemma here by suggesting that a theory is discarded either (i) because, as is typically suggested, it are discovered to be false, or (ii) because, as I suggest, the theory no longer serves the interests of scientists. This is not so. First, scientists might discover that a theory is both false and no longer serves their interests. Second, there might be other reasons as well that lead scientists to discard a theory. I thank one of the referees for *Synthese* for drawing this concern to my attention.

[14] There are affinities between the view I present here, and Carnap's view in "Empiricism, Semantics, and Ontology." Carnap claims that the choice of a language or theory is a pragmatic choice. "The acceptance [of a language or theory] cannot be judged as being true or false because it is not an assertion. It can only be judged as being more or less expedient, fruitful, conducive to the aims for which the language is intended" (Carnap 1950, p. 31). Note the central role that he attributes to the aims of the people adopting the language or theory.

[15] Indeed, some of the variables scientists must work with are determined by funding agencies. For example, the National Institutes of Health (NIH) in the United States might fund a grant program for research on diabetes among African Americans. Clearly, this puts some constraints on the variables that need to be accounted for. But there is much more that needs to be determined, and this is left to the discretion of the scientists.

alternative theories. Stanford's New Pessimistic Induction has proved to be one of the anti-realists' strongest arguments.

Stanford argues that "the history of scientific inquiry offers a straightforward inductive rationale for thinking that there typically *are* alternatives to our best theories equally well-confirmed by the evidence" (Stanford 2001, p. S9). Stanford gives a number of examples, from a variety of scientific fields, including:

> [1] the historical progression from Aristotelian to Cartesian to Newtonian to contemporary mechanical theories…[2] the historical progression from elemental to early corpuscularian chemistry to Stahl's phlogiston theory to Lavoisier's oxygen chemistry to Daltonian atomic and contemporary physical chemistry…[and [3] the historical progression] from Hippocrates's pangenesis to Darwin's blending theory of inheritance…to Weismann's germ-plasm theory and Mendelian and contemporary molecular genetics. (see Stanford 2001, p. S9; numerals added)

Stanford emphasizes that "the evidence available at the time each earlier theory was accepted offered equally strong support to each of the (then-unimagined) later alternatives" (Stanford 2001, p. S9). The more recently developed theories in these series of changes of theory were not adopted *earlier* because they were then unconceived. Stanford argues that reflection on the history of science, specifically, the existence of unconceived alternatives, suggests that even today's best theories are likely to be replaced in the future by as-yet unconceived alternative theories.

My argument, above, suggests that the various then-unconceived theories in the history of science are likely addressing different research problems than the problems addressed by the theories they replaced. Thus, part of the reason 17th Century natural philosophers abandoned Aristotelian physics is because they were developing research interests that were no part of Aristotle's concerns, and thus not fit to be accounted for by Aristotle's theory. The physicists working in the mechanistic tradition associated with Descartes and Galileo, for example, wanted a physical theory that would offer insight into a number of phenomena that were either unknown to Renaissance Aristotelians, or inadequately accounted for by the version of the Aristotelian theory accepted in the Renaissance, including: (i) magnetism, (ii) how the planets stay in their orbits, and (iii) Harvey's discoveries about the physiology of blood flow, to name just a few. Consider the second problem. Until the late 1500s the planets were thought to be embedded in spheres made of ether, so there was no need to explain how they stayed in their orbits. After careful observations of comets made in the 1570s and 1580s, the existence of such spheres was called into question, for the comets appeared to cut through the (alleged) spheres. Henceforth, there was a need to and interest in addressing this scientific problem. Once the spheres were abandoned, explaining how the planets stay in their orbits became a scientific problem.

Similarly, Newton's concerns and interests were not the same as Descartes' concerns and interests. Each theory in the succession of theories in a discipline addresses a different set of problems. Obviously, there is bound to be significant overlap and continuity. But more recently developed theories are developed by scientists concerned with different problems, some of which cannot be adequately addressed with the resources of the theory that is being abandoned.

Thus, it seems that unconceived alternatives are often not conceived earlier, not because of a lack of imagination or a lack of creativity on the part of scientists, but because earlier scientists had different research interests. Obviously this is only part of the story. Research interests are also shaped by developments in instrumentation. For example, with the creation of the air-pump, natural philosophers were able to investigate phenomena that were unimaginable to late-Renaissance Aristotelians. 17th Century natural philosophers were able to examine the effects of the depravation of air on various creatures, lit candles, and barometers (yet another new instrument). What Aristotle and 17th Century Aristotelians had to say about air provided little or no insight into these phenomena. Not surprisingly, scientists turned to a new theoretical framework for insight.

5 Concluding remarks

My aim has been to reexamine the history of science, and to reassess the significance of the pattern of theory change that seems to suggest that theories are apt to continue to be discarded indefinitely into the future. I have argued that the pattern of theory change that the Pessimistic Induction draws attention to is a natural consequence of the development of theories. As scientists develop their theories they are led to ask research questions and to model phenomena that their theories were not designed to answer or model. Rather than marching ever closer to the truth, scientists are constantly, though gradually, altering their research interests and agendas. And changes in research interests can lead scientists to evaluate a theory that they once regarded as successful as inadequate. This is a significant factor in understanding why the history of science is a history of discarded theories.

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